First of all, we appreciate the reviewer's comments and suggestions. In response to them, we have made relevant revisions to the manuscript. Listed below are our answers and the changes made to the manuscript according to those comments and suggestions. Each comment of the reviewer (black) below is followed by our response (blue).

I have carefully read the paper "Examination of varying. Iced-phase stratocumulus clouds in terms of their properties, ice processes and aerosol-cloud interactions between polar and midlatitude cases: An attempt to propose a microphysical factor to explain the variation". The paper proposes a series of simulations to compare the mid-latitude and polar cases of mixed-phase clouds. The motivations of the paper are indeed important, most of the studies are focused on warm clouds and not on mixed-phase clouds, as mentioned by the authors, even if there are more and more studies about them currently. The analysis is based on LES simulations to better understand the aerosol-cloud interactions. Interesting results have been derived from the study, for example ice processes increase the total cloud mass for the polar case compared to the polar warm cloud counterpart when ICNC/CDNC and IWC/LWC are high (and the opposite for mid-latitude clouds). The study is very comprehensive and the authors have done a lot of different simulations to explain their analysis, but I have concerns about the methods and the conclusions seem to be rushed or more clarification is needed. Also, I think clarifications are needed to better understand the paper as it is currently difficult to follow with the names of the simulations which are not clear to understand and remember what is associated with what. The current version needs to be rewritten to make it clearer. I do not have a strong expertise in models so I focused my review on the other parts. My concerns are detailed below.

General Comments

- Mixed phase clouds are different in the Arctic and mid-latitudes: In the Arctic, the liquid layer is usually on top of the ice layer, whereas in mid-latitudes mixed-phase clouds consist of pockets of liquid and ice within the clouds. These differences are not mentioned in the paper and have implications for ice processes, especially Wegener Bergeron process. How are mixed-phase clouds represented in the model? Can the authors comment on the possible biases arising from this?

As seen in Figures 4, 8 and 9, the liquid layer is not on the top of the ice layer in the case adopted by this study. Instead, the liquid layer is in the middle of the ice layer as seen in Figures 4, 8 and 9. Stated differently, the pockets of ice particles are not separated from those of liquid particles but mixed with those of liquid particles in the case adopted by this study as in the mid-latitude mixed-phase clouds including those in Lee et al. (2021). Hence, we do not believe that there are significant biases arising from differences between the Arctic and mid-latitude clouds in terms of the location of the liquid layer with respect to that of the ice layer. To indicate this, the following is added:

(LL495-498 on p17)

We hypothesized that ICNC/CDNC can be an important factor that determines above-described differences between the polar and midlatitude cases. Note that both in the polar and midlatitude cases, pockets of ice particles and those of liquid particles are mixed together instead of being separated from each other as seen in Figure 4 and Lee et al. (2021).

- It is usually difficult to disentangle the dynamical, microphysical changes and natural variability in model simulations, as both are bound in the parameterisation. I wonder if we can disentangle the effect of cloud evolution on ACI just by looking at different simulations, can the authors comment on that?

First of all, since the reviewer here talks about ACI, we want to limit the discussion here to the representative simulations for the polar case with varying aerosol concentrations, which are the standard simulations. The standard simulations are the 200_2, 2000_20, 2000_2 and 200_20 runs. Here, we want to emphasize that differences in these standard simulations are only in the initial aerosol concentrations and there are no other differences between them. For example, synoptic environmental conditions, as initial and boundary conditions, imposed on the simulations are identical among the standard simulations. Hence, differences in the simulated results regarding cloud processes and variables, and their evolutions are caused by differences in aerosol concentrations among the simulations. In particular, the differences in evolutions of clouds and associated cloud processes and variables can involve differences in feedback among cloud processes and variables, and aerosols. This feedback is that differences in aerosol concentrations trigger those in cloud processes, such as cloud particle nucleation and precipitation, and then cloud variables, such as cloud-particle size and concentrations. Then, these aerosol-triggered differences in cloud processes and variables in turn affect aerosol concentrations and their impacts on clouds. In addition to that, aerosol-triggered changes in cloud processes and variables, such as cloud-particle nucleation, size and concentrations, and precipitation, affect latent-heat processes and dynamics (e.g., updrafts), and changed dynamics in turn affect latent-heat processes and cloud processes and variables, such as cloud-particle nucleation, size and concentrations, and precipitation. Here, we want to emphasize that all of the differences or changes in cloud processes and variables, such as cloud-particle nucleation, size and concentration, and precipitation, latent-heat processes and updrafts, their feedback among themselves and their feedback with aerosols are triggered by "differences or changes" in aerosol concentrations among the simulations. Here, changes in these cloud processes, variables and their feedback among themselves represent changes in cloud evolution, and changes in the feedback of cloud evolution with aerosols, which are associated with impacts of changes in cloud evolution on aerosols and their impact on clouds, represent impacts of changes in cloud evolution on ACI as termed by the reviewer here.

In this study, as mentioned, not only the changes in cloud evolution, but also the impacts of these changes in cloud evolution on ACI are triggered by changes in aerosol concentrations. In this study, we take interest in how changes in aerosol concentrations trigger these changes in cloud evolution and impacts of those changes in cloud evolution on ACI altogether. Stated differently, our main interest is in how all of these aerosol-triggered subsequent combined changes in cloud evolution and impacts of those changes in cloud evolution on ACI affect IWP and LWP, but not in the separation of aerosol-triggered changes in cloud evolution from impacts of those changes in cloud evolution on ACI. Hence, we perform analyses of results by looking at the averaged condensation, deposition, IWP, and LWP which are over the whole simulation period and thus final or combined products of aerosol-triggered changes in cloud evolution and impacts of these changes in cloud evolution on ACI.

