Review 2

This manuscript addresses the topic of Arctic multilayer clouds, and specifically ice related processes, using a nested modeling approach and case studies from the MOSAiC expedition. Broadly the topic of Arctic clouds, and mixed-phase clouds specifically, is an important one because of ongoing modeling challenges in representing Arctic cloud phase partitioning. Moreover, most mixed-phase cloud studies have focused on the arguably simpler single-layer stratiform cloud structure, while relatively little focus has been given to multi-layer cloud systems. One of the major challenges with Arctic clouds, single or multi-layer, is the formation and properties of ice crystals. A great deal of research is now pointing to the potentially important role of secondary ice production (SIP) in shaping the phase composition of Arctic clouds, yet there are few modeling tools to study SIP and its role in cloud structure. Through a series of simulations, this manuscript examines different factors that impact the ice within multilayer clouds. Thus, thematically, the manuscript is timely and focused on a topic that is important for improving our understanding of Arctic clouds and their representation in models. The topic is appropriate for ACP.

The manuscript itself has a number of issues ("major revisions") that will need to be addressed before it is ready for publication. Many of the issues, listed below, are a matter of interpretation or description and should be straightforward to address. There are, however, two more significant issues that will require more work to address.

The first is interpretation of results. There are multiple examples outlined in the General Comments below where the authors speculate on why a given situation occurred. However, the speculation is not clearly labelled as such. Moreover, there is no need to speculate here because the results are from a model and the model should provide all information needed to clearly state why the given situation occurred within the model (which may or may not reflect nature). A good example is around lines 327-330 where there is an explanation given for the appearance of a second cloud layer. In my opinion there are physical inconsistencies in the explanation concerning evaporative cooling and mixing. It is possible that related mechanisms are in operation, but instead of speculating it is better to simply look at the tendency terms from the model to definitively state why the model formed the second cloud layer and then why it later went away. Temperature tendency terms (radiative, latent, mixing, etc.), water tendency terms (evaporation, condensation, etc.), and/or a buoyancy analysis would be very informative in this regard. Please

have a look at the many areas where "interpretation" is provided and then include supporting evidence from the model instead of just speculation.

→This comment is answered within the comment section for each instance.

The second significant concern is related to the model itself. Is it possible to evaluate the model's sensitivity to the specified ice properties and SIP mechanisms when the model representation of the liquid water is so far from reality? In general, there appears to be little sensitivity to the ice processes, and this lack of sensitivity might not be realistic. Based on many past model studies of Arctic clouds, there should typically be sensitivity to the specification of ice processes. For example, numerous papers and model intercomparisons have shown how increasing the ice nucleating particle concentration in the model leads to more ice formation (number and mass) and eventually full glaciation of the cloud. While different models have different thresholds for glaciation based on their own specific set of parameterizations, this basic behavior seems to be consistent across most models. Why is there so little sensitivity in this model? It could be that 1) the simulated cases are just so warm that there is not much that can be done to promote ice formation, or 2) the general model set up (the parameterizations and how they are implemented) is not fit for the purpose of simulating these mixed-phase clouds. There are other, contemporary model studies (not yet published) of very warm mixed-phase clouds that are having difficulty simulating ice formation, so #1 could possibly be true. But it is also important to ensure that this model can represent the basic processes that are known to occur in these Arctic clouds. The model should be run on a colder mixed-phase cloud case, like one of the classics from MPACE or ISDAC or a MOSAiC case from earlier in the year. At these colder temperatures, is the partitioning of phase better? Is there sensitivity to INP concentration? If the model cannot represent this arguably easier situation at colder temperatures, then there is clearly a problem with the model that would inhibit it from successfully assessing the sensitivity to various ice processes like SIP. If the model is able to represent reasonable sensitivities to INPs or SIP at the colder temperatures, then the challenges experienced for these warm cases might simply be due to the fact that the model's specific parameterizations are themselves not suited for the warmest temperatures.

→ This is the reason we are looking into this case in such detail. The model in its original setup is not producing ice at warmer temperatures due to limitations in the immersion freezing parameterisation. The new parameterisation for immersion freezing is our solution but we find it is not enough to produce observed levels of ice mass concentrations. It is sensitive to differences in INP however the observed INP concentration is very low, and we argue that this is not enough to initiate further ice processes in the clouds.

The phase partitioning is better in the model for colder clouds as shown in the first case study of the 1st of September. However, we focus on the warmer clouds due to the large discrepancy between the model and observed levels of ice mass

My final summary point is related to the last comment above. If the first order goal is to examine model sensitivity to ice nucleation and SIP processes, it is probably best to do so in a temperature range where these processes are known to be active and effective. At temperatures near 0 C, all ice processes are greatly diminished and even the WBF is not very effective. Thus, the selected cases are not actually great conditions for understanding INP/SIP sensitivities. It would be much preferable to examine these processes at -6 to -20 C where ice is clearly more significant and where a variety of SIP processes are expected to operate.

General comments

Title: This is a detailed title, but I'm not sure it clearly represents the paper. First it is not clear that any of the simulations arrived at a "realistic structure and composition", so it is hard to say what is required to produce those. Additionally, as noted in some of the comments below, "efficient primary" ice processes do not appear to be in operation in these simulations, in large part because apparently the only primary ice nucleation mechanism is rain freezing, which is inefficient in the model and should

be inefficient in these clouds. Lastly "…and secondary ice processes" is also not reflected in this paper, as really the paper only dealt with one SIP process and did not examine the (presumably tunable) efficiency parameters embedded in that SIP parameterization. Thus, I think it is best to come up with a more representative title.

→ I think there is a misunderstanding about the main work in the paper. We aimed to improve the immersion freezing parameterisation by constraining this to observed INP measurements. This is the efficient primary ice nucleation pathway that we discuss. Furthermore, we have included three SIP mechanisms. Breakup upon ice-ice collisions turned out to be the dominant process while the rime-splintering and droplet shattering are present but have smaller impacts. We clearly show the impact of SIP and the need to include at least breakup upon ice-ice collision for a more accurate representation of the clouds.

We never explicitly state that we optimally represent the observed clouds, but we isolate the requirements to achieve this. The title, to us, reflects the requirements, not necessarily that we achieved that perfectly. Through the two days discussed in detail, we show that the structure can be accurately captured (on the 1st) when the thermodynamics in the model is accurately capturing the observations, while to capture the cloud phase we require both efficient primary ice production through the immersion freezing as well as SIP (3rd).

However, we have explored other titles and suggest:

"Sensitivities of simulated mixed-phase Arctic multilayer clouds to primary and secondary ice processes"

Line 47: There are "at least six" SIP mechanisms. Recent work has suggested more.

→ Added "at least"

Line 56: "shown" is a bit of a stretch here. At best, some studies have inferred that these processes might play a role, but little about SIP has been definitively shown in natural clouds based on observations.

→ Pasquier et al. 2022, referenced with this statement, shows nicely the presence of SIP in their observational study in the Arctic, thus the word "shown" is used.

