We would like to thank both referees for their time and effort in reviewing this manuscript.

Review comments are in blue and the responses by the authors are in black text.

Reviewer #1:

1) Throughout the manuscript, it is questionable to draw direct conclusions on the process of ice nucleation because nucleation alone may not be the only mechanism of initiating cloud ice. In particular when riming is assumed to be non-negligible, the potential roles of rime splintering should be pointed out. In addition to that, mechanisms like droplet shattering upon freezing and upon ice-ice collisions have been under discussion recently. Overall, each of the ice-initiating mechanisms seems to be far from well-described or understood. Therefore, it feels more appropriate to refer to modified ice number concentrations only, rather than linking the indirect observations described here to exactly one of these processes. In fact, a recurring finding in studies of ice nucleation is that the concentrations of ice nuclei cannot explain ice number concentrations. Nevertheless, a discussion of potential mechanisms will be valuable at some point of the paper.

We agree with the point made by this comment that it is questionable to draw a clear and direct conclusion on ice nucleation processes based on the evidence we present in the paper. However, we feel these observations point to a shift in cloud microphysical processes due to the influence of aerosols. The manuscript, and specifically the Discussion section, has been reworked so that claims made about process level phenomena are more general in nature.

The Discussion section has been redone as follows:

- We put forth our inferences from the reflectivity and fall speed retrievals: among clean and polluted clouds, the cloud top reflectivity and IWC values are similar but the polluted clouds have significantly higher fall speeds. This is an indication that polluted clouds have fewer, but larger, hydrometeors. We then comment on why we feel ice is present in the clouds and why we feel we are not exclusively observing liquid only clouds. Our argument is that the mean Doppler velocities are relatively high which is suggestive of clouds containing ice particles. This is the main claim we are trying to show: that aerosols tend to coincide with mixed-phase clouds that have reduced ice crystal number concentrations and ice mass production. The discussion of the physics governing this suppression is of secondary importance because we do not have the appropriate measurements to pin down the details.

- We then list several physical mechanisms for producing cloud ice that could be altered by aerosols. We do not attempt to state which processes are responsible for the observed results. The list includes: depositional growth efficiency, riming efficiency, ice shattering and splintering (Hallet-Mossop properties).

- Yes, IN concentrations have been shown to be too low to fully explain the observed levels of ice in Arctic mixed-phase clouds, and therefore other ice crystal production methods must occur, However, these *secondary* methods (e.g. crystal shattering) are dependent on ice crystals being present and therefore, one would assume, on ice crystal number concentrations which are in part a function of the heterogeneous ice nucleation rate. It is therefore reasonable to assume that perturbations in the ice nucleation rate will be relevant to the overall ice crystal number found throughout the cloud layer. The complexities of ice production are an interesting question, and we hope that this paper, and specifically the Discussion section, contributes to the dialogue on how aerosols influence cloud ice properties.

Several reviewer comments related to claims made in the Discussion Section and we attempted to address these concerns through restructuring this section and focusing the paper more on the observed results of the higher IWC profiles found in clean clouds, and less on the physical mechanisms responsible for this shift in cloud IWC.

The Discussion section now reads as follows:

4 Discussion

The observations presented in Sect. 3 indicate that polluted clouds have reduced amounts of cloud ice mass for a given amount of condensed liquid mass. In the cloud top region of polluted clouds, the high V_f signifies larger hydrometeors, and since the differences in IWC and reflectivity here are not statistically significant between cloud types, the implication is that polluted clouds have a reduction in hydrometeor concentration. We feel comfortable stating that the hydrometeor population contains ice crystals for a few reasons: (1) the mean doppler velocity values near cloud top of $\sim 0.2\ ms^{-1}$,represent downward motion of hydrometeors that are characteristic of a cloud containing both ice particles and liquid drops, and these values are higher than expected for a liquid cloud ($\sim 0\ ms^{-1}$). (2) The mean minimum cloud layer temperatures for each LWP bin are below -12℃. (3) The change in velocity observed with increasing aerosol concentrations is inconsistent with that of a cloud containing only liquid droplets, as increasing aerosol concentration typically reduces the effective radius of liquid cloud drops thereby reducing fall speed. Here, the opposite is observed, and we feel this is evidence for a more complex system that contains ice.