- The study is difficult to read with all the different names of simulations that are not explicit about what they correspond to, and considering the number of simulation, it became difficult to figure out the corresponding simulation. I suggest that the names are renamed to better understand.

The other reviewer raised a similar issue as follows:

"Is stating INP meaningful in "noice" experiments? I suggest that the authors replace "200_2_noice" with "200_0" and "2000_2_noice" with "2000_0". In the same way, I suggest to replace "200_2_fac10" with "200_0.07" (also the others including "_fac10") as the names are neither intuitive nor consistent with others"

There are many ways to name simulations. In the paper, since we performed various simulations mostly by varying CCN and/or INP number concentrations, to reflect different CCN and/or INP number concentrations, due to the variation of CCN and/or INP number concentrations among simulations, CCN and INP number concentrations are explicitly put into the name of those simulations. We believe that this method of naming simulations is most effective in making readers understand how simulations are different from each other at a glance. Hence, we stick to the name method in the current manuscript. However, following the comment by the other reviewer above, some of names are changed.

- There are many sentences with the use of brackets to express two ideas in the same sentence, making it difficult to read. For example in lines 621, 632, 635,... I recommend simplifying those.

We minimized the use of those brackets by picking them, which are unnecessarily used, up, remove them and rephrase associated text.

- I did not find many details about the measurements (CCN measurements, Cloudnet...). Can the authors add information on where the data come from? How it is retrieved? Also, I would have liked the authors to show some plots comparing the model output with the observations when it is possible.

The details of CCN measurements are added as follows:

(LL279-289 on p10)

The measurement of the CCN concentration has been carried out at the Zeppelin research station in the domain, using the commercial droplet measurement technologies CCN counter with one column (CCNC-100), managed by the Korea Polar Research Institute, since year 2007. The CCNC-100 measures the CCN concentration at supersaturations of 0.2, 0.4, 0.6, 0.8 and 1% (Jung et al., 2018). The aerosol number size distribution is observed using a closed-loop differential mobility particle sizer (DMPS). The DMPS charges aerosol particles and exposing them into an electric field, which causes them to experience a force proportional to their electrical mobility, resulting in their classification according to size (Tunved et al., 2013). Aerosol composition is measured using aerosol mass spectrometry (AMS). The AMS measures the composition by vaporizing and ionizing aerosol particles.

The details of the Cloudnet observation are added as follows:

(LL241-251 on p9)

In the Svalbard area, Norway, a system of mixed-phase stratocumulus clouds existed over the horizontal domain marked by a red rectangle in Figure 1 and a period between 02:00 and 10:00 local solar time (LST) on March 29th, 2017. These clouds are observed by the Cloudnet ground observation that has been established to provide a systematic evaluation of clouds in forecast and climate models. The Cloudnet observation aims to establish a number of ground-based remote sensing sites, which would all be equipped with a specific array of instrumentation, using active sensors such as lidar and Dopplerized mm-wave radar, in order to provide vertical profiles of the main cloud variables (e.g., LWP and IWP), at high spatial and temporal resolution (Hogan et al., 2006). The Cloudnet observation provides data of important cloud variables such as LWP and IWP to the public and this study utilize these data.

(LL376-383 on p13)

This study adopts the Cloudnet ground observation to evaluate the 200_2 run. Observed LWP is provided by radiometer in the Cloudnet observation. The retrieval of IWP is performed by using radar reflectivity and lidar backscatter in the Cloudnet observation as described in Donovan et al. (2001), Donovan and Lammeren (2001), Donovan (2003) and Tinel et al. (2005). In the retrieval, the lidar signal and radar reflectivity profiles are combined and inverted using a combined lidar/radar equation as a function of the light extinction coefficient and radar reflectivity. The combined equation is detailed in Donovan and Lammeren (2001).

More plots comparing the model output with the observations are added in Figure 5.

- On line 721, the authors say that ICNC/CDNC increases with IWC/LWC and base their analysis on this. But this is not true for the 200_2 simulations, as the authors state. So I do not understand why they infer results and generalize it when one of the situations does not work. Can the authors comment on that?

Just want to emphasize that as described in text, this study is only about an "attempt" to test a factor which is ICNC/CDNC and can help us with the development of the general principle about the variation of IWC/LWC among different cloud systems but not about the establishment of a perfect, complete general principle as already stated in text as follows:

(LL162-169 on p6)

Here, we want to emphasize that this study does not aim to gain a fully established general principle, but aims to test the factor that can be useful to move ahead on our path to a more complete general principle. Hence, this study should be regarded a steppingstone to the established principle, and should not be considered a perfect study that get us the fully established principle. Taking into account the fact that even attempts to provide general factors for the general principle have been rare, the fulfilment of the aim is likely to provide us with valuable preliminary information that streamlines the development of a more established general principle.

