

We thank both reviewers for their thoughtful and constructive comments. Revising the paper to address their concerns has significantly improved the manuscript. We have addressed the concerns expressed in the reviews as completely as possible in the point-by-point responses and in the revised paper. The reviewers' comments are in black font and our responses are below each comment in blue font.

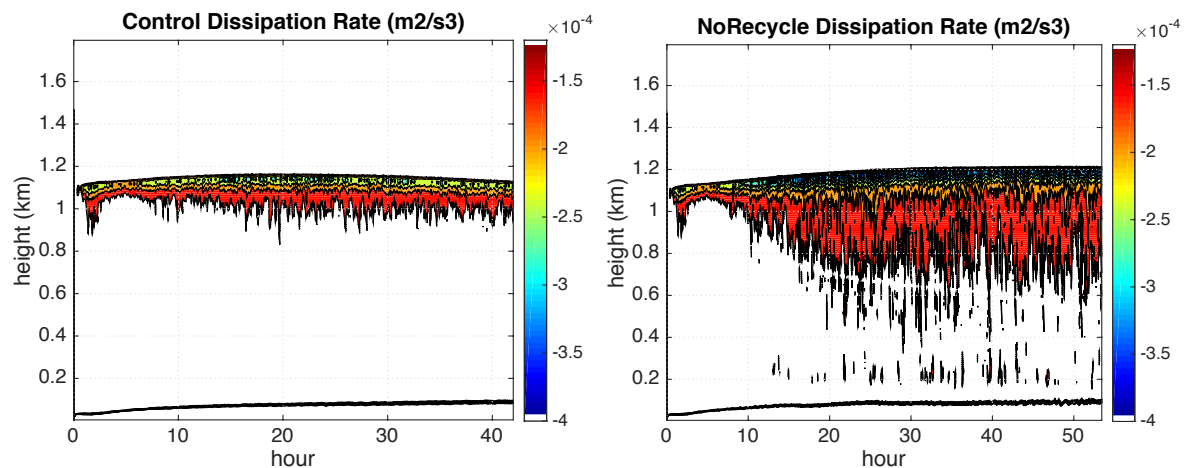
## Reviewer #1

### 1 General Comments

This study investigates the persistence and liquid-ice phase partitioning of mixed-phase stratocumulus clouds typical of the Arctic. The authors suggest that subcloud ice crystal sublimation produces ice nuclei that recycle back into the cloud layer to reactivate ice particles. The recycling of ice nuclei act to maintain the ice water content over a longer time-scale than without recycling, and with the combination of cloud-top radiative cooling, both liquid and ice contents can be steadily maintained. The authors also imply a diurnal impact on the maintenance of mixed-phase stratocumulus in that both liquid and ice productions are weakened in the presence of shortwave radiation, which in turn reduces ice precipitation fluxes out of the layer, and hence further prolonging the lifetime of the system.

Previous studies on the maintenance of mixed-phase stratocumuli involve the discussion of the rapid glaciation and dissipation of these clouds due to efficient ice depositional growth via the evaporation of the liquid content, usually at higher ice concentrations. Because ice nuclei recycling effectively maintains a consistent ice concentration, it would be curious to see what role recycling would play in liquid/cloud dissipation rates.

The dissipation rates are proportional to the TKE in each run. Here are figures of the dissipation rates for Control and NoRecycle:



Maximum TKE in NoRecycle is  $\sim 28\%$  larger than Control because of the larger LWP which is consistent with the  $\sim 32\%$  increase in the maximum dissipation rate.

Furthermore, figures 7a and 10a indicate that, over time, the liquid water content achieves higher values when the diurnal cycle is consistent, in contrast to the ice water content, which

drops to lower values. This result is not discussed, but one would imagine that the diurnal cycle would also help to maintain a mixed-phase cloud that would otherwise dissipate.

On page 11742 line 9 we have added, "...and promotes the persistence of a mixed-phase cloud system."

The work presented is very interesting and compelling. The recycling technique appears to compare well with previous work that employ relaxation ice concentration methods for simpler studies, and so perhaps the recycling effect could be considered a motivation for simulations with assumed constant ice concentrations. The manuscript is well written, concise, and organized thoughtfully. The manuscript content and figures, however, are very compact and complex, so I would urge the authors to consider simplifying and/or shortening sentences throughout for ease of reading. I would also encourage the authors to simplify figures so that they are easier to interpret.

Thank you, we have gone through the manuscript to identify sentences that can be simplified and shortened.

With the advice given above and the suggestions listed below, my recommendation for this manuscript is *accept for publication with major revisions*.

## 2 Specific Comments

1. Page 11729, Lines 4-7, "However, unlike subtropical...at cloud top": Which process are considered to dissipate subtropical mixed-phase clouds that do not occur in the Arctic? One would imagine that subtropical clouds could also be supplied with moist air at cloud top. Are AMPS unique from all other mixed-phase stratocumuli in other regions (e.g., midlatitudes)? If so, is there a specific quality (e.g., temperature, solar zenith) in AMPS to contribute to these differences?