Further down in the cloud layer, as ice crystals undergo growth processes, V_f and IWC both increase. A key characteristic is that clean clouds, for all LWP bins, have substantially greater increase in IWC by cloud base. This is evidence that clean clouds have microphysical *properties that are conducive to increasing ice mass. While we do not have the observations to pinpoint the exact physical processes responsible for the increased IWC, we briefly speculate on a few of the possible mechanisms.*

4.1 Cloud top reduction in ice formation

The cloud top region is an area of the cloud where ice crystals tend to have had little time to interact with the cloud system, and therefore this region contains the most direct picture of heterogenous ice nucleation. As previously stated, polluted clouds appear to have lower ice crystal concentrations, which is evidence for a reduced ice crystal nucleation rate. Two potential mechanisms for this reduced nucleation rate are:

1. *Hydrophobic aerosols can be coated by hydrophilic compounds, and thus limit their effectiveness to nucleate ice (Diehl and Wurzler, 2004; Girard et al., 2005; Kulkarni et al., 2014). Variations in the winter and spring time scattering coefficient measurements used in this study are most strongly influenced by fluctuations in* SO_4^- *sulfate aerosols (Quinn et al., 2002). Therefore, the conditions we define as polluted are ones in which INP are likely to be coated by sulfates, reducing the efficiency at which they nucleate ice. Through this lens, we would expect polluted conditions to be associated with a reduction in ice crystal nucleation rate. We refer to this as the INP mechanism for ice nucleation suppression.*

2. *There may be a size dependence to the ability of liquid droplets to freeze, but the literature is murky as to why this is the case. Ideas include freezing point depression from increased solute concentrations (de Boer et al., 2010), though at the liquid drop sizes typical of Arctic mixed-phase clouds, this seems unlikely to be causing the reduced nucleation rate. More probable is that the greater surface area of larger drops provides a larger interaction area for the liquid drop with their environment, such as contacting an INP, and thus larger drops have a greater probability to freeze. Through the first indirect aerosol effect, the increase in aerosol concentration reduces both the mean droplet size and the width of the drop size distribution (Chandrakar et al., 2016). The suppression of the ice nucleation rate through a reduction in the mean diameter of liquid droplets in mixed-phase clouds (e.g., Lance et al. 2011) could explain the observed reduction in ice crystal number at cloud top. We refer to this as the CCN mechanism for ice nucleation suppression.*

Our observations are consistent with simulations done by Girard et al. (2005), that show increasing sulfuric acid aerosols in Arctic clouds reduces ice crystal number concentrations while mean ice crystal size is increased. Other studies have found evidence for the ability of sulfates to suppress the onset of heterogeneous freezing (Eastwood et al., 2009), and such inhibition results in the generation of fewer but larger ice crystals (Jouan et al., 2014). However, we currently do not have the measurements needed to determine which mechanism (CCN or INP) is playing the bigger role in controlling ice production in these clouds. Observing in-cloud ice crystal size distributions with optical probes would provide insight into the size and shape variability of nucleated and grown ice crystals. The variability in nucleated ice crystal size should be linked to the in-cloud CCN or INP properties, with higher ice crystal size variability expected if INP are the dominant control on nucleation.

4.2 Ice mass growth

The reduced nucleation rate in polluted clouds has implications for the total amount of depositional ice mass growth in the cloud layer. The depositional growth rate for an individual crystal is proportional to the inverse of the effective radius of that ice crystal for most crystal habits (Rogers and Yau, 1989). This implies that depositional growth will lead to convergence of ice crystal sizes given sufficient time for growth to occur. In the clouds under study, we believe the in-cloud residence time of an ice crystal is greater than the time it takes for this size convergence to occur – see Appendix A. This suggests that IWC gained through deposition is strongly determined by initial crystal number, and not by initial crystal size. Thus, the higher ice crystal number concentrations found in clean clouds directly result in greater total amounts of depositional growth.