Hence, this study does not aim to come up with a perfect, complete general principle which can be applicable without any limit or conditions. Instead, this study aims to come up with a principle which can be applicable with a certain degree of limit or conditions, though this study tries to minimize the magnitude of the degree. As stated in text, we want to emphasize that "Even attempts to provide general factors for the general principle have been rare". Thus, even a principle with a certain level of limit can provide us with valuable preliminary information that streamlines the development of a more established general principle. This research philosophy for this study is stated as follows:

(LL940-956 on p31-32)

This study finds that the relation between ICNC/CDNC and IWC/LWC is highly non-linear. This high non-linearity is closely linked to how the number concentrations of CCN and INP, and associated ICNC/CDNC change. For a specific situation where the ICNC/CDNC variation is relatively small and both the number concentrations of CCN and INP reduce, the increase in ICNC/CDNC can reduce IWC/LWC, although it is found that as a whole, the increase in ICNC/CDNC enhances IWC/LWC. Hence, mechanisms identified in this study, especially regarding the use of ICNC/CDNC as a simplified and useful tool to explain differences in IWC/LWC among different cloud systems, are not complete and entirely general. In addition, results in this study are from only two cases in two specific locations in the midlatitude and Arctic regions and the more generalization of these results in this study merits more case studies over more locations in those regions, for example, in terms of above-mentioned sedimentation intensity, different factors (e.g., environmental factors) other than ICNC/CDNC, different sources and advection of aerosols, the magnitude of the variation of ICNC/CDNC and the way number concentrations of CCN and INP vary. Hence, findings particularly about relations between ICNC/CDNC and IWC/LWC in this study should be considered preliminary ones that initiate future work to streamline the development of the general parameterizations.

The limit or conditions to consider for the use of ICNC/CDNC to explain the IWC/LWC variation are described as follows:

(LL803-814 on p27)

The high-degree non-linearity in the variation of IWC/LWC is epitomized by the 1706 percent increase in IWC/LWC for the 163 percent increase in ICNCavg/CDNCavg from the 200_0.7 run to the 2000_2 run. This 1706 percent increase in IWC/LWC is induced by increases in both the initial number concentrations of CCN and INP between the runs (Table 1). In other transition from a simulation in a row to that in the next row in Table 4, there are decreases in both the initial number concentrations of CCN and INP, or there is either a change in the initial number condensation of CCN or INP. When either the initial concentration of CCN or INP changes in the transition, less than a 100% increase in IWC/LWC is shown. The decreases in both the initial number concentrations of CCN and INP, which are from the 2000_20 run to the 200_2 run, result in the decrease in IWC/LWC. Hence, depending on how the initial number concentrations of CCN and INP change, the magnitude and sign of the change in IWC/LWC can vary substantially.

(LL793-802 on p26-27)

Here, it is notable that the percentage difference in ICNCavg/CDNCavg is ~9% between the 2000_20 and 200_2 runs and the smallest among those differences in Table 4. The other differences are larger than 80%. Hence, the percentage difference in ICNCavg/CDNCavg for a pair of the 2000_20 and 200_2 runs is at least ~one order of magnitude smaller than that for the other pairs of the runs in Table 4. This means that findings from the comparison between the 200_2 and 200_0.07 runs are not suitable to explain the variation of IWC/LWC among clouds when the variation of ICNC/CDNC is relatively insignificant. According to Table 4, it seems that the variation of ICNC/CDNC should be greater than a critical value above which those findings are useful to account for the IWC/LWC variation among clouds.

- The authors infer different glaciation processes from the change in ICNC/CDNC, but I am not convinced that this is not directly related to the temperature difference between the Arctic and mid-latitudes. Can the authors make the argument explicit to rule out the temperature dependence?

Yes, it is true that in the comparison between the polar and midlatitude cases in this study, differences in ICNC/CDNC are mainly caused by differences in temperature between the cases as described in text:

(LL514-533 on p17-18)

ICNCavg/CDNCavg is 0.22 in the control run (i.e., the 200_2 run) for the polar case and 0.019 in the control run for the midlatitude case which is described in Lee et al. (2021). Henceforth, the control run for the midlatitude case is referred to as the control-midlatitude run. ICNCavg/CDNCavg is ~one order of magnitude higher for the polar case than for the midlatitude case. This is despite the fact that the ratio of the initial number concentration of aerosols acting as INP to that of acting as CCN is identical between the 200_2 and control-midlatitude runs. In addition, identical model, model setup such as vertical resolutions, and source of reanalysis data are used between the 200_2 and control-midlatitude runs, although there are differences in environmental conditions (e.g., temperature), cloud macrophysical variables such as cloud-top height and horizontal resolutions between the runs. Here, while taking these similarities and differences into account, we hypothesize that the significant differences in ICNCavg/CDNCavg between runs are mainly due to the fact that ice nucleation strongly depends on air temperature (Prappucher and Klett, 1978). When supercooling is stronger, in general, more ice crystals are nucleated for a given group of aerosols acting as INP. The average air temperature immediately below the cloud base over the simulation period is -16 ºC in the 200_2 run and -5 ºC in the control-midlatitude run. The average air temperature immediately above the cloud top is -33 ºC in the 200_2 run and -15 ºC in the control-midlatitude run. Hence, supercooling is greater and this contributes to the higher ICNCavg/CDNCavg in the polar case than in the midlatitude case.

However, it should be noted that although ICNC/CDNC differences are made by temperature differences between the cases in this study, for other cloud systems, we mentioned that ICNC/CDNC differences between those other cloud systems can be made by factors other than temperature differences as follows:

(LL897-904 on p30)

It should be emphasized that although this study mentions air temperature as a factor that affects ICNC/CDNC, ICNC/CDNC can be affected by other factors such as sources of aerosols acting as INP and those acting as CCN, and/or the advection of those aerosols. Hence, even for cloud systems that develop with a similar air-temperature condition, for example, when those systems are affected by different sources of aerosols and/or their different advection, they are likely to have different ICNC/CDNC, IWC/LWC, relative importance of impacts of INP on IWC and LWC as compared to those impacts of CCN, and relation between warm and mixed-phase clouds.