This sentence has been changed to read, "However, unlike subtropical cloud-topped boundary layers where decoupling enhances cloud breakup by cutting the cloud system off from the surface source of moisture, decoupled AMPS can persist for extended periods of time due to weak precipitation fluxes out of the mixed layer and relatively moist air entrained into the cloud layer at cloud top.

2. Page 11730, Line 22, "We posit that recycling...": The term "recycling" has been used many times thus far, but has never been defined or conceptually explained.

A clarifying sentence has been added on page 11730 at line 12, "This feedback loop is referred to hereon as "recycling".

3. Page 11730, Line 27, "...while AMPS...": Perhaps change to "...while persistent AMPS". AMPS are not necessarily always persistent, so please be sure to differentiate throughout.

Sentence changed as suggested.

4. Page 11731, Line 2: Please indicate whether recycling is turned off or on for the Control simulation as it is never indicated.

Lines 3-4 have been changed to, "...with recycling turned on and shortwave radiation turned off (to compare with previous simulations of this case that use different IN formulations and shortwave radiation turned off)..."

5. Page 11735, Line 2: Are the IN that are produced via sublimation always recycled back into the layer? Is it possible that some IN become "inactive" after sublimation?

Yes, recycling just means IN become available for activation but the extent to which they become active depends on large-scale circulation, mixing by turbulent eddies, and the in-situ temperature and water vapor. The sentence has been left unchanged.

6. Page 11735, Lines 8-13: Interpretation of figure 2 is unclear. Do you only consider activation at these threshold temperatures, or would a concentration of 1.3-1.5/L nucleate at -20 versus 0.75/L at 15°C? What is unique about these temperatures that make them "threshold"?

"used to initialize IN number concentration in each bin" has been added to the Figure 2 caption. After initialization prognostic equations for each bin that include advection, turbulent mixing, and sources and sinks are used to calculate the evolution of IN in each bin. The threshold temperatures are a representation of the dependence of nucleation on surface characteristics and size. The nucleation scheme used in this study approximates this relationship with the number of IN available as a function of a threshold temperature (given the discrete bins used in this study) based on the empirical study of DeMott et al. (2010). Threshold temperatures are only needed because we are discretizing the continuous relationship shown in Figure (2).

7. Page 11735, Line 22: What is the importance of the "modification of activation thresholds" and why is this consideration unnecessary for this work?

The sensitivity of ice nucleation to the preactivation of IN is an interesting topic but outside the scope of this study since it is so poorly defined by measurements at this time. This will be included in future studies if laboratory studies can quantify the impact of preactivation on ice nucleation.

8. Page 11736, Lines 13-14: "Crystal size...Fig. 5": Why would the maximum ice size (5 mm) be larger than the maximum snow size (0.7 mm) in Figure 5? What are the shape or aspect ratios and densities considered for snow and ice? What are the physical processes considered for snow and ice?

“ICE” and “SNOW” need to be reversed in the legend in Figure 5. This has been corrected. In our model setup, the particles are assumed to be spherical and  $d$  is set equal to 3. The bulk density of ice is assumed to be  $0.5 \text{ g cm}^{-3}$  and the bulk density of snow is assumed to be  $0.1 \text{ g cm}^{-3}$ . Beside the nucleation parameterization described in the paper, ice and snow mass can change due to riming, condensation/deposition, and autoconversion.

9. Page 11736, Line 24: To interpret figure 6, it would help to mention that IN are “lost” to activation of ice crystals.

The sentence has been changed to read, “IN are produced by sublimation of ice crystals below the cloud layer, advected to the cloud layer by turbulence, and removed from the population when activated as ice crystals”

10. Page 11737, Lines 15-23, “Over the...subcloud layer”: If the cloud was coupled, could you expect different results since rather than the turbulent eddies sweeping the IN back into the cloud, the IN could sediment to lower levels and not be recycled.

Yes, if the cloud becomes coupled to the surface layer then eddies can mix IN in layers from the surface to the cloud base into the cloud layer and the IN reservoir at the base of the mixed layer is quickly activated. This causes more IN to be lost from the system through increased precipitation.

11. Page 11737, Lines 23-25, “The continuous...mixed-layer base.” This statement is unclear. Is this statement suggesting a “residence time” effect in that IN are advected into the cloud layer out of the subcloud layer more quickly than new IN produced via crystal sublimation?

Yes, this is correct. There is a continual depletion of IN from below the cloud layer while the ice number concentration decreases more slowly. This is seen clearly in Figures 8b and 10e where sublimation is less than the IN flux at cloud base. The sentence has been left unchanged.

12. Figure 7b: Perhaps this is already explained, but why does the temperature warm more for recycling? Perhaps recycling induces activation which increase the release of latent heat? This should be discussed.

The temperature is colder for the NoRecycling case because there is more liquid in the cloud and therefore stronger cloud top cooling, which is mixed downward by cloud-driven turbulent mixing. Differences in latent heat release are not as significant as differences in radiative cooling at cloud top (not shown). A sentence has been added on page 11738 line 13, “Increased cloud liquid water when recycling is turned off results in increased radiative cooling at cloud top, which causes the cloud-driven mixed layer to cool more rapidly (Figure 7b).”