In addition to deposition, riming and rime splintering can contribute to increased IWC in clean clouds. The highest LWP regime, LWP4, is the only case in which ice crystals in clean clouds have greater V_f than ice crystals in polluted clouds at cloud base. Here, we suspect liquid water and ice properties in clean clouds promote greater levels of riming, leading to the observed high fall speeds. Riming is an efficient mechanism for increasing fall speeds of larger ice crystals because unlike deposition, riming efficiency increases with ice crystal effective radius (Erfani and Mitchell, 2017). Additionally, riming efficiency increases liquid drop size, and it has been shown that riming efficiency is strongly related to the presence of large liquid drops (Borys et al., 2003; Lohmann, 2004). Clean clouds are expected to have greater concentrations of efficiently collected large liquid droplets ($> 10\mu m$), along with higher numbers of ice crystals. Conversely, in polluted clouds production of both large liquid drops (Chandrakar et al., 2016) and ice crystals is suppressed. Moreover, riming is associated with rime splintering, a process that generates small ice crystals when a liquid drop is collected by an existing ice crystal (Hallett and Mossop, 1974. Rime splintering, which occurs more commonly at warmer temperatures ($-3 \text{ to } -8℃$), increases the ice crystal number concentration. This may help to explain why LWP4 bin clouds have the highest observed levels of IWC despite the tendency of these clouds to be warm. Likewise, at temperatures too cold to support rime splintering, fracturing of ice crystal due to crystal on crystal collisions can increase number concentrations.

We expect the level of riming to be related to the amount of liquid water contained within the cloud. In the LWP1,2 cases, the low amount of liquid water makes riming relatively less efficient and perhaps non-existent. Therefore, we speculate that ice mass gains in these low LWP clouds are mainly occurring through depositional growth. These clouds also tend to have cold temperatures, promoting the growth of dendritic crystal habits. Dendrite fall speeds are slow relative to other crystal types with similar mass (Kajikawa, 1974), and therefore these ice crystals have long in-cloud residence times, enhancing depositional growth. Such depositional ice mass growth is consistent with the observed high cloud-base IWC and low V_f of LWP1,2. For LWP3, we suspect that higher amounts of liquid water promote greater rates of riming. With sufficient liquid water, riming alters ice crystal shape and mass such that it falls at greater velocities relative to its size (Jensen and Harrington, 2015). The higher V_f reduces ice crystal residence time, limiting depositional growth. Changes to ice crystal habit can also decrease the surface area of the ice crystal, which limit the depositional rate (Jensen and Harrington, 2015). We speculate that in LWP3, which has an intermediate LWP level, ice mass growth through riming cannot compensate for the limited mass gain through deposition, and thus relatively lower cloud base IWC with a corresponding higher V_f are observed (in relation to LWP1,2). In *the LWP4 cases, high riming rates lead to fast-falling ice crystals and reduced cloud residence times. Mass gained through deposition is relatively small, but this reduction in depositional growth is more than compensated for by the elevated levels of riming and increased ice crystal numbers due to rime splintering. This is consistent with observed high fall speeds at cloud base in the LWP4 case to go along with high IWC levels.*

2) A point that remains unclear to me is related to the subsampling of data. The focus of this study are mixed-phase clouds, so the threshold for humidity is chosen to be 100% saturation with respect to ice. However, mixed-phase implies the presence of liquid droplets. In a mixed-phase cloud, humidity is usually close to water saturation (or higher in strong updrafts), otherwise the cloud droplets would evaporate quickly. Therefore it would be straightforward to choose water saturation, or a value close to that. Instead, the choice of RHi=100% explicitly includes humidities well below water saturation since temperatures are constrained to T<6 ◦ C. The authors indicate there may be also clouds with tiny or without any liquid in the LWP0 category, sometimes explicitly called ice clouds (e.g. page 11, line 7). I suspect that such definitions may have a big impact on the results within this LWP bin, and indeed the properties seem to behave distinct in some ways. Therefore I strongly suggest to explore the impact of the threshold for humidity. Nevertheless, this kind of threshold may be problematic generally, since the saturation in clouds can be highly variable and is strongly tied to the structure of turbulent eddies, in particular with a small content of cloud droplets. So how meaningful is the profile of a single sounding which is supposed to represent intervals of 6 to 12 hours? Based on the manuscript I also cannot get an idea of how helpful the Mergesonde product is in addressing the problem of variability. Generally, to improve the clarity of the overall picture of mixed-phase, it might be beneficial to exclude ice-only clouds and introduce a lower threshold for LWP in the LWP0 category, e.g., to exclude effects like sublimating small ice (see also below).

We apologize for any confusion that the LWP0 (ice cloud) bin may have caused. The motivation for including clouds with little to no liquid water path, is to provide a null case that allows for greater contrast when examining the physical processes behind IWC production that become available when liquid water is present. That is, we are using LWP0 bin clouds as a basis for comparison between cloud types that have the potential for high rates of deposition and riming to those that do not.