To effectively discern the impact of varying ICNC/CDNC on the variation of IWC/LWC across diverse cloud systems, this study specifically selected two cases characterized by notable temperature disparities, thereby yielding significant differences in ICNC/CDNC. However, as mentioned in text, discrepancies in ICNC/CDNC can arise from factors such as differences in sources and advection of aerosols beyond temperature differences between different cloud systems. While this study utilized temperature differentials to identify cases exhibiting significant disparities in ICNC/CDNC, its primary objective lies in comprehending the inherent role of ICNC/CDNC variations themselves in the discrepancies observed in IWC/LWC across diverse cloud systems, regardless of whether disparities in ICNC/CDNC are caused by those in temperature or in other factors, but not in how the disparities in ICNC/CDNC are caused. With the fulfillment of this objective, as long as we take interest in the role of ICNC/CDNC variations themselves among different cloud systems, the insights gleaned from this study regarding the influence of varying ICNC/CDNC on IWC/LWC can be extrapolated to scenarios where factors other than temperature differences contribute to discrepancies in ICNC/CDNC among different cloud systems. To clarify this, the following is added:

(LL904-910 on p30)

Regarding factors, which affect ICNC/CDNC, such as sources and advection of aerosols together with temperature , it should be noted that while this study utilizes differences in temperature among those factors to identify cases exhibiting significant disparities in ICNC/CDNC, its primary objective does not lie in the role of temperature differences in disparities in ICNC/CDNC, but in comprehending the inherent role of ICNC/CDNC variations themselves in the discrepancies observed, for example, in IWC/LWC, across diverse cloud systems.

- The nucleation mode between the Arctic and the mid-latitudes is very different due to the moisture regime. The Arctic region is less prone to immersion freezing and condensation freezing compared to the mid-latitudes because of the expected supersaturation of water vapour with respect to liquid. Is this related to the authors' conclusion? If so, would the information on relative humidity be helpful?

Yes, differences in dominant nucleation modes between the Arctic and mid-latitude cases can be considered to better understand findings from this study microphysically. However, we just want to emphasize that similar to what is mentioned in our response above, the primary objective of this study does not lie in the role of differences in factors such as not only aerosol sources and advection but also freezing modes as pointed out by the reviewer here, in ICNC/CDNC, but in comprehending the inherent role of ICNC/CDNC variations themselves in the discrepancies observed, for example, in IWC/LWC, across diverse cloud systems. Hence, we do not pursue the understanding of roles of freezing modes in differences in ICNC/CDNC between the cases.

- I think the authors' conclusion is too general (e.g., when the authors refer to polar regions, it actually corresponds to a region around Svalbard, which is a region of the Arctic). The conclusion should not be so promising because it could mislead readers.

The following is added:

(LL948-956 on p31-32)

In addition, results in this study are from only two cases in two specific locations in the midlatitude and Arctic regions and the more generalization of these results in this study merits more case studies over more locations in those regions, for example, in terms of above-mentioned sedimentation intensity, different factors (e.g., environmental factors) other than ICNC/CDNC, different sources and advection of aerosols, the magnitude of the variation of ICNC/CDNC and the way number concentrations of CCN and INP vary. Hence, findings particularly about relations between ICNC/CDNC and IWC/LWC in this study should be considered preliminary ones that initiate future work to streamline the development of the general parameterizations.

Specific comments

-Line 94: Mixed-phase clouds can also be found in convective clouds in the tropics.

The following is added:

(LL95-97 on p4)

When mixed-phase stratiform clouds are associated with convective clouds, they can form even in the tropical region.

- Line 105: "The radiative properties of liquid particles are substantially different from those of ice particles", I slightly disagree with this sentence. It is true that the radiative properties are not exactly the same, but the main difference is that ice crystals are larger and less numerous than cloud droplets (due to lower concentration of INP compare to CCN), so ice clouds tend to be less reflective than liquid clouds, and ice crystals precipitate, potentially altering the cloud lifetime.

The text pointed out here is removed and just added more references that indicate the importance of the relative proportion of liquid mass and ice mass in cloud radiative properties and in feedback between these radiative properties and changing climate, as follows:

(LL103-107 on p4)

The relative proportion of liquid mass, which can be represented by liquid-water content (LWC) or liquid-water path (LWP), and ice mass, which can be represented by ice-water content (IWC) or ice-water path (IWP), in mixed-phase stratiform clouds plays a critical role in cloud radiative properties and thus their climate feedbacks (Tsushima et al., 2006; Choi et al., 2010 and 2014; Gettelman et al., 2012; Zhang et al., 2019).

- Lines 134-136: "Many factors, such as environmental conditions..." The spatial distribution of the liquid and ice phases is also important as mentioned in the general comments.

See our response to the first general comment about the relative locations of liquid and ice phases. As seen in the response, the relative locations can be considered not to play an important role in differences in IWC/LWC between the two cases. However, in text pointed out here, we indicated the potential role of the relative locations of the layers of ice crystals and droplets or the spatial distribution of the liquid and ice particles in differences in IWC/LWC among different clouds as follows:

(LL134-137 on p5)

Lots of factors such as environmental conditions, which can be represented by variables such as temperature, humidity and wind shear, and macrophysical factors one of which is the relative locations of ice-crystal and droplet layers, can explain those differences.

- Line 165: "Get us the fully established principle": What would be needed to get the fully established principle?

As described in “Summary and conclusions”, the more fully established principle may need to take factors, such as sedimentation, wind shear, stability, aerosol sources, aerosol advection and the variation of ICNC/CDNC in the context of how CCN and INP concentrations vary, into account to explain differences in IWC/LWC (or IWP/LWP) and aerosol-cloud interactions among different cloud systems. To indicate this, the corresponding text in "Summary and conclusions” is revised or additional text is added to “Summary and conclusions” as follows:

(LL880-885 on p29)

Thus, different mechanisms controlling the differentiation can be expected regarding factors such as stability and wind shear as compared to ICNC/CDNC. The examination of these different mechanisms among stability, wind shear and ICNC/CDNC deserves future study for more comprehensive understanding of the differentiation or for an above-mentioned more fully established general principle explaining the differentiation.