Line 66: It is better to say "near the north slope of Alaska" as the flights themselves were often over the adjacent ocean.

→ Okay, changed.

Line 66-67: It seems that a definition for cloud needs to be given somewhere. Individual layers within a cloud sounds like it could be two clouds or it could be one, depending on the definition. Since this whole paper is about multi-layer clouds, it is important to give a clear definition for what is meant.

→ Line 59 gives our definition of MLCs.

Line 91: "moored to an ice floe"

→ Ok.

Line 95: LWP is "retrieved" not "recorded"

→ Ok.

Line 96-99: There are additional uncertainties for this type of cloud product specifically related to the cloud type classification, and unfortunately these are unquantified. For example, while a given IWC retrieval might have a quoted uncertainty of 40%, that is when the retrieval is applied to the appropriate cloud. But if it is applied to the wrong type of cloud the uncertainty can be much higher. Cloud type classification is the challenge here and Cloudnet has some challenges in that regard. If nothing more, it is worth mentioning that there are other uncertainties associated with the full way in which the cloud retrievals are applied.

→ We believe the errors associated with the retrieval are substantially covered in the papers covering the algorithms. However, we will acknowledge the difficulty in obtaining reliable classifications by adding this sentence:

"Cloud classification is a challenging topic. During the days investigated here, radar and lidar products were available, making the classification fairly confident and reducing the errors associated with the method."

Figure 1: The caption discusses degrees C while the axis label is in K. It would be best to have a consistent temperature unit used throughout the paper.

→ As the new immersion freezing parameterisation is given in Kelvin we would like to keep the x-axis in Kelvin but we have now added a second x-axis showing Celsius (see Fig. i below)
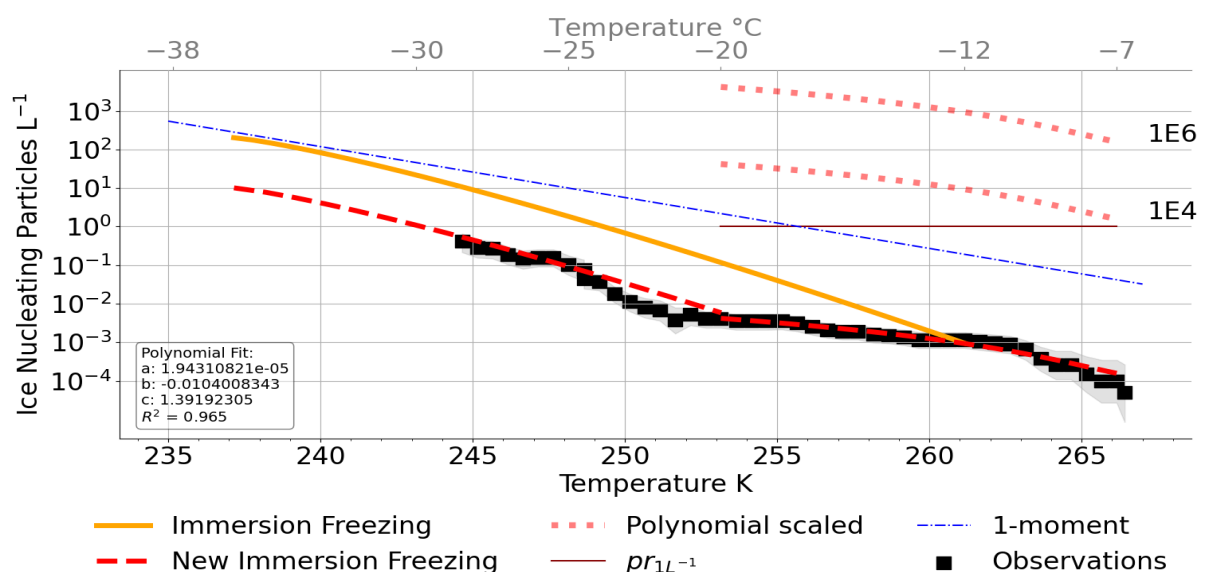
Line 122-125: It would be very useful to know the vertical resolution in the boundary layer and/or at cloud level. I understand that the resolution changes in the vertical, but some information is needed on how well this model set up is able to resolve the appropriate cloud structures.

→ Sorry about forgetting this, we add this to the model setup section:
"This translates into a vertical grid spacing at the lower cloud top (~600 m) of about 55 m for the 6 km and the 1.6 km simulations. The 400 m and 100 m simulations have 39 m, and 32 m vertical grid spacing respectively."

Line 135-136: It is not clear what this statement means. Typically, the "spin up" is to spin up the turbulence while the thermodynamic state is largely advected into the domain based on the model forcing. Certainly, there is also interaction between the turbulence and thermodynamic state. Please clarify.

→ Sorry for the confusion, the statement is phrased a bit awkwardly. By this, we mean that if we let the model spin up for longer (before analysing the output) we lose the accurate thermodynamic state from the initialisation. I.e. the model starts deviating from the observed state. The new sentence reads;
"This relatively short spin-up time was deemed necessary to not substantially deviate from the observed thermodynamic state."

Line 141: We have gotten to the end of the description but so far there is no documentation of the spatial scale of the different model domains. This spatial scale information (similar to the vertical resolution information) is needed to understand how well the cloud systems are resolved within the domain. Without knowing this information, it is difficult for me to comment on the appropriateness of the applied domains and resolutions.

→ We apologise for the lack of information. The extent of the domains is now given in the caption of Figure 3 where the large domain extents are given in degrees and the small ones are given with the radius in km. It reads: "The 6km domain spans 60N-90N and the 1.6 km domain 85N-90N. The 400 m domain has a radius of 112 km and is centred on the ship location. The 100 m domains also follow the ship location with a 33 km radius."

Line 148: As stated here, CCN activation is based on vertical velocity. Yet, one of the conclusions of this paper is that the horizontal resolution doesn't matter much. It is not clear how this can be true unless the CCN is an insensitive parameter in this model set up. At 1.6 km resolution the individual eddies (i.e., updrafts) in these clouds are not well resolved, such that the grid-scale vertical motions are likely much smaller than the actual vertical motions that occur at the (smaller) cloud scales. Thus, the CCN activation is likely less and more homogeneous across the cloud

compared to the spatially inhomogeneous way that CCN are activated in natural Arctic stratiform clouds. It seems that a discussion of this point is quite relevant somewhere in the paper, especially its implication on the apparent insensitivity of the simulated clouds to the CCN perturbations. Additionally, it would be very informative to show vertical velocity results to provide insight into how well the model resolves cloud-scale processes as a function of resolution. There is literature (including some of the papers in the references section) that can provide insight into the expected magnitude of vertical air motions in these clouds.

→ We apologise for the unclear language. The clouds do change with a finer grid spacing and we have now altered the language throughout the manuscript to better represent this. In general, we were expecting larger impacts and thus the language has more reflected this lack of large impacts.