If having LWP0 bin clouds remains an issue for the reviewer, we can easily remove these clouds from the analysis without significant alteration to the overall results of this paper. This LWP bin is a function of the uncertainty in the LWP retrieval from the microwave radiometer, which is roughly 15g/m2. We cannot say with a high probability that any one cloud with a LWP below this threshold contains liquid water, and so at times, we refer to these as *ice clouds*. We have gone through the paper and removed instances where we call the LWP0 bin as ice clouds and instead refer to them as LWP0 bin clouds.

Regarding using relative humidity with respect to ice (RHi) as a designator throughout the paper, rather than with respect to liquid, we chose to use RHi because portions of this study deal with ice crystal nucleation and depositional growth processes. These physical processes are linked to the level of saturation with respect to ice in the cloud. Additionally, we have other (and better) methods of determining the presence of water in the cloud. Specifically, liquid water path is determined from microwave radiometer data. This is done for precisely the reason the reviewer mentions in the comment -- we do not have a sufficient data record (at the needed spatial or temporal resolution) on cloud humidity levels to determine saturation at the accuracy needed to make claims about the presence of liquid water. The cloud structure is too dynamic and variable to rely on 6-12 hour interpolations of radiosondes to determine humidity fields. The motivation for using RHi, is not to ensure a mixed-phase cloud, but rather it is used to gain a general view of the synoptic scale conditions that the cloud forms in. This paper does not rely on the mergesonde data to make strong claims, but rather we look for any differences in ice saturation across the complete sample of clouds to see if there is an obvious cross correlation that can explain observed differences in IWC. And finally, since ice supersaturation is the driver behind ice deposition, and therefore, we feel that it is the intuitive variable to use for defining the level of water vapor in the cloud layer.

We do have data for regions that are sub-saturated with respect to ice (for example, below cloud base), however we omitted these clouds from the study to simplify the set of physical processes that are occurring within the cloud layer. We only want to focus on deposition of ice, and less on clouds where sublimation is a significant feature.

3)The discussion of results would benefit a lot by outlining the strategy of how the profiles of reflectivity, ice water content and fall speed will be interpreted. At this point it will be also helpful for the reader to explain what it means to use a reflectivity-weighted fall speed which actually represents the tail of the largest particles of the size distribution. For example (as on page 11, line 8), assume we compare two situations with the same IWC, but different Vf, where the latter difference would be caused only by the size distribution width. What is the measure of mean size and what would it mean in terms of the number difference? Otherwise, is the general assumption that the width of the size distribution is the same for both polluted and clean clouds and is it a good assumption?

A hydrometeors ability to reflect radiation back to the radar is approximately proportional to the sixth power of its diameter. This non-linear relationship between particle size and reflectivity means that the largest particles in a sampled volume contribute the most signal to the reflectivity measurements. In the case of an ice crystal size distribution, given a fixed IWC, whether the distribution is broadening or shifting upward in size cannot be explicitly determined from the radar reflectivity measurements. However, an increase in reflectivity implies more large particles in both cases. For the results in this paper, the implication is that the increase in V_f, a reflectivity weighted fall speed, found in polluted clouds is the result of an increase in the presence of larger ice crystals. This, in conjunction with the IWC information, allows us to make broad claims about particle concentrations.

To provide the reader with more context for interpreting the results, the following paragraph has been added to the start of section 3:
In this section we present mean in-cloud profiles of reflectivity, IWC and V_f for the polluted and clean clouds found in each of the LWP bins. We use the relative relationships between IWC and V_f amongst the clean and polluted clouds to make inferences about ice crystal size and number concentrations. This is followed by a discussion of possible microphysical processes within the cloud that may be causing the shifts in crystal size and concentration.

To section 3.3, the following discussion has been added:
Since the measured radar reflectivity scales approximately with the sixth power of hydrometer size, it is the largest hydrometers that will reflect the most radiation back to the radar. Thus, the reflectivity, and in turn the fall speed signal, are dominated by the largest hydrometers in the sampled volume. If a fixed amount of ice is sampled, it is not possible to determine if increases in reflectivity are due to an increase in the ice crystal size distribution, or a broadening of this distribution. However, the non-linear response of reflectivity to ice crystal size does mean that there is an increase in the presence of large ice crystals (sizes greater than the geometric mean). This knowledge about the relative populations of large ice crystals lets us make broad claims about ice crystal number concentrations in these clouds.

Minor points:
Page 2, line 25: suggest Barrow, Alaska
Updated manuscript by adding the word *Alaska*, to reflect comment. Additionally, the Barrow name has been changed to Utqiaġvik, to reflect the current name of the city.