(LL893-896 on p30)

For more generalization of results here as a way to the more fully established general principle, this potential role of sedimentation needs to be investigated by performing more case studies involving cases with strong precipitation in the future.

(LL940-956 on p31-32)

This study finds that the relation between ICNC/CDNC and IWC/LWC is highly non-linear. This high non-linearity is closely linked to how the number concentrations of CCN and INP, and associated ICNC/CDNC change. For a specific situation where the ICNC/CDNC variation is relatively small and both the number concentrations of CCN and INP reduce, the increase in ICNC/CDNC can reduce IWC/LWC, although it is found that as a whole, the increase in ICNC/CDNC enhances IWC/LWC. Hence, mechanisms identified in this study, especially regarding the use of ICNC/CDNC as a simplified and useful tool to explain differences in IWC/LWC among different cloud systems, are not complete and entirely general. In addition, results in this study are from only two cases in two specific locations in the midlatitude and Arctic regions and the more generalization of these results in this study merits more case studies over more locations in those regions, for example, in terms of above-mentioned sedimentation intensity, different factors (e.g., environmental factors) other than ICNC/CDNC, different sources and advection of aerosols, the magnitude of the variation of ICNC/CDNC and the way number concentrations of CCN and INP vary. Hence, findings particularly about relations between ICNC/CDNC and IWC/LWC in this study should be considered preliminary ones that initiate future work to streamline the development of the general parameterizations.

- Lines 197 to 203: I do not understand the logic, the authors refer to hydrometeors, then to aerosols, then back to aerosols. Can the authors rephrase the sentence?

The corresponding sentence is revised as follows:

(LL196-205 on p7)

Size distribution functions for each class of hydrometeors, which are classified into water drops, ice crystals (plate, columnar and branch types), snow aggregates, graupel and hail, are represented with 33 mass doubling bins, i.e., the mass of a particle m_k in the kth bin is determined as $m_k = 2m_{k-1}$. Each of hydrometeors has its own terminal velocity that varies with the hydrometeor mass and the sedimentation of hydrometeors is simulated using their terminal velocity.

Size distribution functions for aerosols, which act as cloud condensation nuclei (CCN) and ice-nucleating particles (INP), adopt the same mass doubling bins as for hydrometeors.

- Line 205: So aerosols do not sediment, is this true? Can the authors confirm that this hypothesis is close to reality?

The settling speed of aerosol particles in the accumulation mode, which is the most important mode among aerosol modes in terms of droplet or ice-crystal nucleation, ranges from 0.5 to 1 cm hr^{-1}, according to Seinfeld and Pandis (1997). This speed is negligible as compared to the terminal speed of droplets which is on average ~4-6 orders of magnitude greater than the settling speed of aerosols in the accumulation mode. Hence, we consider the settling speed of aerosols negligible as compared to the terminal speed of droplets, and based on this, we set the settling speed of aerosols at zero, while allowing droplets to fall down with its own terminal speed.

- Line 247: Since the altitude bins are not linear, would it be possible to have them in the supporting information? Otherwise it would be difficult to replicate the study.

The information is included in the supplement.

- Line 266 and line 281: Aerosol concentration is assumed to be constant with time and altitude in the PBL, can the authors confirm that this is an appropriate hypothesis?

The time-averaged observed aerosol concentration is imposed on the simulations as an initial aerosol concentration. During the simulation period, the imposed concentration of aerosols, including those in the PBL, is not fixed with respect to time and space but varies not only with time but also with the horizontal and vertical domains via processes as described in Section 2.1.

We do not have the observation of aerosol vertical distributions. Hence, we have no choice but to assume that aerosol number concentrations do not vary up to the PBL top and then above the PBL top, they reduce exponentially only at the first time step, based on the numerous previous observational studies (e.g., Gras, 1991; Jaenicke, 1993; Seinfeld and Pandis, 1998) that support this assumption. Since there is good consistency between modeling results and observation as described in Section 3.1.1, we believe that the assumption about aerosol concentrations is not that unreasonable.

The corresponding text is revised as follows by including related references and putting “at the frist time step” in the text:

(LL310-316 on p11)

This study takes an assumption that the interpolated CCN concentrations do not vary with height in a layer between the surface and the planetary boundary layer (PBL) top around 1 km in altitude at the first time step, following the previous studies such as Gras (1991), Jaenicke (1993) and Seinfeld and Pandis (1998). However, above the PBL top, they are assumed to decrease exponentially with height at the first time step, based on those previous studies, although the shape of size distribution and composition do not change with height.

To clarify the evolution of aerosol concentration, the following is added:

(LL308-310 on p11)

Note that although these parameters or the shape of aerosol size distribution does not vary, associated aerosol concentrations vary over the simulation domain and period via processes as described in Section 2.1.

- Line 291: The dust concentration is higher than the observed INP concentration, as is the INP concentration from the presentation. Can the authors state whether the INP concentration from the study is of the same order of magnitude as the dust events?

Yes, according to Hartmann et al. (2021), the INP concentration in this study is at the same order of magnitude as in the strong dust events. Based on this, the corresponding text is revised as follows:

(LL322-325 on p11)

However, Hartmann et al. (2021) observed the INP concentration that was at the same order of magnitude as assumed here in the Svalbard area when strong dust events occur, meaning that the assumed INP concentration is not that unrealistic.

- Line 385, is the temperature range from Choi et al. (2014) at the top of the cloud? Also, is the temperature range the same as the temperature range from the present study?