13. Figure 7d: Please explain the units  $\text{m L}$  (meters/liter?).

In Figure 7 caption “(meters/liter)” added.

14. Figure 10c (number in column): The feedback loop in figure 9 is not apparent in figure 10c as one would expect oscillations in  $N_{NI}$  to correspond with those in  $N_{IN}$ .

This would only be the case if precipitation, turbulent mixing and entrainment are negligible, which is not the case for this simulation. The schematic has been left unchanged.

### 3 Technical Corrections

15. Page 11729, Line 10: “radiatively-important” should be “radiatively important”.

Changed as suggested.

16. Page 11729, Line 26: Please replace the semi-colon (;) with either a colon (:) or a comma (,).

Changed as suggested.

17. Page 11729, Line 28: Please remove the first “or”.

Changed as suggested.

18. Page 11730, Line 11: Both “large-eddy” and “large eddy” have been used.

“large-eddy” has been replaced with “large eddy”.

19. Page 11730, Line 13: While it may be obvious to most readers, please consider expanding D.O.E.

Changed as suggested.

20. Page 11733, Line 7: “horizontal resolution” should be “horizontal resolutions”

Changed as suggested.

21. Page 11733, Line 23: Please replace “amplitude” with “amplitudes”.

Changed as suggested.

22. Page 11733, Line 28: Please replace “...where the slope of liquid-ice static energy exceeds...” with “...where the slopes of liquid-ice static energy exceed...”

Changed as suggested.

23. Page 11734, Line 28: Water vapor mixing ratio has already been defined.

Second definition removed.

24. Page 11739, Line 13: “Figure” should be “Figures”.

Changed as suggested.

25. Page 11741, Line 8: “control” to indicate the control case is sometimes capitalized and other times not.

“Control” is used uniformly throughout the paper.

26. Page 11742, Please consider absorbing the first paragraph into the second as the first paragraph contains only one sentence.

Changed as suggested.

27. Figure 1: There is an overlap in the x-axes of the two plots. Also, the caption indicates “grey shading” that does not appear in the figure.

The figure has been corrected.

28. Figure 3: Please add a legend.

A legend has been added.

29. Figure 4: This caption is very difficult to follow. Perhaps consider removing the first three sentences as that information is contained in the image. Also, please add the “control” lines to Figs. 4B and D and legends to B-D.

Thank you for your comment but we would like to keep the first three lines in the caption to identify the fields plotted in each figure. The wording of the caption has been simplified to clearly specify the fields plotted in each panel. Control LWP and IWP have been added to 4B and 4D. Legends are now in all four panels.

30. Figure 6 and throughout: Please consider relabeling the number of ice crystals to something like  $N_i$  as  $N_{IN}$  and  $N_{NI}$  are very easily confused.

“ $N_{NI}$ ” and “ $T_{NI}$ ” have been changed to “ $N_{ICE}$ ” and “ $T_{ICE}$ ” to clearly distinguish from “ $N_{IN}$ ” and “ $T_{IN}$ ”.

31. Figures 7a and b: Are IWP and temperature calculated for just within the cloud, within the mixed layer, or for the entire domain.

IWP and minimum temperature are calculated for the entire column. The caption has been changed to read, “Minimum horizontally-averaged temperature in the column, in units of °C.”

32. Figures 7, 8, and 10 are missing plot labels as referenced in the captions (e.g., a, b, c, d) and x-axes.

Labels and axes added.

33. Figure 10: “CB”, “CL”, and “ML” should be defined in the caption. Also, what do the grey shaded columns indicate?

All abbreviations and shading are now defined in the figure caption.

## Reviewer #2

This manuscript investigates the role of IN recycling from the sub-cloud layer on the persistence of mixed-phase conditions in springtime Arctic stratiform clouds. The study is based on long Large Eddy Simulations of a single mixed-phase stratiform cloud case using prognostic IN concentrations. Several aspects of the problem that deserved more attention from the modeling community are addressed in this work. For example, the role of IN recycling has been isolated here from other potential sources of ice forming particles. In addition, the persistence of mixed-phase clouds has been investigated for rather long periods of time, up to 3 full diurnal cycles (including short-wave solar radiation), when most other LES studies rarely exceed 12-24h. The results and methods presented are thus particularly relevant for the field, and give new insights into the processes maintaining long-lived Arctic mixed-phase stratiform clouds.

I would therefore strongly recommend the publication of this article in ACP. Several issues need however to be addressed in order to improve the overall clarity of the manuscript.