Page 3, line 12; Page 17 line 11: I recommend to rephrase “secondary ice mass growth” because it may be misleading and imply some connection to secondary ice production such as rime splintering. Personally I don't see a need to call growth secondary, assuming that any initiating process would not be "growth".

Valid point and the passage has been reworded to omit the word "secondary".

Text now reads: *This includes aerosol influences on both nucleation of ice crystals and ice mass growth processes*.

Page 5, line 1: Does the analysis ensure that only cloud decks are analyzed with a cloud fraction of 100% for a period considerably longer than the 120 min window? The statement on page 9, last line, seems to imply there would be lateral entrainment at the cloud boundaries.

No, the analysis does not select for periods when cloud fraction is 100 percent. However, we did perform sensitivity analysis on the impacts of the averaging window time period on the ice crystal fall speed, V_f, and V_f remained rather consistent. This consistency also indicates that the majority of the sampled cloud volumes did not come near cloud edge, and that the eddy structure contained within the clouds was variable. The statement at the end of Page 9 was referring to the possibility of vertical mixing. This sentence has been updated to reflect this:

This decrease in the rate of IWC increase near cloud base is likely due to the impacts of less saturated air entraining vertically into the bottom of the cloud, slowing growth processes in this region.

Page 7, paragraph 1: To get a sense for the analyzed data, the total amount or fraction of analyzed days would be interesting.

A table with summary statistics, including fractional occurrence of cloud type, has been added to this section of the paper.

Added table:

$LWP\ Bin$	$Mean\ Cloud\ depth, [m]$	$Mean\ LWP, [gm^{-2}]$	$Mean\ T_{min}$, [°C]	$Mean\ RHi_{max}$,[%]	$Number\ profiles$
LWP0,C	541	3.49	−21.8	110.2	986
LWP0,P	580	3.61	−23.7	111.1	1155
LWP1,C	380	17.85	−17.2	109.5	832
LWP1,P	433	17.15	−20.5	114.7	972
LWP2,C	372	43.22	−14.7	109.3	3286
LWP2,P	395	39.87	−18.2	112.2	2236
LWP3,C	448	109.07	−13.2	109.1	2785
LWP3,P	422	91.51	−15.9	108.6	1846
LWP4,C	548	213.31	−12.3	108.1	1491

LWP4,P	492	244.35	−15.0	106.5	654
All Bins	444	72.12	−16.4	109.9	16244

Table 1: Summary statistics for the sampled clouds in each LWP bin. The last row is the aggregate sample from all LWP bins.

Page 12, line 13: This is one of the points when I stumble over ice clouds rather than mixed-phase clouds while both the title and abstract make different statements. The effect of ice sublimation would be a clear indicator of lacking water, so why include such situations?

We include ice clouds mainly as a set of clouds to reference the liquid containing clouds against. Ice clouds are used as a null case where riming, and other liquid dependent ice mass growth processes, are not a factor.

Page 16, line 5: With droplet freezing being the primary nucleation mechanism, the saturation as such is not the relevant variable, but temperature is.

This section of the paper has been reworked in response to other comments and the sentence is no longer in the paper. However, we were trying to say that, in regions of the cloud where riming is not a significant factor in IWC production, heterogeneous ice nucleation and deposition are the main controls of IWC. And while temperature is the main control on nucleation, the available water is a control on deposition.

Page 16, line 7: How reliable are such conclusions about ice number when the estimate of IWC may have a relative error of 100%?

We attempt to address the uncertainty of the IWC retrieval with the use of large samples of clouds that can be used to represent the cloud population. We preformed statistical test at a 95 percent confidence level to determine if differences between clean and polluted clouds exists.

Page 16, line 16: Hoffer 1961 may be an appropriate reference for the freezing behavior of drizzle or rain drops in which the aerosol content would scale, more or less, with drop mass due to collisional growth. However, cloud droplets mainly grow by condensation, and thus will contain the same amount of aerosol during growth. This is different from Hoffer's method of producing particle-containing drops, while we expect that more aerosol surface area per drop would yield a higher chance of freezing.

In the process of responding to major comment 1), the Hoffer reference has been removed. The discussion on the possible physical processes controlling ice growth in clouds has been significantly altered to the point where we feel this comment has been sufficiently addressed.