The temperature range in Choi et al. (2014) is based on the measurements of temperature throughout the cloud layer that includes the cloud top, cloud base and cloud parts between the cloud top and base.

The temperature range in Choi et al. (2014) is from 0 °C to -50 °C and this range includes the temperature range, which is between -16 and -33 °C, in the cloud layer in the case selected and simulated in this study. We picked up the supercooled cloud fraction (SCF) at the temperature between -16 and -33 °C from Choi et al. (2014) and the SCF picked up is shown in text.

- Line 462: The authors refer to the Lee et al. (2022) study for the mid-latitude case. Are the model and method similar and is CTH similar? I think the authors should describe this paper and the method used a bit more.

The simulated average CTH is 2.2 km in the midlatitude case and 3.3 km in the polar case. Both cases are simulated using an identical model, identical vertical resolutions, an identical source of reanalysis data for background conditions such as temperature, humidity and wind. However, the horizontal resolution is 100 m for the midlatitude case and 50 m for the polar case. To indicate this, the following is added:

(LL520-527 on p18)

In addition, identical model, model setup such as vertical resolutions, and source of reanalysis data are used between the 200_2 and control-midlatitude runs, although there are differences in environmental conditions (e.g., temperature), cloud macrophysical variables such as cloud-top height and horizontal resolutions between the runs. Here, while taking these similarities and differences into account, we hypothesize that the significant differences in ICNCavg/CDNCavg between runs are mainly due to the fact that ice nucleation strongly depends on air temperature (Prappucher and Klett, 1978).

Just want to say that there are similarities and differences in model setup (i.e., resolutions) and in CTH. However, this study focuses mostly on discrepancies in ICNC/CDNC and IWC/LWC, and through sensitivity tests, this study aims to understand how these discrepancies in ICNC/CDNC lead to those in IWC/LWC. The aim of this study is not in the examination of the cause of discrepancies in ICNC/CDNC between the cases, as detailed in our response to one of general comments above, although this study mentions temperature as a key factor causing the discrepancies in ICNC/CDNC. Factors like not only aerosol sources and advection as mentioned in "summary and conclusions" but also model setup and CTH can act as other factors than temperature causing the discrepancies in ICNC/CDNC. However, the examination of the cause of discrepancies in ICNC/CDNC between different cloud systems is out of scope of this study. We believe that this examination of the cause of discrepancies in ICNC/CDNC between different cloud systems requires significant research efforts, equivalent to those exerted on this paper, and can be done via additional future studies.

- Line 478-482. The much higher IWC than LWC in the polar case compared to midlatitude. The polar case is also associated with lower temperature, so I am not sure what more information can be derived from this.

As mentioned in our response above, while this study utilized temperature differentials to identify cases exhibiting significant disparities in ICNC/CDNC that control significant disparities in IWC/LWC, its primary objective lies in comprehending the inherent role of ICNC/CDNC variations themselves in the discrepancies observed in IWC/LWC across diverse cloud systems, regardless of the cause of those disparities in ICNC/CDNC. As mentioned in our response above, these disparities in ICNC/CDNC and IWC/LWC can be caused by other factors such as aerosol sources and advection than temperature. With the fulfillment of this objective, as long as we take interest in the role of ICNC/CDNC variations themselves among different cloud systems, the insights gleaned from this study regarding the influence of varying ICNC/CDNC on IWC/LWC can be extrapolated to scenarios where factors other than temperature differences contribute to discrepancies in ICNC/CDNC among different cloud systems.

- Line 495: The authors present new simulations here, which is confusing, why did they not present them in section 2.2.2?

Section 2.2.2 briefly introduces simulations, including new simulations pointed out by the reviewer here, to readers, so, they can get a glimpse of simulations performed for this study. Then, in the following sections, we describe and interpret results and based on this interpretation, we raise issues or hypothesis, for example, as in Section 3.1.3. To resolve the issues or hypothesis, we perform simulations and their results are described, for example, as in Section 3.1.4. We believe that it should be after presenting results and after finding issues or hypothesis to tackle from the results as in Section 3.1.3, we present simulations, tackling the issues or hypothesis, as in Section 3.1.4. In summary, readers can get a brief recognition of new simulations in Section 2.2.2, and then gain an understanding of why simulations are performed in Section 3.1.3 before getting to the simulations tacking the issues and hypothesis in Section 3.1.4. Hence, moving simulation results, as described in Section 3.1.4, to Section 2.2.2 removes chances for readers to understand why those simulations are performed and thus to better figure out implications of the results of those simulations. Thus, we believe that letting the presentation of the simulations pointed out by reviewer here stay in Section 3.1.4 is better for readers.

- The process associated with cloud formation and evolution does indeed depend on geographical regions. The Arctic is known to be prone to radiative cooling, whereas the mid-latitudes are prone to convective processes, so it is difficult to compare the two regions. Can the authors comment on this?

The simulations with and without radiative processes for the polar (or the Arctic) case in this study demonstrate that radiative cooling does not drive results from the polar case as in the midlatitude case. Hence, if we limit the driving force to two types, which are radiative cooling and convective processes as phrased by the reviewer here, there are no differences in the type of the driving force between the polar and midlatitude cases.

Anyway, this study aims to understand the substantial difference in IWP/LWP between the polar and midlatitude cases. For this understanding, we pick up and focus on ICNC/CDNC among many potential factors and examines its role in the difference in IWP/LWP between the cases. Yes, as mentioned in our responses above, there can be differences in other factors than in ICNC/CDNC which can explain the difference in IWP/LWP between different cloud systems. For the more fully established general principle, we need to carry out more studies to examine the roles of differences in other potential factors in the difference in IWP/LWP between different cloud systems in the future.

