Thank you to the reviewer for these comments. In response to comments from both reviewers, we decided to make major changes to our simulations. The surface temperature has been increased to 273.15K, a value that is frequently measured in the transition seasons over ice in the Arctic. Solar radiation is now turned off, as suggested by Reviewer #2, in order to simplify the setup and remove the need to specify a date associated with the simulations. Ice processes remain on, but because these simulated clouds are only slightly supercooled, the ice is unimportant and is now completely neglected in the presentation of the results. A small suite of sensitivity tests is presented in which the initial thermodynamics conditions and the subsidence rate are modified. Among these are simulations that are very similar to our original simulations that used a surface temperature of 263 K. We do not intend for these sensitivity simulations to be a complete investigation of the sensitivity of our results to these choices, but they do serve to illustrate possible alternative outcomes.

This study uses LES to simulate idealized mixed-phase clouds in a relatively clean Arctic boundary layer environment. Sensitivity tests are conducted to explore the impact of higher tropospheric aerosol concentrations to cloud properties. Aerosols in the boundary layer is kept 20 per mg while it is increased up to 50x in the free troposphere. Results show that entrained aerosol from the free troposphere can suppress precipitation and help to sustain clouds for longer time. I think the simulation results are clear and make sense to me. My major concern is how those simulations are relevant to real clouds in the atmosphere.

I’m skeptical about the relatively high droplet number concentration at the cloud top from the simulations (Figure 6). Are those simulations realistic? Therefore, I read some MOSAiC-related papers cited in the manuscript to learn more about the observations. I might miss some other papers, but I do not find observational papers to support model setups and conclusions in this study. I recommend the authors to add more observations to justify their model setup and/or conclusions. Some specific comments are listed below:

1. Figure 1 is the only figure related to the observation. I have several questions: do all those days have clouds? Where is the cloud layer on each day? What about the profile of large aerosol particles (those can contribute to droplet formation), instead of all aerosols larger than 12 nm? I recommend the authors plot similar figures like Figures 6, 13, 14 in Lonardi et al. (2022), for all cases chosen in this study. The model setup and conclusions would be more convincing if the authors can show observational evidence of the existence of aerosol layers above the cloud layer, and/or the potential impact of aerosols in the free troposphere to clouds in the boundary layer.

Yes, as best we can tell, all of these days have clouds, but we don't believe that all of the data was taken in cloudy conditions – we suspect that in some cases the data may have been taken during a clear period. Regardless, we have updated Figure 1 to include the N12 and N150 concentrations as well as an estimate of the location of cloudy layers. Estimating the cloudy layers was difficult to do for most flights because in situ cloud data are typically not measured. Estimates come primarily from broadband longwave fluxes, in some cases in combination with RH data or the VIPS cloud flag (only available for two flights analyzed here). The broadband fluxes and VIPS data are typically not coincident with the aerosol data but rather typically come from 1-3 hours earlier since not all instrumentation could be flown together on the tethered balloons.

We have moved the discussion of these observations to a new section (#2) and provided more information about the data sources and more discussion of the data as it relates to this study. While we have not exactly reproduced Figures 6, 13, and 14 of Lonardi, we have added new panels d-f to briefly analyze the size distribution of the particles with respect to the mixed layer tops. Here is the revised Figure 1:

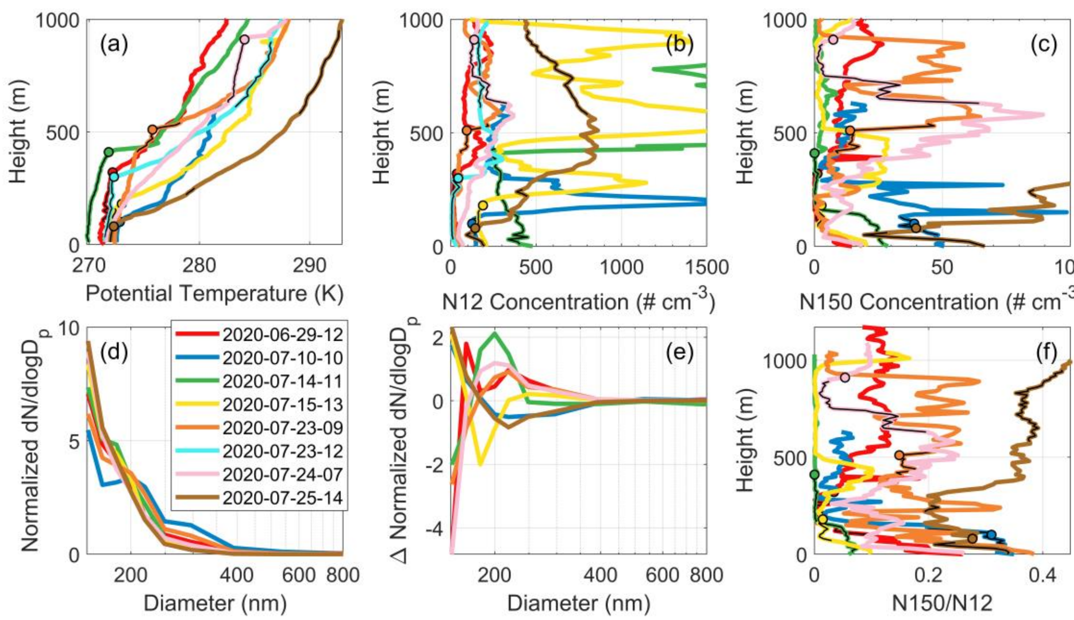

Figure 1. Profiles of (a) potential temperature, (b) concentration of particles with diameter >12 nm (N12), (c) concentration of particles with diameter >150 nm (N150), and (f) the ratio of N150 to N12 for select tethered balloon flights during the MOSAiC campaign. Black outlined circles represent the top of the mixed layer for each profile. Thin black lines indicate the most likely location of a cloud layer. (d) Normalized distributions averaged over 100m above the mixed layer top and (e) the normalized size distribution averaged over 100 m above the mixed layer top minus the normalized size distribution averaged 100 m below the mixed layer top.

2. Based on Lonardi et al. (2022) Figure 6, clouds on July 23 and July 24 are all above the top of the boundary layer. Therefore, I think Figure 1 might be misinterpreted by the readers that clouds are in the boundary layer and they are affected by aerosols above during the MOSAiC campaign.

   Yes, the clouds are in a mixed layer that is decoupled from the surface. We have clarified the wording and now explicitly included an estimate of where the clouds are in Figure 1.

3. The authors said that "Here we extend the analysis presented by Lonardi et al. (2022)..." However, based on Lonardi et al. (2022) Figure 6, the large temperature inversion on July 23 and July 24 are at about 600 m and 900 m, respectively. However, in Figure 1 of this study, the top of the boundary layer on these two days are at about 300 m and 100 m. Please explain why they are so different.

   For July 24, this was our error – we had correctly identified the large temperature inversion, but that height was modified in a transcription error that we did not catch. It is corrected now. For July 23, the difference arises because there are two datasets for July 23, and we used the second dataset, not the first which is shown in Lonardi et al. We had unconsciously excluded the first because the meteorology data file is formatted differently which led to a problematic read of the data, but one that did not throw errors in our script, only produced NaNs. This flight is now included. We do note though that our estimate of the cloud layer is different from that shown in Lonardi et al 2022. The cloud layers are our own best estimate given that the Lonardi et al. 2022 methods are not documented.

4. Figure 2. Are the initial profiles of potential temperature and relative humidity based on the observation durign the MOSAiC campaign? Based on Lonardi et al. (2022) and also Figure 1 in this paper, surface temperature is very close to 0 C and the cloud temperature is just slightly below 0 C. But the initial temerapture profile for simulations in this study is much lower. Please justify the model setup.

   No, the profiles are not based on the data from Lonardi et al. (2022). Rather, we used idealized profiles for initialization. These profiles have been modified from the originals in the revised submission. More justification is provided in the main text.

5. Please provide formules of the profiles such that simulations can be rerun by others. What about the initial wind profiles? Do you nudge those profiles?

$$\theta(z) = \begin{cases} \theta_0, & z \leq 700\text{m} \\ \theta_0 + a(z-700), & 700\text{m} < z \leq 800\text{m} \\ \theta_0 + 100a + 0.005(z-800), & z > 800\text{m} \end{cases}$$

$$w(z) = \begin{cases} w_0, & z \leq 700m \\ w_0 + \frac{0.75w_s(800) - w_0}{100}(z-700), & 700\text{m} < z \leq 800\text{m} \\ \frac{0.75}{2} w_s(z)\left(\exp\left(-\frac{z-800}{200}\right)+1\right), & z > 800\text{m} \end{cases}$$