We have now added more figures to the appendix (see Fig. ii below) to better show the impacts of the simulations and discuss the changes in the resolution more in detail. Time-averaged (06-12UTC) cloud droplet number concentrations are shown and show large impacts due to changing the horizontal grid spacing (approximately 4-time (15-time) increase from the pr_1L^{-1} simulation for the 400m (100m) simulation respectively while at the same time, the cloud thickness decreases).

Fig. ii: (a) Cloud droplet number concentrations, (b) cloud water mass content, (c) cloud ice number concentration, (d) cloud ice mass concentration and, first and second saturation adjustments, effectively the cloud droplet condensation routine, called to adjust the excess or deficit of vapour. Panel (d) only shows the first four simulations as listed in the legend. All were calculated for the 3rd of September as a temporal and spatial mean between 06-12 UTC. Added as Fig.A2 in the new manuscript.

However, this is not the variable of interest for us as we are mostly concerned with better representing the cloud ice mass concentration which has a larger discrepancy compared to observations. When considering cloud ice mass concentration, the finer

grid spacing does improve from the pr_1L^{-1} simulation by a factor of 4. However, this is still a small impact compared to the scalings required to reach observed levels of ice and we argue that the computational cost required to run sensitivity studies on this high resolution is not justified.

→ Histograms of vertical velocity are provided here in Fig. iii

Fig. iii: Histograms of vertical velocity from four simulations with different grid spacings. All grid points and heights within the evaluation region are plotted (same region as contours). The bin size is adjusted to the size, N, of the dataset by 2*N^(1/3).

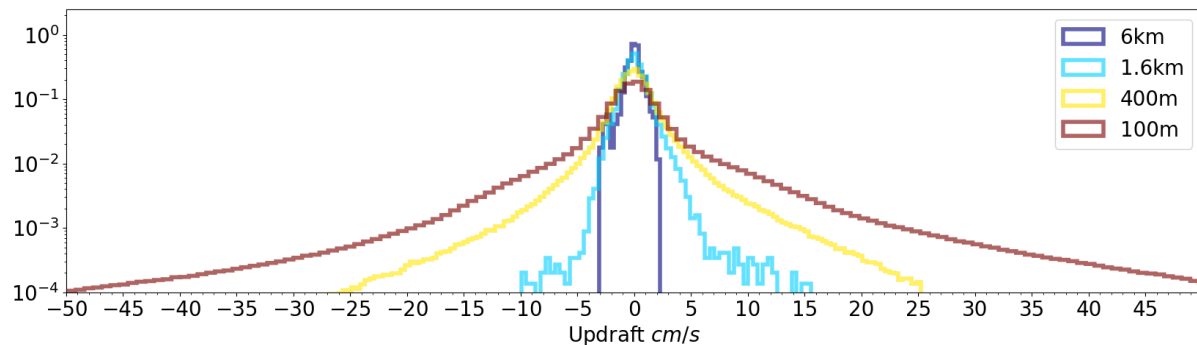→ Capturing how real clouds activate CCN with a model requires very fine resolution to resolve updrafts combined with prognostic aerosols capable of cloud formation. We believe a discussion on how CCN activates in natural clouds versus models feels out of the scope of this paper but we have added a statement to the discussion:

"With a prescribed CCN concentration and coarse horizontal grid spacing, capturing realistic cloud droplet activation is challenging. Prognostic aerosols using a dynamic aerosol model such as ICON-ART (Aerosol and Reactive Trace gases module) would improve the representation of the local CCN concentrations and provide a more realistic cloud droplet activation. Furthermore, accurate representation of updrafts ensures a more realistic simulation of cloud droplet activation, ultimately enhancing the overall predictability of cloud properties."

Line 153-154: This sentence is repetitive with the following sentences and can be removed.

Ok.

Line 161-162: Is there some justification for why "rain freeze" is the only primary nucleation mechanism for T > -12 C? Most (all?) of the pertinent clouds in these simulations are within this temperature regime, such that immersion freezing, and other nucleation mechanisms are not important at all. This would then require rain to form before ice could start forming. "Rain" is not common in these cloud as there is simply not the moisture and dynamics to form rain drops. There can be supercooled drizzle at these temperatures, so is that included in the "rain freeze" mechanism? If so, then this point should be discussed more clearly. If not, then it is not surprising

that the lowest level cloud is typically almost entirely comprised of liquid water. Ice in that layer would only start to form due to what should be rare formation of rain or seeding from above (which could all then be multiplied by SIP).

→ Rain freeze was the only primary ice production mechanism in the original version of the model, this corresponds to the 1.6km simulation. We have clarified this statement by referring to Sect. 3.2 where the temperature threshold for immersion freezing is altered to better represent these warm clouds, we also clarify that rain includes drizzle.

Sentences have been altered to better describe this: "Rain freezing, which also includes the freezing of drizzle drops, is counted as a source of primary ice as rain droplets implicitly contain many INPs. "

and
"Rain freeze is the only ice production mechanism above -12°C currently implemented in the model, Sect. 3.2 introduces the changes performed on the immersion freezing parameterisation to improve on this representation." We have also added further emphasising sentences to ensure this is clear in Section 3.2 as well as in the result section when mentioning the INP scaling.

If the model says that there is a lot of rain forming in these clouds, then it is probably not properly representing natural clouds and the issue should be better understood.

→ Rain doesn't necessarily mean large droplets falling to the surface, but the formation of larger droplets inside the cloud due to collision-coalescence.

Finally, there is discussion (i.e, Line 193-196) about scaling all of the ice nucleation modes by a factor of 0.05. But when are deposition and immersion freezing active?

→ This is specified in Section 3.1.2.

The clouds simulated in the case studies are all warmer than the cut off thresholds for these two nucleation mechanisms. Then in lines 197-198 there is a statement about adding INPs at high temperatures (up to -7C), but it is not clear if this is an adjustment to immersion freezing so that it can occur up to -7 instead of only -12C. That point should be clarified.

→ We have added some clarifying sentences, thank you for pointing this out.

Even if immersion is adjusted to be active up to -7C, it still is not active in a lot of the clouds that are simulated.

→ Yes, some clouds (including the observed ones) are at temperatures above what is possible through primary ice production. This is why an investigation into the seeding and SIP is warranted as these are the only mechanisms possible to allow for ice at these temperatures.

Line 164-166 (and Line 440-441): "is known" is too strong here. Perhaps "is hypothesized." The community simply does not understand SIP well enough right

now to know what mechanisms are in action under what conditions, and what their net impact is on the ice properties.

→We can understand your wariness of SIP as these are mechanisms that are not fully understood. We hope our discussion section provides the critical view we share with you in regard to this.

To better reflect this view we change " is known" and "are commonly thought" to "hypothesised" in Line 164-166 and Line 440-441.

There is a "gap" between measured INP concentrations and measured ice crystal number concentrations, and people speculate that SIP might fill this gap. The "gap" between observations and models is another thing altogether. Surely it is possible to build and tune SIP parameterizations to fill any gaps that are present, but this does not confirm that SIP processes are actually the reason for the gap.