### Major comments:

First of all, a more detailed description of the prognostic IN method is required to fully understand the modeling approach and help interpret some of the results. If I understood correctly, there exists two requirements for IN particles within a given bin to activate: 1) the in-situ temperature must exceed the threshold temperature assigned to the bin, and 2) local conditions must be at or exceed water saturation. But how is the nucleation tendency calculated (activation term in equation 1)? I guess that the number of particles within bin  $k$  is initially given by the number of IN calculated by equation 2 at the threshold temperature  $k + 1$  minus that calculated at temperature  $k$ . But are all the particles (or 50% of them) within bin  $k$  activated instantaneously as soon as the threshold temperature  $k$  is exceeded (at or above water saturation)? In other words, is ice nucleation considered to be an instantaneous process or is it allowed to vary smoothly in time? This may be of importance as IN recycling involves interactions between sublimation, droplet activation and freezing, with droplet activation often considered to occur instantaneously, and ice nucleation being a much slower, and thus limiting, mechanism.

Yes, this is correct. IN number concentrations are initially specified using equation 2, such that the initial IN in bin  $k$  is equal to the number of IN calculated by equation 2 at the threshold temperature  $k + 1$  minus that calculated at temperature  $k$ . After the initial time 50% of the IN available in a bin nucleates if the in-situ temperature is above the threshold temperature and the local conditions exceed water saturation. This approach is motivated by the study of Ervens and Feingold (2013; doi:10.1002/grl.50580), where it was shown that, for Classical Nucleation Theory, nucleation rates are far more sensitive to temperature ( $T$ ) than to time. If we consider the same nucleation rate, an uncertainty of  $\Delta T = \pm 0.2$  K translates to the same uncertainty as differences in time of about two orders of magnitude.

Regarding observations of time dependent nucleation as in Westbrook and Illingworth: Ervens and Feingold (2013) note that "Based on our analysis, we estimate that differences of



five orders of magnitude in  $t$  (10s vs. 1day) are equivalent to a change of  $\Delta T \sim 2K$  (at  $T \sim 258K$ , i.e., the approximate  $T$  of the observed clouds)." This happens to be the same  $T$  as our Arctic case. So observations that suggest a time dependence might actually be masking small(ish)  $T$  fluctuations that are hard to measure at high spatial temporal resolution. However, temporal aspects of nucleation may play an interesting secondary role and is currently an active area of research. These temporal aspects need to be investigated in a Lagrangian model since in an Eulerian model temporal information is not available. Therefore, within the model framework of an Eulerian bin model we can represent IN distributions as a function of temperature but not time.

Also, is the  $F$  factor used in equation 2 applied at each time-step, i.e. is the number of activated IN after each time-step equal to 50% of the unactivated particles within the bin at the beginning of the time-step?

Yes, this is correct. As discussed above, the factor  $F$  is applied at every time step such that 50% of the unactivated IN nucleate if the in-situ environment is saturated with respect to water and the in-situ temperature is above the threshold temperature for that bin.

Finally, can you please comment on the relevance of omitting the dependence of IN nucleation on water and ice saturation? In particular, assuming that IN particles are available for nucleation as soon as water saturation is reached suggests that the particles acting as IN, whatever they are, activate as cloud droplets at water saturation, regardless of their size and composition. Should we expect different conclusions on the role of IN recycling if activation and freezing were assumed to be size and composition dependent (and thus more sensitive to water saturation)?

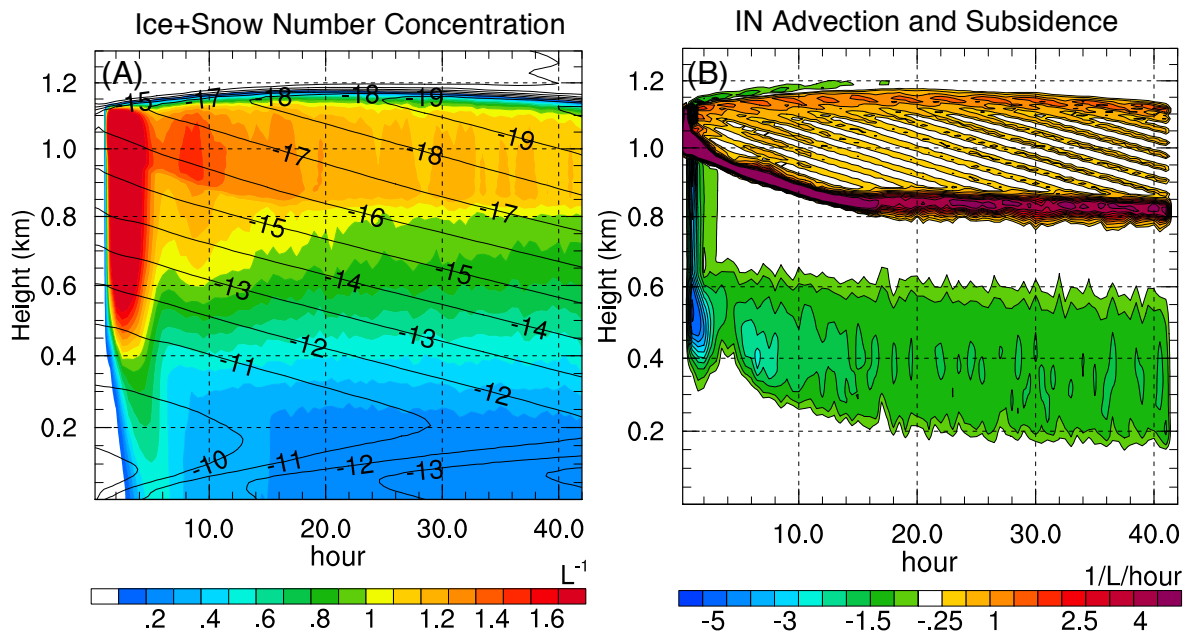
Yes, we are only including ice nucleation where drops are a necessary precondition. We are actually including the dependence of IN nucleation on water and ice saturation by using an empirical relationship between IN and temperature to specify the initial IN number concentrations in each bin. The number of IN available for nucleation in each bin is an implicit function of temperature, composition, and size. This is the simplest parameterization for IN that doesn't include aspects of nucleation that the model cannot resolve.