As mentioned in the manuscript, even attempts to identify a general factor to explain differences in IWP/LWP between different cloud systems have been rare and thus, findings in this study can be a valuable steppingstone to the fully established general principle, although this study deals with one factor, which is ICNC/CDNC.

- Line 597: 200_2_fac10, 100_2_fac10_CCN10, 200_2_fac10_INP10 represent mid-latitude cases. I thought the mid-latitude cases were from Lee et al. (2022) (line 159). Can the authors explicit that?

As mentioned in our responses above, this study focuses on only one factor, which is ICNC/CDNC, and tries to examine it as thoroughly as possible in terms of its role in the differentiation of IWP/LWP between the runs, as a steppingstone to the more general principle. Based on this, the 200_2_fac10 run in the old manuscript or 200_0.07 run in the new manuscript represents the mid-latitude case in Lee et al. (2022) or the control-midlatitude run in the sense that the 200_2_fac10 and control-midlatitude runs have similar ICNC/CDNC, although as mentioned in our responses above, there can be differences in factors, such as temperature, cloud-top height and model setup, between the 200_2_fac10 and control-midlatitude runs.

- Line 786: Should IFN be INP?

Corrected

First of all, we appreciate the reviewer's comments and suggestions. In response to them, we have made relevant revisions to the manuscript. Listed below are our answers and the changes made to the manuscript according to those comments and suggestions. Each comment of the reviewer (black) below is followed by our response (blue).

The authors have tried to make answers to my questions in the previous review. While some of them are adequately answered, others are not yet. Moreover, I would like to add some minor comments.

Major

1. The question in the previous review that points out the causality between the sedimentation and the total cloud mass is not properly answered. As sedimentation obviously acts as a sink term of cloud mass, the sentence "the droplet sedimentation tends to increase the total cloud mass in the 200_2 run" should be revised. In other words, to state the drop sedimentation as a cause of the increase in the total cloud mass, neither just mentioning some previous studies nor simply saying "it is not important" is not sufficient. The sentence should be revised like as: "Of the two runs, the drop sedimentation is greater in the case with the greater total cloud mass". I can find similar expressions at many points throughout the manuscript.
Related with this, I do not understand the phrase "changing sedimentation" in L527. Does changing mean increasing or decreasing? Also, I do not think that neither increasing nor decreasing of total cloud mass cannot be yielded by "changing sedimentation".

Following the comment here, sentences related to comments here are removed or revised. For details, see the revised text in Sections 3.1.2, 3.1.4 and 3.2.1.

2. By conducting additional experiments, the question that points out the importance of ICNC/CDNC is answered to some extent. However, it is still questionable whether ICNC/CDNC is indeed the critical factor. The experiments in the manuscript do not show any "intermediate" IWC/LWC values, saying 2–20 although ICNC/CDNC increases relatively gradually (Table 4). This means that the IWC/LWC shows an abrupt change, like a regime shift. I strongly recommend that the authors should clarify this in a relevant point of the manuscript.

To respond to this comment, the following is added:

(LL803-814 on p27)

The high-degree non-linearity in the variation of IWC/LWC is epitomized by the 1706 percent increase in IWC/LWC for the 163 percent increase in ICNCavg/CDNCavg from the 200_0.7 run to the 2000_2 run. This 1706 percent increase in IWC/LWC is induced by increases in both the initial number concentrations of CCN and INP between the runs (Table 1). In other transition from a simulation in a row to that in the next row in Table 4, there are decreases in both the initial number concentrations of CCN and INP, or there is either a change in the initial number condensation of CCN or INP. When either the initial concentration of CCN or INP changes in the transition, less than a 100% increase in IWC/LWC is shown. The decreases in both the initial number concentrations of CCN and INP, which are from the 2000_20 run to the 200_2 run, result in the decrease in IWC/LWC. Hence, depending on how the initial number concentrations of CCN and INP change, the magnitude and sign of the change in IWC/LWC can vary substantially.

3. I strongly suggest the authors to show more diverse types of analysis in the manuscript (not as supplementary). For example, the authors should add time-height plots of IWC or time series of IWP. For L585, the authors can add drop (ice) size distributions as the authors utilized a spectral bin microphysics model. For the description in the Section 3.1.1, the authors should add vertical profiles, time series, or time-height plots of RH with respect to liquid and ice.

The following is added:

(LL396-402 on p14)

To provide additional information of cloud development, Figure 5 shows the time evolution of the simulated and observed cloud-top and bottom heights, simulated and retrieved IWP and simulated and observed LWP together with the evolution of the simulated surface sensible and latent-heat fluxes; the simulated evolutions in Figure 5 are from the 200_2 run. This is based on the fact that the cloud-top and bottom heights, IWP and LWP are considered a good indicative of cloud development and the surface fluxes are considered important parameters controlling the overall development of clouds.

(LL508-513 on p17)

Figure 7 shows the time series of the averaged supersaturation over gird points where deposition occurs in the presence of both droplets and ice crystals in the 200_2 run. Figure 7 indicates that on average, supersaturation occurs for both droplets and ice crystals over those grid points. Hence, on average, the above-described situation of qv > qsw is applicable to deposition when droplets and ice crystals coexist in the 200_2 run.

(LL646-657 on p22)

In Figure 10a, we see that the number concentration of ice crystals with diameters smaller and larger than ~40 micron increases and decreases, respectively, as we move from the 200_2 run to the 200_20 run, which indicate a shift of the sizes of ice crystals to smaller ones. From the 200_2 run to the 200_20 run, the sedimentation of droplets at the cloud base decreases as shown in Table 2, mainly due to decreases in LWC. Figure 10b shows that the number concentration of drops decreases throughout almost all parts of the size range from the 200_2 run to the 200_20 run, which indicates a negligible shift in the drop size but a reduction in LWC. It is found that changes in the average rates of the droplet and ice-crystal sedimentation over the cloud base and simulation period are ~three to four orders of magnitude smaller than those in the average integrated condensation and deposition rates between the 200_2 and 200_20 runs (Table 2).