→ We apologise for the ambiguous language. We here speak about the discrepancy between modelled and observed ice crystal number concentrations that are commonly seen in the Arctic. Line 165 is now changed to "SIP effectively increases ice number concentrations and is hypothesised to "fill the gap" between observed INPs and measured ice crystal number concentrations as well as between measured and modelled ice crystal number concentrations."

Line 171-172: Same as my above comment. There are a number of statements throughout this paper that tend to push the conclusions about SIP beyond what can really be concluded. In this sentence "has been shown to have a considerable impact" is a challenge because these are model studies. In a model a given SIP parameterization can be tuned such that it has an impact on the modelled clouds, but that does not mean that those same processes are important in natural clouds. Some of the language here should be tempered to more closely reflect the state of understanding based on observational and laboratory studies, which is not definitive at this point.

→ We agree with you that SIP is not a process we fully understand, and we lack well-constrained parameterisations which further introduces uncertainties. We have tried to change the language to better reflect this uncertainty. This line now reads: "This process has been found to be weak when acting alone but in combination with ice multiplication from breakup upon ice-ice collisions, it has been shown to have a considerable impact in simulations of Arctic clouds" instead of "it has been shown to have a considerable impact on Arctic clouds".

Figure 5: It is hard to read the contour labels, please replot with larger font.

→ We have increased the size of the star as well as the contour labels, we hope this is large enough to read. An updated figure can be seen below (Fig. iv) together with the trajectories (following suggestions from Reviewer #1).
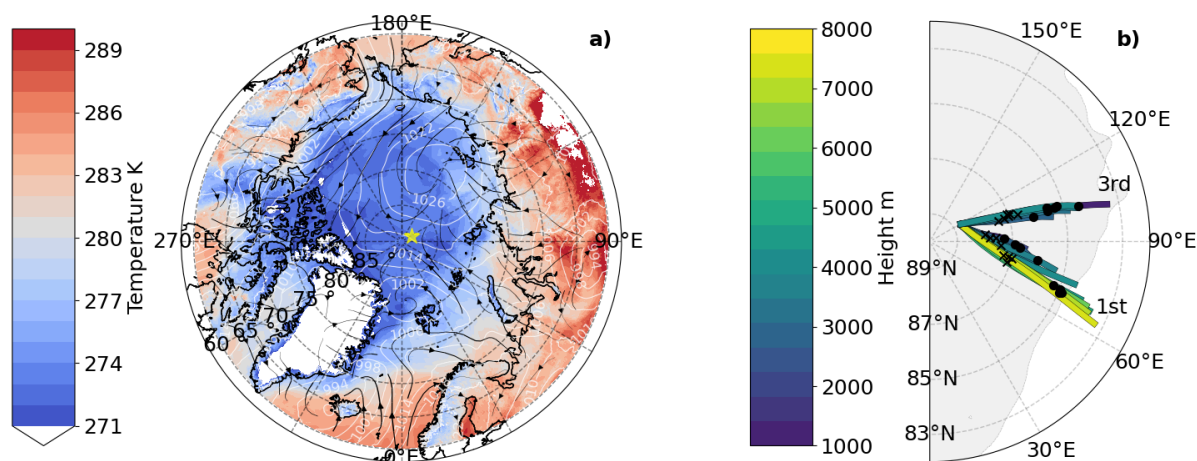
Fig. iv: (a) Synoptic situation similar to Fig.3 in the original manuscript and (b) back trajectories similar to Fig. B1 in the original manuscript.

Line 246: I'm not sure I've heard "hydrometeor content" before. How about just "water content" as in the labels?

Ok.

Line 263-264: What does this mean? Is there data assimilation at other times?

→ New information regarding the data assimilation procedure has been obtained. We will update the manuscript accordingly. Please see the section at the bottom of this document.

Line 264-265: I'm not sure that the strength of the inversion is what sustains the cloud. It is likely more so the other way around. The inversion is not present because the cloud is not there to radiatively cool and drive vertical mixing. I also do not see the justification for the next statement about excessive vertical mixing being the culprit. Please provide further clarification / justification.

→We can agree that it is not always clear if the cloud is driving the inversion or the other way around. To avoid miscommunication, we have decided to remove this statement.

→In regard to excessive mixing, few studies exist on this, and we commonly blame excessive mixing without further proof, however, one reference has evaluated this, and we instead refer to this one. The sentence now reads; "ICON is found to place inversions too high in comparison with observations, a similar error has been found to be due to excessive vertical mixing in the European Centre for Medium-Range Weather Forecasts (ECMWF) Integrated Forecast System (IFS) model (Sandu et al. 2013)"

Line 268-269: From Appendix A it is not possible to determine if the simulations are improved. On the 1st, the figure set up and chosen contours do not allow one to see the potential impact on resolving cloud-scale motions and variability, which would be

expected with higher resolution. On the 3rd, it does look like the higher resolution is starting to resolve some pulses of ice formation. I believe that starting to resolve some of these structures is actually a step in the right direction.

→ As we state in the discussion, higher resolution may definitely be helpful for certain analyses. We decided to not spend the computing resources on more high-resolution simulations as they did not improve enough compared to the cheaper simulations. We have now clarified our language:

"Further increases in resolution beyond 1.6km have large impacts on the cloud droplet number concentration (Fig.A2a) due to higher vertical velocities resolved with smaller grid spacing. Cloud ice is less affected but shows an increase with the 100~ simulation. This is shown in Appendix A. Due to the computational cost of running the high-resolution simulations the improvement (on especially cloud ice) is found to be too small to further study microphysical perturbations with this setup."

A new plot of the 100m contours is provided as it was noticed this simulation was plotted at a higher temporal frequency than the other simulations. For better comparability, this has now been amended.

We have now added vertical profiles of cloud mass and number concentration (of both liquid and ice) to the appendix (see Fig. ii and Fig. v below) and added a discussion on the increase in droplet number concentration with a smaller grid spacing.

On the 3rd:

"The time-averaged (06-12UTC, Fig.A2b) maximum liquid water content in the lower layer barely increases between the runs. In terms of droplet number concentrations larger impacts are seen with a 4-time (15-time) increase from the pr_1L^{-1} simulation for the 400m (100m) simulation respectively (Fig.A2a). This shows the impact of decreased horizontal grid spacing on the CCN activation. With a smaller grid spacing an increase in vertical velocity is found which increases the CCN activation. An increase in cloud ice by a factor of 4 can be seen for the 100m simulation (Fig.A2d), but this is deemed to be not enough impact to justify the large computational cost of performing microphysical sensitivity tests at such high resolution."