The results section is very concise, with very clear figures, and gives the essential information needed to understand the outcome of the study. The analysis of IN and NI fluxes through cloud base provides interesting insights on the mechanisms involved in the maintenance of IN concentrations sufficient to sustain mixed-phase cloud conditions. However, several points deserve clarifications. First of all, Figure 6 shows that ice production and IN activation are larger around cloud base. This result seems to contradict former modeling studies showing that ice formation primarily occurs at cloud top, where the temperature is lower. A short discussion of the vertical distribution of IN activation, which peaks at cloud base, may provide interesting information.

A figure has been added of the time evolution of the horizontally-averaged ice+snow number concentration and IN advection+subsidence (shown below in Figure S1). This figure shows that the majority of IN activates at cloud base, which is a bit warmer than cloud top but is

sufficiently cold to activate many of the IN. However, IN from bins with colder threshold temperatures are advected higher into the cloud where they activate at their threshold temperature. A secondary maximum is seen at cloud top where the coldest temperatures are found. Also, it is seen that IN are advected into the cloud layer at cloud top for the first 15-18 hours, but this source of IN decreases as IN in the upper entrainment zone are depleted.

Regarding how this fits with other model studies, the location of particle nucleation is based on how IN processes are specified in each model. If nucleation is simply diagnosed based on temperature, then it will occur more towards cloud top. The use of a prognostic equation to describe IN evolution in this study causes there to be a source of IN below the mixed-phase cloud layer that can nucleate ice given the right conditions near cloud base. Observationalist, such as Alexei Korolev, have suggested from measurements that all sizes of frozen hydrometeors are observed near cloud top (suggesting turbulent mixing of hydrometeors within the cloud layer). In addition, it is very difficult to distinguish this information from remote sensors because there is a mixture of new small particles combined with the larger particles that are growing and eventually fall faster than the updrafts. This is consistent with the results from our model studies, where ice activates quickly and then is efficiently mixed with snow in the mixed-phase cloud layer. This is clearly seen in Figure S1a, where ice+snow number concentrations are well-mixed in the cloud layer. Results from these model studies, remote sensing of mixed-phase clouds, and in-situ measurements highlight the difficulty of determining where ice nucleates in a cloud layer that is well-mixed by turbulent eddies. This discussion has been added to Section 4.1.



**Figure S1:** Time-height cross sections of (A) ice+snow number concentration, in units of  $L^{-1}$ , and (B) IN advection plus subsidence, in units of  $L^{-1}hour^{-1}$ , from CNT simulation. Temperature, in units of  $^{\circ}C$ , shown with black contour lines.

A key aspect of the study resides in the IN reservoir formed by the sub-cloud mixed layer. While SubCL  $N_{IN}$  decreases much faster than CL  $N_{NI}$  after 10h, it seems that  $N_{IN}$  also increases faster than  $N_{NI}$  before 10h in the control run (there is a jump of  $\sim 200 \text{ mL}^{-1}$  for  $N_{IN}$  compared to  $\sim 100 \text{ mL}^{-1}$  for  $N_{NI}$ ). This looks almost like more IN particles are released in the sub cloud layer than ice crystals are formed in the cloud. Could you please clarify this point and explain why sub-cloud  $N_{IN}$  increases so rapidly at first? How do the total number  $N_{IN}+N_{NI}$  in the cloud and sub-cloud layers evolves in each case (this could be an addition to figure 7d)? Also, what is causing the reversal in the integrated  $N_{IN}$  trend after 10h while sublimation fluxes remain roughly constant?

The rapid increase in subcloud  $N_{IN}$  is due to the rapid deepening of the mixed-layer (due to the height of the mixed-layer base) in the first 10 hours, seen Figure 8c. After the first 10 hours the mixed-layer deepens at a slower rate.

Looking at Figures 7 & 8, it seems that the simulated clouds in the control and noRecycle runs are almost identical at the beginning of the analysis period, that is at 6h. Only the IN flux through cloud base is initially different between the two cases, highlighting the influence of sub cloud IN recycling. Could you indicate why is recycling apparently so unimportant in controlling the cloud properties before 6h?