Minor

1. In the manuscript, the authors should specify the model integration time and the period where the average is calculated. Note that the first a few hours of simulation are generally excluded from calculating average as they are regarded as spin-up in a typical idealized cloud simulation.

Simulations in this study are not idealized simulations for clouds, but real-case simulations which generally do not use the spin-up time, and this study does not adopt the spin-up period.

The model integration time is indicated as follows:

(LL254-256 on p9)

The simulation of the observed system or case, i.e., the control run, is performed three-dimensionally over the red rectangle and the period between 02:00 and 10:00 LST on March 29th, 2017.

(LL294-296 on p10)

Note that the average of a variable with respect to time in the rest of this paper is performed over this period between 02:00 and 10:00 LST, unless otherwise stated.

2. The authors should describe about the model in more detail. For example, does the model include the source of aerosol concentration other than turbulent mixing and advection, such as regeneration by drop evaporation or surface emission? Otherwise, how can the aerosol concentration be maintained throughout the model integration? In addition, whether the model consider freezing of activated drops should be described, considering the experimental condition. Moreover, the way large-scale subsidence (L254) is considered in the model should also be described.

Aerosol regeneration is considered in the model. To indicate this, the following is added:

(LL205-209 on p7-8)

The evolution of aerosol size distribution and associated aerosol concentrations at each grid point is controlled by aerosol sinks and sources such as aerosol advection, turbulent mixing, activation and aerosol regeneration via the evaporation of droplets and the sublimation of ice crystals. Aerosol regeneration follows the method similar to that as described in Xue et al. (2010).

Yes, the model adopted in this study considers not only the homogeneous freezing of drops but the heterogeneous freezing of drops via contact, immersion and condensation-freezing pathways, as already described in text as follows:

(LL224-230 on p8)

To represent heterogeneous ice-crystal nucleation, the parameterizations by Lohmann and Diehl (2006) and Möhler et al. (2006) are used. In these parameterizations, contact, immersion, condensation-freezing, and deposition nucleation paths are all considered by taking into account the size distribution of INP, temperature and supersaturation. Homogeneous aerosol (or haze particle) and droplet freezing is also considered following the theory developed by Koop et al. (2000).

The large-scale subsidence in the reanalysis data is imposed on the model to consider the large-scale subsidence in the simulations. This imposition of large-scale subsidence is similar to the well-known imposition of the field of horizontal winds in the reanalysis data on the model in the sense that both the large-scale subsidence and the field of horizontal winds are imposed on the model as “background wind fields”. Similar to the situation where the imposed horizontal winds affect those generated by processes such as cloud processes, the imposed background large-scale subsidence suppresses updrafts and intensifies downdrafts generated by those processes. To clarify this, the following is added:

(LL268-270 on p9-10)

This large-scale subsidence is imposed on the control run as a part of background wind fields and interacts with updrafts and downdrafts generated by relatively small-scale processes including those associated with clouds.

3. L424 and others: How is the entrainment rate defined? Why does it have a unit of cm s−1? The entrainment rate has the unit of s−1 in general.

The entrainment rate can be approximated by the difference between the cloud top growth rate (or the rate of increase in cloud-top height) in cm s^{-1} and the large-scale vertical velocity (or large-scale subsidence) in cm s^{-1}, following studies such as Moeng et al. (1999), Jiang et al. (2002), Stevens et al. (2003a and 2003b) and Ackerman et al. (2004).

The following is added:

(LL468-471 on p16)

Here, entrainment rate is defined to be the difference between the rate of increase in cloud-top height and the large-scale subsidence, following Moeng et al. (1999), Jiang et al. (2002), Stevens et al. (2003a and 2003b) and Ackerman et al. (2004).

4. L621: All of the studies mentioned consider drop autoconversion, not ice autoconversion. Therefore, it is not proper to presume that ice autoconversion is roughly proportional to ice concentration based on these studies.

To support ice autoconversion, proportional to ice concentration, Murakami (1990) and Morrison et al. (2005, 2009, 2012) are added.

5. Is stating INP meaningful in “noice” experiments? I suggest that the authors replace “200_2_noice” with ”200_0” and “2000_2_noice” with “2000_0”. In the same way, I suggest to replace

“200_2_fac10” with “200_0.07” (also the others including “_fac10”) as the names are neither intuitive nor consistent with others.

The simulation names are changed following the comment here.

6. As the descriptions about the “norad” experiments are very simple, I suggest not to include them in Table 1 for simplicity.

Done.

7. The authors stated that the reanalysis has a temporal resolution of 6 hours (L248). However, in another line, they stated that Figure 2c shows the time series of surface temperature in the reanalysis (L257), which varies with a temporal resolution of 1 hour.

We find errors in a plotting program constructing the evolution of the surface temperature in Figure 2c. A corrected figure using the corrected program is put into the manuscript.

8. I strongly suggest the authors to split the L343–391 and to make them as another subsection, named “model validation”. Related with this, if there is no other problem, I suggest the authors to include Fig. S1 in the manuscript.

Done.

9. L214: the most widely used -> a

Corrected.

10. L303: IFN -> INP

Corrected.

11. L398, 400: The order of IWC and LWC should be reversed.

Corrected.

12. L405: this -> that, L406: this -> the

Corrected.