On the 1st:

"The time-averaged (10-12UTC) vertical profiles are shown in Fig.A4. An impact on cloud liquid is noted with a 3-time increase in droplet number concentration for the 100m simulation compared to the pr_1L^{-1} simulation. Meanwhile, the liquid water content only increases by a factor of 1.1. Cloud ice mass content in the upper layer increases by a factor of 2 while the ice number concentration increases by 5. In contrast, the lower layer does not see an increase in cloud ice with a smaller
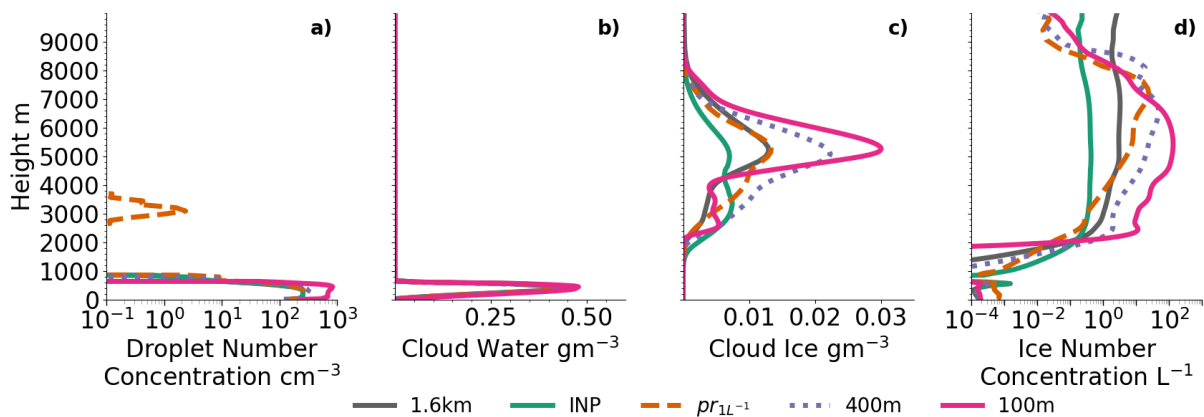
Fig.v: (a) Cloud droplet number concentrations, (b) cloud water mass content, (c) cloud ice mass content, and (d) cloud ice number concentration. All were calculated for the 1st of September as a temporal and spatial mean between 10-12 UTC. Added as Fig.A4 in the new manuscript.

Line 278-279: Based on some of the comments above, it is not surprising that there is no large impact on the lowest cloud. Since only rain freezing is possible at the given temperature of this cloud, and rain formation should be very rare in these clouds, it doesn't really matter how many INPs are present.

→ The upper cloud discussed here spans most of the troposphere and thus most temperatures for heterogeneous freezing. A sentence is added clarifying which layer is discussed. Furthermore, we add a longer discussion on the changes seen in ice mass concentration to highlight that we do see differences with a change in primary ice production:

"The upper layer shows small changes due to the scaling of the heterogeneous freezing/nucleation parameterisations. A reduction in INPs (at cold temperatures above 4km, see Fig.8e) induces small reductions in ice mass concentration in the upper levels of the upper cloud (above 4km). This amounts to a time-averaged (10-12UTC) reduction by a factor of 0.5 in the ice mass concentration. In the lower levels of the upper cloud (at warmer temperatures), an increase is noted. The time-averaged difference amounts to an increase by a factor of 1.3. The lower layer is unaffected by the changes due to its high temperature (Fig.6a,b)."

Line 284-286: This interpretation is not convincing for a couple of reasons. First, just based on Clausius-Clapeyron, sublimating ice will not provide enough moisture to then lead to liquid water saturation without a significant cooling of the air parcel. That cooling would have to come from vertical lifting of the parcel. But how would the parcel be lifted, ie. what provides the buoyancy? The text suggests that this is due to latent heating (i.e., condensational heating). But the source of moisture was from sublimation (cooling), which would cause the parcel to sink not rise. I don't think it is possible to have both sublimation and buoyancy generated from condensational heating at the same time. Perhaps I'm missing something that needs to be clarified?

In general, the model should provide the information that is needed to understand the thermodynamic balances at play as a function of height and to clearly distinguish why the model produced liquid water. Generally, there will need to be some convergence of moisture at that height, likely supported by advection (as suggested by the soundings), and the ice deposition rate must be small enough that some liquid water can form. Once that liquid water forms, the typical mixed-phase processes will kick in (radiation-turbulence-microphysics feedback) to allow the layer to persist for some time in the face of the low ice crystal concentration. To help understand this situation, it might be useful to do a simulation where ice is turned off altogether. I suspect that liquid water will form at that height supported by moisture advection. Then, as ice is turned on, and turned up, eventually the liquid cloud cannot sustain itself (as is shown by some of the simulations).

→We agree that we have not provided enough evidence for our line of thought. We have removed the discussion regarding the liquid layer appearance as it also does not contribute much to the storyline.

Line 287-288: Where is the evidence of this seeding? It looks like there is a full gap between layers without any falling ice in between.

→ Fig. 7 shows contour lines of snow and graupel entering the lower layer between 10-12UTC. A reference to this plot is added after Line 289 to clarify.

Line 289-290: The fact that the cloud layers "seem quite impervious to perturbations" is concerning. There is a lot of literature on modeling studies that show clear sensitivities to ice. It should be possible to turn up the INP concentration high enough to achieve glaciation in the lowest cloud. So why does this model not show much sensitivity? It could be that 1) the cases are just so warm that there is not much that can be done to promote ice formation, or 2) the general model set up (the parameterizations and how they are implemented) is not fit for the purpose of simulating these clouds. What happens if this model runs a colder mixed-phase cloud case (like one of the classics from MPACE or ISDAC or a MOSAiC case from earlier in the year)? Is there sensitivity to INP concentration?

→ This is answered above under "The second significant concern". To be added here, yes, for this cloud, option 1) is true. To achieve glaciation of the lower layer on the 1st we require very efficient seeding as there is no primary ice production in the lower layer due to its high temperature (cloud top temperature about -3°C). We achieve some seeding (pr_1L^{-1} simulation) but this weak seeding is not enough to impact the lower layer due to the inefficient glaciation at this temperature. The seeding impact will be the focus of future publications so to limit the extent of this paper we decided to not perform further scalings of sedimentation velocity and INPs to perturb the lower layer through seeding.

Line 308-310: WBF can only glaciate the cloud if there are enough ice particles. To better understand this point, what are the actual ice number concentrations?

→Thank you for pointing this out, we have indeed not shown any ice number concentrations. Some of the time-averaged (06-12UTC) ice number concentrations were previously shown in Fig. B6. This plot, shown below (Fig. vi), is updated in the manuscript and moved to the main text (new Fig. 11). For INPx1E4 and INPx1E6 we are looking at maximum ice number concentration values in the upper layer of 0.22 per L and 6.7 per L respectively, compared to 0.00060 per L for the INP simulation. We have now added these values to the text with references that also do not see glaciation at these ice number concentrations.