This is because the NoRecycle run is started with the Control state at hour 6 in order to prevent the runs from diverging during spinup. A sentence has been added on page 11734 line 20 which reads, 'The NoRecycle run is started from the Control run at hour 6 to prevent the two simulations from diverging due to spinup.'. Apologies for leaving this information out of the first version of the paper.

Moreover, on p11738-114/17, it is stated that "the rapid increase in LWP [...] increases the turbulent mixing of IN from the sub cloud layer into the cloud layer". This statement seems however to contradict Figure 8b where the IN turbulent fluxes at cloud base are always lower in noRecycle compared to control.

Turbulence is larger in NoRecycle due to the larger LWP but the IN gradients are smaller due to the rapid depletion of IN, therefore the turbulent flux of IN at cloud base is smaller in NoRecycle compared to Control. The sentence has been left unchanged.

Finally, Figure 8b also shows almost constant sublimation fluxes in the control run during the whole simulation time. Sublimation should however release water vapor and thus reduce the sublimation rates and IN recycling. This does not appear to be the case here. How can sublimation be sustained at nearly constant rates in these conditions, without the sub-cloud layer becoming saturated with respect to ice? How do ice saturation and the water vapor mixing ratio evolve in the sub-cloud layer in your simulations? Additional figures showing the evolution of ice saturation, water vapor mixing ratio and temperature in the sub-cloud layer might help. More generally, relative humidity in the mixed-layer seems to be the most important ingredient determining the role of IN recycling in sustained ice production. From that perspective, how representative is the studied case compared to typical AMPS?

Two time-height cross sections of horizontally-averaged relative humidity with respect to ice and water vapor mixing ratio have been added to the paper in Figure 7c,d. The Figure shows the continuous drying of the sub-cloud layer due to the dominance of the drying due to the water vapor flux over the moistening due to sublimation. This is what allows sublimation to be maintained in the simulations. The consistent drying and cooling of the mixed layer due to radiative cooling and turbulent mixing in these simulations is fundamental in any AMPS that is precipitating ice out of the mixed layer and has limited fluxes of water vapor at the mixed layer base. Of course, if water vapor fluxes at the mixed-layer base increases to the extent that the mixed layer becomes saturated with respect to ice then recycling will be shut off.

#### **Minor comments and technical corrections:**

1. p11728-124: "a liquid layer that precipitates ice crystals" sounds a bit strange. Please consider rephrasing.

The sentence has been rewritten to read, "AMPS are characterized by a liquid cloud layer with ice crystals that precipitate from cloud base even at temperatures well below freezing..."

2. p11729-120: "mineral dust, soot, sea salts and bacteria", as well as the list of references given 122-24. Perhaps match the corresponding references with the different IN compounds, i.e. "mineral dust (Mohler et al., 2006; Welti et al., 2009...), soot (DeMott, 1990...), sea salts (Wise et al., 2012)..."

Sentence changed as suggested.

3. p11730-122/26: "We posit that recycling plays a significant role more generally since, for example, assuming an adiabatic vertical profile, a 650 m-deep mixed layer with a cloud-top temperature of -16C requires a water vapor mixing ratio of at least 1.7 gkg<sup>-1</sup> at mixed-layer base to be saturated with respect to ice, i.e, in order for recycling to be a negligible source of ice nuclei in the mixed layer." The sentence may be slightly too long. Please consider splitting or rewording it. Besides, isn't it a little bit too strong to suppose that IN recycling may be significant unless the mixed-layer is saturated with respect to ice (if I understood correctly the meaning of this sentence)?

Yes, this sentence is worded too strongly. We feel the sentence read better as one sentence and does not need to be split into two sentences. The wording has been rewritten to read, "We posit that recycling may make IN available for nucleation more generally since,..."

4. p11731-124/26: "a cloud layer extending into the inversion by 100 m, cloud base at 0.9 km, and cloud top at 1.5 km." How stable were these conditions? How did the cloud boundaries evolve in time according to the radar retrievals? How long did the AMPS persist over Barrow?

This sentence has been changed to read, “Measurements from ground-based, vertically pointing, 35-GHz cloud radar, micropulse lidar, and dual-channel microwave radiometer at Barrow indicated a mixed-phase cloud layer starting at 8UTC on 8 April 2008 with a cloud top at approximately 1.5km that slowly descended to approximately 0.5 km over a 26 hour period. At the time of the 17:34 sounding the cloud layer extended into the inversion by 100 m, had a cloud base at 0.9 km, and cloud top at 1.15 km.”

5. p11732-l10: How was the average value of 0.4 L-1 calculated based on the 2D-S and 2D-P probes? Is it an average from the two devices, or were data from 2D-S and 2D-P for different size ranges combined?

The data from the 2D-S and 2D-P were averaged to together to get an average number concentration. The sentence has been reworded to read, “Ice crystal number concentrations measured by Stratton Park Engineering Company 2D-S and Particle Measuring Systems 2D-P optical array probes for sizes larger than 100 mm together averaged 0.4 L<sup>-1</sup>.”