In the base set of simulations, θ_0 = 273.15K, a = 0.06 K/m, and w_0 is set by the mixing ratio that gives 100% relative humidity at cloud base. The excess water vapor in the cloud layer is converted to liquid water by RAMS at the associated latent heat is added to the temperature profile. This is perhaps an unusual way to initialize with a cloud layer, but the model succeeds in producing typical looking boundary layer profiles quickly. In the reduced temperature simulations, θ_0 = 263.15K. In the weakened inversion simulations, a = 0.03 K/m. In the stable layer simulations, we modify the equations for the boundary layer:

$$\theta(z) = \begin{cases} \theta_0 + 0.015z, & z \leq 500\text{m} \\ \theta_0 + 7.5, & 500 < z \leq 700 \\ \theta_0 + 7.5 + a(z-700), & 700\text{m} < z \leq 800\text{m} \\ \theta_0 + 7.5 + 100a + 0.005(z-800), & z > 800\text{m} \end{cases}$$

$$w(z) = \begin{cases} w_0 - 4\text{x}10^{-6}(700-z), & z \leq 500m \\ w_0, & 500\text{m} < z \leq 700\text{m} \\ w_0 + \frac{0.75w_s(800) - w_0}{100}(z-700), & 700\text{m} < z \leq 800\text{m} \\ \frac{0.75}{2} w_s(z)\left(\exp\left(-\frac{z-800}{200}\right)+1\right), & z > 800\text{m} \end{cases}$$

where the parameters are defined in the same way as for the base simulations. The water vapor gradient in the stable layer was chosen to give a relative humidity profile that is similar to that of the other simulations.

Winds are set to zero. None of the profiles are nudged.

All of the profiles used to initialize the RAMS simulations are specified in files beginning with "SOUND_IN". These files are located in the code repository at https://doi.org/10.5281/zenodo.7991354.

6. Figure 6. Is there any observational evidence to show that cloud droplet number concentration is maximum near the cloud top?

We are not aware of any observations of coincident aerosol and droplet concentration measurements in high Arctic clouds which could be used to compare with our simulations. However, for reasons that we did not investigate, the enhancement in droplet concentration near cloud top is much reduced in our new simulations, and we suspect that it would be difficult to detect in aircraft-based observations.

7. Page 4, Line 102, what is the CCN size distribution in the model? Is it fitted based on observations? I think results are sensitive to the CCN distribution. If the authors do not test its sensitivity, it should be clearly stated.

We do not test the sensitivity to the CCN distribution or composition. The sea salt aerosol is lognormally distributed with a modal diameter of 200 nm and a standard deviation of 1.5.

8. Entrainment rate is critical to bring aerosols from the free troposphere to the boundary layer. It would be nice to plot the time series of entrainment rate for different cases.

We at one point did have a time series of the entrainment rate, but felt that it didn't add anything beyond what is shown with the BL top time series. The entrainment rate can be inferred from this time series.

9. Page 9, Line 180: "This is indicative of a decrease in the amount of aerosol being entrained into the cloud…" Do you know the amount of aerosols entrained from the free troposphere as a function of time?

No, we do not know the aerosol entrainment rate. We have modified this statement to read "In each simulation, cloud number concentrations are decreasing and mean radii are increasing in time. This is indicative of either a decrease in the availability of CCN in the boundary layer and/or a decrease in the amount of aerosol being entrained into the cloud."

10. Page 10, Line 215: "Figure 7b shows domain-average time series of \sigma_w^2". People usually calculate \sigma_w in the boundary layer where turbulence is vigorous. Just want to make sure \sigma_w is averaged in the whole domain or just in the boundary layer? It makes more sense to only average in the boundary layer. It would be better to show the \sigma_w profile.

We've taken your suggestion and replaced the time series with the average vertical profile at the end of the simulations.

Other comments:

1) Page 7, Line 127: delete “is”

   The sentence was grammatically correct with the double ‘is’ but was certainly awkward. We have rephrased the sentence.

2) Page 7, Line 130; Page 11 Line 219: “not shown” is not acceptable. Please consider adding it in the main text or supplement.

   With the new simulations, ice is even less important than before. We have removed this statement, but note that we no longer show any results regarding ice.

3) Page 12, Line 230: “Mauritsen et al. (2011) also found that...” This sentence is not clear to me. Please check.

   This sentence has been removed.

4) Page 12, Line 246: “In these simulations, the boundary layer is ...” This sentence is not clear to me. Please check.

   The sentence now reads: “In these simulations, the boundary layer is collapsing which leaves behind a layer of air with aerosol concentrations that are much lower than those is the rest of the free troposphere.”