"With this high INP concentration, the ice number concentration increases as well. Time-averaged (06-12UTC) maximum ice number concentrations in the upper cloud layer reach 0.22 per L and 6.7 per L for the INPx1E4 and INPx1E6 simulations respectively (Fig 11e). This is compared to 0.0006 per L for the INP simulation, an increase of two and four orders of magnitude respectively. However, irrespective of this substantial increase in ice number concentration, these values do not necessarily lead to the glaciation of the cloud (Stevens et al. 2018, Solomon et al. 2018) and this phenomenon is not observed in these simulations either."
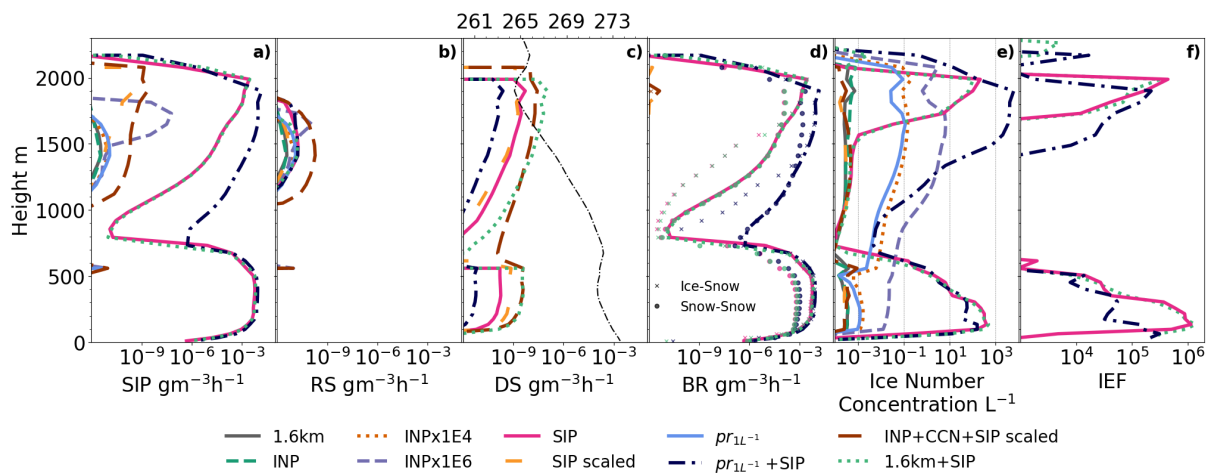
And new Fig.11:

Fig. vi: Updates to Fig.B6. Two new runs have been added; INP+CCN+SIP scaled and 1.6km+SIP. Vertical profiles of other runs have been added and the ice enhancement factor is shown as a vertical profile (f).

We further add a comment on the ice number concentrations reached for the SIP simulations within Section 5.4:

"Time-averaged (06-12UTC) maximum ice number concentrations in the lower cloud layer reach 484 per L, 396 per L and 156 per L for the 1.6km+SIP, SIP and pr_{1L}^{-1} + SIP respectively (vertical profiles are shown in Fig.11e). The large increase in ice number concentration drives the increase in vapour deposition through the WBF process (Fig.B2c,f) resulting in the glaciation of the lower layer."

Is it the ice number concentration that increased by two orders of magnitude or only the INP concentration?

→We apologise for the ambiguous language. Here we mention the ice mass concentration; this has now been remedied in the paper. The INP concentration has been increased by 1E4 and 1E6 respectively for the simulations discussed here.

In these simulations it seems apparent that primary nucleation is a limiting factor such that there are not enough ice particles to make WBF much of a factor; if the available INPs were to nucleate into ice crystals there should be plenty of ice for the WBF (although WBF is also limited at these warm temperatures). Generally, these results seem to suggest that the number of INPs is not the problem with this model set up, but it is rather the parameterizations for ice crystal nucleation.

→ We have tried to improve the language throughout the text to better show the impacts of changing the primary nucleation. We do see impacts as discussed further above and we hope through these new additions this is shown more clearly.

Line 312: Here the CCN activation rate perturbation is introduced. It would be useful here or earlier when the simulations are introduced to more clearly outline why the different simulations are performed. In this case, for example, why is a perturbation of the CCN implemented? Is there some hypothesis that the properties of the liquid drop size number/distribution are important in the ice processes? Please explain so that the reader understands the logic behind the different simulations.

→ Line 209 states the CCN activation scaling. This scaling was decided as the model produces too much liquid water.

We add a clarifying sentence "To investigate the impacts of the cloud droplet activation on the too-thick modelled liquid layer, a sensitivity study with a decreased cloud droplet activation rate (pr_1L^{-1} + CCN) is performed."

Line 319: It looks to me that the LWP is 3-4 times too high and not 5 times too high.

→ Yes, you are right. 4.2 to be exact, we will change to "4 times".

Line 327-330: The explanation for the appearance of this second layer seems to be speculative and it is not clear why evaporative cooling would lead to mixing. Additionally, there is speculation in the following sentences about the upper layer then radiatively shielding the lower layer so that it "dissipates." It is possible that these mechanisms are in operation, but instead of speculation it would be best to look at the tendency terms from the model to definitively state why the model creates another layer and then why that layer goes away. For example, temperature tendency terms (radiative, latent) or water tendency terms (evaporation, condensation) will be informative in this regard.

→As we have not obtained these tendency terms at the time of the simulation we will simply remove the speculation in regards to the formation of a third layer. A sentence

in regards to this now states: "The cloud structure changes during the time window 07-09UTC where the presence of a third layer is evident."

Line 333: This statement about the upper layer causing the lower to dissipate is likely true and has been described by a few papers both observationally and within models. However, the description of this process has revealed the point that the authors tend to describe what is playing out in this Eulerian perspective as if it were a Lagrangian perspective. While the additional cloud layer only lasts for a couple of hours within the stationary vertical column, this perspective does not represent how long the cloud itself actually lasts. This could simply be a second cloud deck that advects into and then out of the vertical column of interest. When it leaves the vertical column, this does not necessarily mean that it dissipates as it is also advecting at the time. The authors should consider the difference between Eulerian and Lagragian perspectives when describing the interactions and transitions, both here and throughout the manuscript.

→Yes, you have a valid point. We have played around with an upstream perspective as well but that has not been used for this analysis. We have decided to remove the discussion about the generation of the third layer due to the lack of advective tendency outputs from the model.

Line 336: Which increase in condensation?

→ The increased condensation (due to the saturation adjustment) in the "third layer" was shown in Fig. B4e in the original submission at a height of about 1200m. This plot has now been updated and the condensation rates are shown in Fig. A2 in the revised manuscript.

We clarify: " With the presence of more cloud liquid, due to the presence of a third cloud layer (Fig.9e), condensation increases due to the saturation adjustment within this layer (Fig.A2e) and the resulting impact of the CCN scaling is small."

Line 355: "ice concentration": Does this mean ice number or mass concentration? Please be clear.

→ We have now edited all instances where this is ambiguous, thank you for pointing this out!

Line 355-356: This statement could possibly be true under certain circumstances, but it should be shown with model results. This process would totally depend on the ice crystal number concentration, which is not given. In general, even for enhanced ice cases, the number concentration of ice crystals is still likely very low. When one considers the size of a crystal, and the number of ice crystals and liquid droplets per volume, there is typically a large physical spacing between ice crystals with many liquid droplets (i.e., lots of surface area for evaporation) in the vicinity of each ice crystal, providing ample vapor supply to grow the ice.

→We may remove this part of the sentence as it does not provide much more information for the next part.