6. p11732-l24: How was this particular value of the divergence (2.5x10<sup>-6</sup> s<sup>-1</sup>) selected? What about the initial surface pressure and surface fluxes?

The section on the divergence has been changed to read, “Divergence is assumed to be 2.5x10<sup>-6</sup>s<sup>-1</sup> below the temperature inversion and zero above, giving a linear increase in large-scale subsidence from zero at the surface to 2.7mms<sup>-1</sup> at the base of the initial inversion (z = 1.1 km). This value for divergence was chosen so that the height of the temperature inversion at cloud top is steady. The divergence used in this study is smaller than the divergence used in the WRFLES study of the same case by Solomon et al. (2014) due to the reduced LWPs in this current study and therefore reduced turbulent entrainment that balances large-scale subsidence in a steady simulation.” In addition, the sentence, “Initial surface pressure is 1020 hpa.” has been added before the description of divergence.

Additional details have been added about the surface layer scheme, “Surface fluxes are calculated uses the modified MM5 similarity scheme with calculates surface exchange coefficients for heat, moisture, and momentum following Webb (1970) and uses Monin-Obukhov with Carlson-Boland viscous sub-layer and standard similarity functions following Paulson (1970) and Dyer and Hicks (1970).”

7. p11734-l12/23: I would suggest moving the paragraph at the end of the section, i.e. after the description of the IN/recycling parameterization.

The paragraph has been moved to after the description of the IN/recycling parameterization.

8. p11736-l6/7: If I am correct, 5.8 L-1 is the TOTAL IN concentration, i.e. the sum of the NIN in each bin. Maybe this should be specified. Also, it could be interesting to have an idea of the NIN distribution with respect to the threshold temperature. You could for

example indicate what the IN concentration within the first and last bins is. Besides, is there any particular treatment required for the first bin as it includes all the particles active between -15.5C and 0C? Is it correct and realistic to say that all the IN within the first bin spontaneously nucleate ice as soon as water saturation is reached, i.e. at cloud base?

Page 11736 lines 4-5 have been changed to read, "...an initial  $N_{IN}$  summed over all bins at every gridpoint equal to  $5.8 \text{ L}^{-1}$ ..." Also, at line 6 a discussion has been added regarding initial  $N_{IN}$  in each bin, "In a discrete bin formulation this results in  $3.26 \text{ L}^{-1}$  in the warmest bin and  $0.23 \text{ L}^{-1}$  additional IN that are available for nucleation in the coldest bin. Given the initial temperatures in the cloud layer, all IN from the first bin in the cloud layer nucleate. This causes an initial spike in cloud ice number concentration, which also causes a large precipitation flux out of the mixed layer. It takes approximately 6 hours for the cloud layer to reach a quasi-equilibrium with steady cloud ice production."

9. p11736-l23: Is it a liquid or a mixed-phase cloud layer?

"liquid" has been changed to "mixed-phase".

10. p11737-l2 (and whole text): Notations " $N_{NI}$ " (number of ice crystals) and " $N_{IN}$ " (number of IN) can be easily confused. Perhaps a different notation for the number of ice crystals could be used.

Yes, this is confusing. " $N_{NI}$ " and " $T_{NI}$ " have been changed to " $N_{ICE}$ " and " $T_{ICE}$ " to clearly distinguish from " $N_{IN}$ " and " $T_{IN}$ ".

11. p11740-Equation 7 and p11741-Equation 8b: Perhaps change the notation and add a subscript to  $f$  to distinguish between cloud base and mixed-layer base values. Also describe more clearly what allows you to transform equation 8b into 8c.

Thank you but we think the notation for  $f$  is clear and does not need a subscript. To clarify the steps that allow for the transform between eqs. 8b and 8c, "...and since  $f|_{\text{Mixed-Layer Base}}$  is downward and  $f|_{\text{Mixed-Layer Top}}$  is negligible (eq. 5), ..." has been added between eqs. 8b and 8c.

12. p11741-l18: "In SW": please rephrase to introduce more clearly the sentence (maybe "in the presence of short wave radiation", is it what you mean?).

The sentence has been changed to read, "In the presence of shortwave radiation (i.e., in the SW simulation),..."

13. p11741: The analysis does not account for any water vapor flux. Sublimation releases water vapor in the sub-cloud layer, and vapor is also transported through the mixed-layer boundaries by turbulence. With the water vapor content possibly increasing in the sub-cloud layer, we may expect the sublimation flux to decrease. How would this be

reflected in the simple mixed-layer model, and what would happen in case of a saturated mixed-layer with respect to ice?

A saturated mixed-layer with no sublimation does not change the mixed-layer analysis presented in Section 5. However, from eq 8c using the same assumptions of  $f|_{\text{Mixed-Layer Base}}$  is downward and  $f|_{\text{Mixed-Layer Top}}$  is negligible, when  $S=0$ ,

$$T_{IN}|_{\text{Mixed-Layer Base}} < T_{IN}|_{\text{Cloud Base}}$$

causing IN to be depleted in the mixed layer since entrainment at cloud top is weak.

Also, see the two time-height cross sections of horizontally-averaged relative humidity with respect to ice and water vapor mixing ratio that have been added to the paper in Figure 7c,d. The Figure shows the continuous drying of the sub-cloud layer due to the dominance of the drying due to the water vapor flux over the moistening due to sublimation.

14. p11741-end of page: Please provide a short summary of the main implications for actual AMPS clouds, and for the conditions under which IN recycling is relevant.

This discussion has been added at the end of page 11741, “This mixed-layer analysis provides a framework to understand the results presented in Section 4. Specifically, sublimation being less than the turbulent flux of IN is seen to be a property of a well-mixed layer where the total flux at mixed-layer base is downward and the total flux at the mixed layer top is negligible. In the case where the mixed layer is saturated with respect to ice, sublimation is equal to zero and the turbulent flux of IN at the mixed-layer base is less than the turbulent flux of IN at the cloud base, reducing the flux of IN into the cloud layer. The relationships outlined in this section are appropriate for any AMPS with weak entrainment at cloud top, weak large-scale advective fluxes, and net downward fluxes at the mixed-layer base.”.

15. p11742-13: If I understand correctly, the start of the green arrow corresponds to sunrise, the tip of the blue arrow corresponds to maximum SW, and the tip of the red arrow corresponds to sunset. Am I right? It looks actually like the moon and sun symbols on Figure 11761 are not absent in the manuscript.

The start of the green arrow corresponds to maximum SW (see Figure 11b). The arrows are used as a means to refer to transitions in the cloud mixed-layer properties. Again, apologies for the missing symbols. It must have been a challenge to review this paper with incomplete figures and we appreciate the effort that you have gone to to understand the results of this study.

16. p11742-15/7: What causes the relative humidity to be low at this time? More generally, what causes the simulated RH cycle despite continuous sublimation and surface decoupling? Besides, what happens to the precipitation flux at this time?

When incoming solar radiation is at a maximum, LWP is at a minimum, which causes turbulent mixing to be at a minimum. Also, the cloud layer and mixed layer warm, causing less ice production and less precipitation into the subcloud layer. The warming of the subcloud layer together with the reduction in precipitation causes the relative humidity to be at a minimum.

17. p11742-123/24: An increase in in-cloud ice concentrations could enhance the precipitation fluxes in the sub-cloud layer and may dominate over the decrease in downward turbulent NI flux. Why isn't it the case here? More generally, the role of precipitation should be more specifically stressed in section 6. To complement the discussion, it may also be interesting to compare the states at the beginning and at the end of a full diurnal cycle (Figure 11 shows only a 20h cycle).

We actually do say here that both IWP and precipitation increase when incoming solar radiation is a minimum. There is a net downward flux of NI out of the cloud layer. This discussion was just meant to point out that turbulence acts against this downward flux. Figure 11 shows a 24-hour cycle, the red arrow only extends to hour 62 and the green arrow begins at hour 42, while the cycle is for the 40-64 hour period.

18. p11743-12: Sub-cloud drying has not been mentioned earlier in the study, and Figure 11 does not seem to confirm that. A figure showing the evolution of below cloud humidity in the control and SW runs would be very instructive. Again referring to my main comment, relative humidity in the mixed-layer is a key ingredient controlling IN recycling via sublimation. Can you please comment on this and stress the relevance of the case study presented compared to typical AMPS.

Two time-height cross sections of horizontally-averaged relative humidity with respect to ice and water vapor mixing ratio have been added to the paper in Figure 7c,d. The Figure shows the continuous drying of the sub-cloud layer due to the dominance of the drying due to the water vapor flux over the moistening due to sublimation. Fig. 7 also shows the time-height cross sections of horizontally-averaged water vapor mixing ratio and relative humidity with respect to ice. This brief discussion has been added at the end of the second paragraph in Section 4, "Fig. 7 also shows the time-height cross sections of horizontally-averaged water vapor mixing ratio and relative humidity with respect to ice. These figures show that the continuous drying and cooling of the mixed layer results in continuous sublimation in the subcloud layer."

19. Figure 1: The grey shadings do not appear clearly or are absent.

There seems to have been a problem with the figures in the online version of the paper. I'm sorry I didn't catch this before the paper was sent out for review. All the figures are complete in the revised paper.

20. Figure 3: Could you please add a legend and axis labels to the figure?



I'm sorry, I don't know why the legends and labels were removed from the online version of the paper. We will make sure the revised version is correct.

21. Figure 4: In the caption, I guess that the second sentence refers to figure c).

The figure caption has been reworded for clarity, “A,B,D) Sensitivity of LWP and IWP to snow density and fall speeds. LWP shown with solid lines and IWP shown with dashed lines, in units of  $\text{g m}^{-2}$ . C) Fall speeds used in sensitivity studies, in units of  $\text{m s}^{-1}$ .....”

22. Figure 7, 8 & 10: The horizontal axis labels and titles are missing.

Our apologies for this. The labels and titles were removed in the online version for some reason. We have added the labels and titles back in and will insure that the revised online version is complete.