Line 381-383: this collisional breakup is a positive feedback, where more particles makes more collisions, which makes more particles. Thus, it is indeed an attractive mechanism to multiply ice concentration. This also means that the decisions made regarding the parameterized efficiencies (collision efficiency, breakup efficiency, number of resulting particles per breakup event, etc) are very important and some might be highly sensitive. This point should probably be discussed.

→We briefly discuss this point in the discussion section where we deemed it not worth the computing resources to arbitrarily tune these parameters to our setup. Instead, we provide a simulation using the scaled breakup upon ice-ice collision parameterisation, which uses the diameter of the colliding particles for a more physical constraint of the breakup process.

Line 384-391: Given that seeding plays only a small role and does not appear to be the culprit for glaciation towards the end of the case, why does this glaciation occur?

→Thank you for bringing this point up. We have investigated and it seems the SIP in the lower layer (which is the more obvious glaciation as the upper layer dissipates in all runs) is driven by a small change in temperature in the lower layer at 10UTC-11UTC, please see the plot below (Fig.vii). Even a small change in temperature substantially changes the number of fragments generated within the breakup parameterisation.
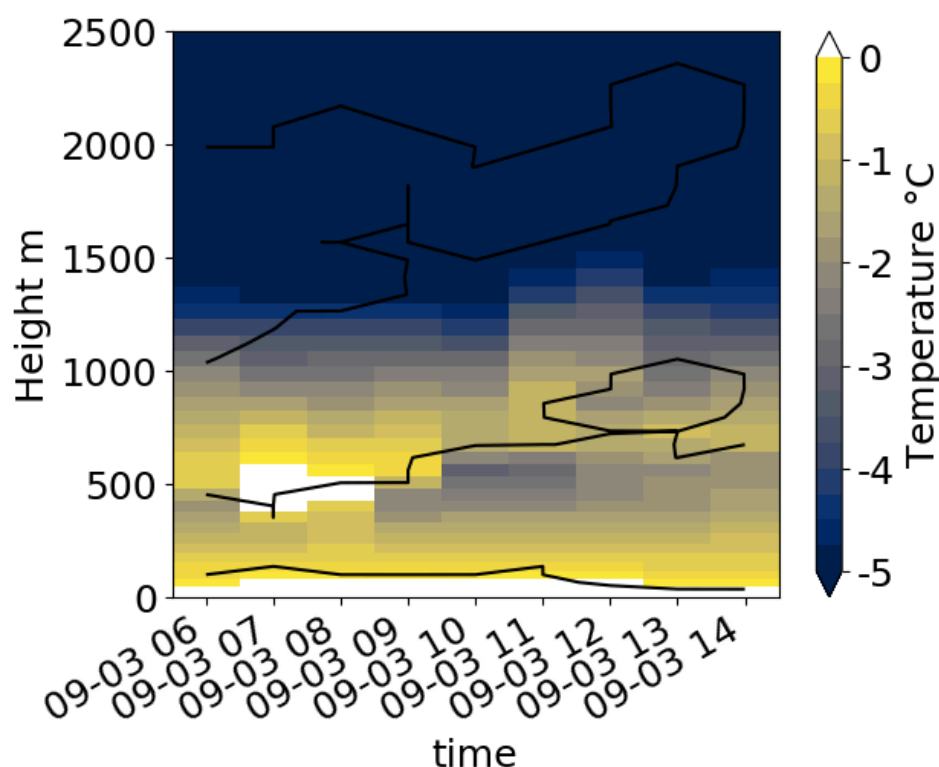

Fig. vii: Time-height plot of temperature for the SIP simulation. The explosive tendency in the breakup upon ice-ice collision mechanism is governed by the temperature drop starting at 10UTC in the lower layer. Contours show liquid water

content plus ice water content at a specified contour at 10^-5 g/m^3 to outline the cloud.

We update the manuscript:

"The breakup upon ice-ice collision parameterisation is highly sensitive to temperature fluctuations and the sudden onset of glaciation through ice particles generated by SIP is due to a drop in temperature of the lower layer."

The SIP processes are presumably occurring for many hours prior to the point of full glaciation. What is special about that transition? Line 398-399 further suggests that there is a time dependence to the SIP impact. First, it is difficult to interpret time-dependence in this Eulerian perspective. Second, if there is indeed time dependence, what is the mechanism for this?

→We talk about time dependence from this Eulerian perspective with regard to how long the simulation has been running but we agree that it is ambiguous. We have now removed this statement.

Line 393-395: I think this sentence states that there are no sinks of ice and that the ice that occurs at the analysis point was initialized when the model was initialized and advected all the way to the analysis point (i.e., it is the same ice). However, I do not believe this is the case unless there is something strange with how the model represents fall speed. Ice crystals typically fall at about 1 m/s, so they would fall more than 3 km in 1 hour. Thus, there is a sink of ice crystals from any given layer of the atmosphere due to fall speed. Moreover, the residence time of ice in the atmosphere relative to advection from the domain boundary is something that can be determined directly from the model.

→We apologise for the unclear statement. We simply mean that some of the cloud ice within the domain is not produced during the simulation due to the initialisation and the updating boundaries. However, there is a lack of strong sinks such as large rates of snow formation. Regarding sedimentation, we have calculated the sedimentation velocities of the ice crystals through mean ice crystal mass and found these to be low. Through a mass-diameter power law, ice crystal sizes vary around 60-80 micrometres (close to the cloud top on the 3rd of September) in diameter which gives a sedimentation velocity of about 10 cm/s. This means it takes ice crystals found at the top of the upper layer on the 3rd about 6 hours to fall to the surface indicating that the ice seen at the ship location is indeed replenished through the simulation.

We update the line; "Here, as we are dealing with a real setup, some of the cloud ice within the domain is supplied from the continuous advection of cloud hydrometeors through the domain boundaries."

→ Unfortunately, no advective tendencies were collected during the simulation.

Line 433-435: It is not clear how the extent of these clouds should matter. Please explain further. There are many past model studies that have been set up in a similar way and have shown sensitivity to CCN, so why not in this model?

→We realise our language does not reflect our results as well as we would have hoped. We were surprised by the lack of large impacts and thus this language has followed within the manuscript. Of course, we do see impacts and these have now been clarified throughout the manuscript as well as here:

"The reduction of CCN activation within the model impacts the structure of the clouds and the appearance of a third layer is evident. This reduces the impact of the reduction of CCN due to increased condensation within this layer and only a small change in LWP (reduction by a factor of 1.28) can be seen."

Line 437: "…does not survive for more than two hours." Given the Eulerian perspective you can only say that the cloud does not exist in the analysis column for more than two hours. However, within the model you could track the cloud elements to see how long the cloud actually lasts along its Lagrangian trajectory, assuming the domain is sufficiently large.

→Yes, you are right, this statement is misleading, we have removed it.

Line 455-456: Indeed, the theta profile from the case suggest that there is not strong coupling with the near surface, such that the surface aerosol measurements are likely not representative of cloud level. In fact, the INP concentrations are likely larger aloft (based on some other work from MOSAiC and elsewhere).

→Yes, thank you for the comment. As stated in the manuscript we agree with your reasoning.

Line 467: It is more useful to know the actual vertical resolution at cloud level than the number of layers in the simulation.

→This is now added to the model setup section. The sentence now reads: "The 6km and 1.6km runs are kept to 90 levels while the 400m (100m) run is increased to 150 (200) vertical levels. This translates into a vertical grid spacing at the lower cloud top (~ 600m) of about 55m for the 6km and the 1.6km. The 400m and 100m simulations have 39m, and 32m vertical grid spacing respectively at a similar height."

Figure A2: This comparison suggests that there is way too much ice/snow formed in the upper cloud. What does this say about other problems with ice nucleation and growth? Also, it is not possible from these plots to evaluate the impact of resolution on the lowest clouds, which should be the ones that are most impacted by resolution. It looks like no ice is forming in them, leaving way too much liquid.

→ This is discussed within section 5.2, where we tune down the heterogeneous freezing mechanisms at colder temperatures. The lower layers are too warm for heterogeneous freezing to occur, they can only be seeded from above which is

something that we see in one of the simulations on the 1st but it is weak and does not impact the lower layer.

We have added vertical profiles in the appendix that may improve the analysis of the lower layer.

Figure B1: What is the gray shading? That should be added to the caption.

→ Thank you for noticing. We have now added a line explaining the sea ice concentration extent in grey contours.

Figure B5: What is the "heterogeneous freezing" and why is it at the bottom of the upper clouds? I assume this is the rain freeze mechanism and thus there is freezing at the bottom where there is the most rain? How are the particles at the top of the cloud formed? All of the particles will, on average, fall so there needs to be a particle source at the top, not just the base.

→ Heterogeneous freezing is mostly immersion freezing but also includes small additions from rain freeze. It is present all through the cloud layer, however, note the x-axis, comparatively to vapour deposition the rate of heterogeneous freezing is very low. To make this more clear, we have edited the plot such that vapour deposition is the last to be stacked. The new figure is shown below in Fig. viii.
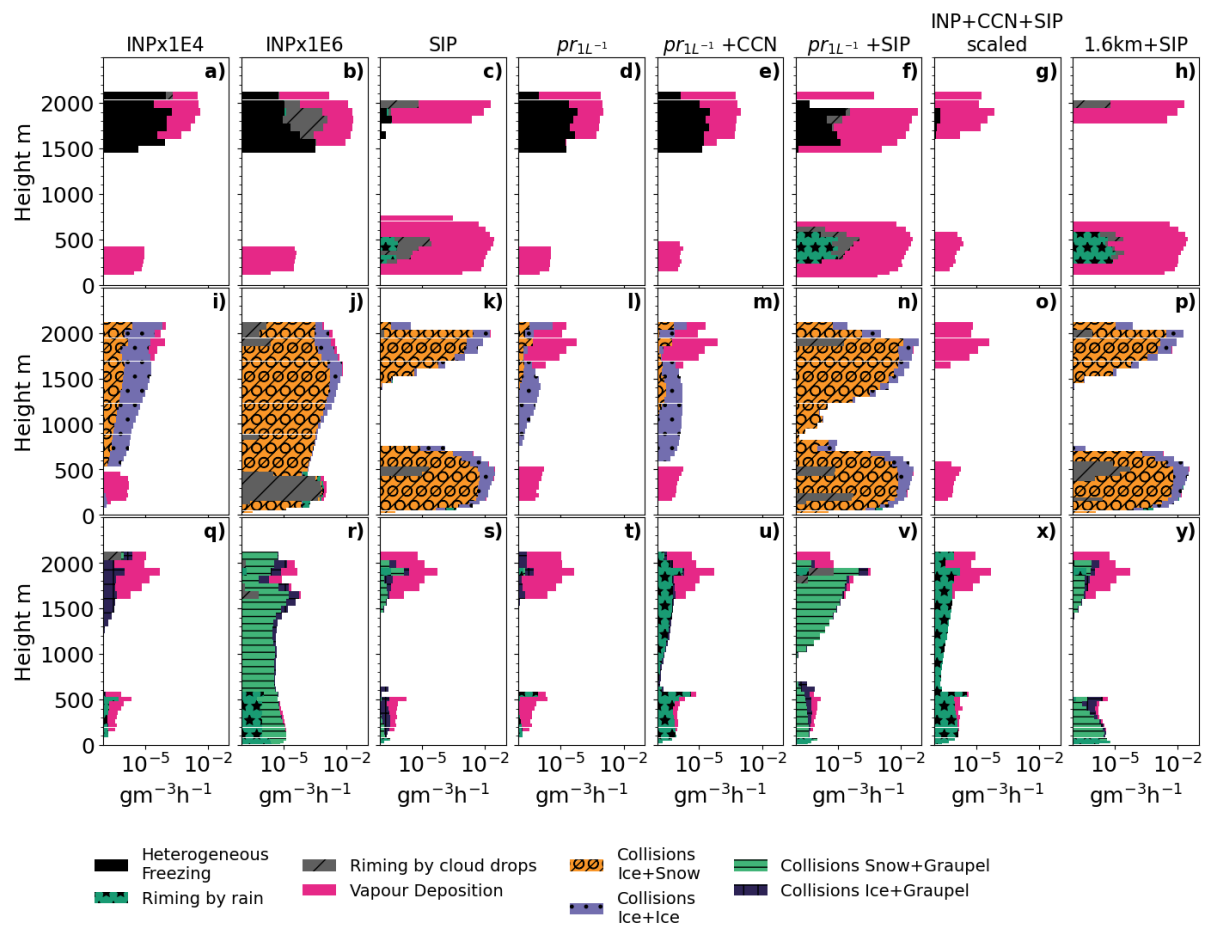
Fig. viii: Process rates as shown in Fig. B5 in the original manuscript, here with a change of plotting order to highlight the heterogeneous freezing and the new runs added to the analysis (as suggested by Reviewer #1) (INP+CCN+SIP scaled and 1.6km+SIP).

Line 567-568: This citation is incomplete. Should include: Atmospheric Research, 51, 45-75.
Line 576-577: This citation is incomplete. Should include: Atmos. Phys. Chem., 12, 9817-9854.
Line 639-640: This citation is incomplete. Should include: J. Atmos. Sci., 62, 1665-1677.
Line 646-647: This citation is incomplete. Should include: Nat. Geosci., 5, 11-17.
Line 671-672: This citation is incomplete. Should include: J. Atmos. Sci.
Line 691: This citation is incomplete. Should include: J. Atmos. Sci., 63, 697-711.
Line 723-724: This citation is incomplete. Should include J. Climate
Line 736-737: This citation is incomplete. Should include J. Atmos. Sci.

→ Thank you for finding these discrepancies, all have now been edited.

Additional changes:

New information regarding the data assimilation strategy of MOSAiC radiosondes has been obtained. All radiosondes were assimilated from the campaign. Variables include temperature, wind profiles, and relative humidity. We are updating the manuscript accordingly.