Page 16, line 29: Due to the low temperatures investigated by Eastwood et al. 2009, it seems that this publication is hardly relevant for very most of the clouds summarized in Fig. 5. Also their humidities were mostly well below water saturation, while I am still assuming that the manuscript focuses on mixed-phase clouds.

This a good point, and the fact that the Eastwood et al. paper deals with conditions that are not saturated with respect to liquid, was an oversight on our part. However, there is a case in the Eastwood et al. paper in which sulfates require that there be water saturation for ice nucleation to occur. This does not provide absolute evidence for our claim, but it does support it.

Page 17, line 1: The statement on CCN is hard to understand, please rephrase.

This section of the text has been rewritten and in that process this statement was removed.

Page 17, line 14: typo: Yao
Corrected

Figure 6: "December is assigned the value 0" might be showing up inadvertently, otherwise I do not understand.

This is a typo and has been removed.

Reviewer #2:

While this study is timely and sorely needed, I feel that their criteria for determining what is an ice cloud at the minimum are not well explained and need to be better justified as their criteria can easily include liquid clouds with drizzle or even larger cloud droplets. Since this affects most of their dataset I feel that this issue is the most important to address before I can accept this for publication.

This general comment is addressed in the response to the following comments.

Line 15: "ice nucleation" – do you mean reduced secondary production or reduced nucleation via increasing the liquid water content and therefore decreasing total available Supersaturation?

In response to another comment, the term *secondary production* has been removed from Line 15. In this paper, ice nucleation only refers to the generation of a new ice crystal through homogeneous or heterogeneous nucleation, and it does not include rime splintering or any other secondary ice production processes.

The sentence containing line 15 has been altered to the following:
We additionally analyze radar-derived mean Doppler velocities to better understand the drivers behind this relationship, and conclude that aerosol induced reduction of the ice crystal nucleation rate, together with decreased riming rates in polluted clouds, are likely influences on the observed reductions in IWC.

Section 1, Paragraph 5, introduction: I think a more detailed introduction to the three indirect effects in mixed phase clouds are needed: the thermodynamic indirect effect, the glaciation indirect effect, and the riming indirect effect. Figure 1 of Jackson et al. (2012) provides a good summary of Lohmann and Feichter's three mixed phase indirect effects.

We agree that providing a more thorough background in aerosol indirect effects strengthens the paper and we have added the following to paragraph 5 of section 1:

That being said, several aerosol-cloud effects have been detected in mixed-phase cloud systems: the first and second aerosol indirect effects have been observed (Lohmann and Feichter, 2005). These two aerosol indirect effects, associated with the liquid phase of cloud, lead to further aerosol-induced implications in mixed-phase clouds. The thermodynamic indirect effect, whereby the reduced mean liquid drop diameter caused by increasing CCN makes cloud conditions less favorable to secondary ice production (e.g. rime splintering, contact nucleation), has the effect of reducing IWC in mixed-phase clouds with high CCN levels. Similarly, the riming indirect effect, the process in which CCN reduce the liquid drop size distribution so that the liquid drops are less efficiently collected by falling ice crystals, reducing the riming rates within a mixed-phase cloud (Borys et al., 2003). The reduced riming rate decreases ice production, and lowers cloud IWCs. Finally, the glaciation indirect effect, in which an increase in aerosols (traditionally INP from black carbon) is associated with greater levels of ice nuclei, which promotes greater conversion of liquid to ice within the mixed-phase cloud layer (Lohmann, 2002). Yet the specifics of how these cloud processes play out over time to determine the macroscale properties of clouds is poorly understood.

Several observational studies have found evidence for aerosol impacts on Arctic mixed-phase clouds. Using surface-based sensors at Barrow, both Garrett and Zhao (2006) *and Lubin and Vogelmann (2006) showed that a reduction of droplet size associated with elevated aerosol particle concentrations results in elevated emissivity of the cloud layer, thereby significantly increasing longwave radiation at the surface and contributing to warming. Lance et al. (2011) used in situ data from Arctic clouds to show that CCN concentrations, through the first indirect effect and riming indirect effect, may have a stronger influence on ice production than do INP concentrations. These past studies suggest that further interrogation of aerosol alterations to the microphysical state of mixed-phase clouds systems is warranted.*

Section 1, Paragraph 6: This paragraph seems to be out of order and interrupts the flow of the paper. I think the information here belongs more to where you discuss how phase partitioning is critical, as it helps to justify why we need to study the phase partitioning of mixed phase clouds.

Agreed, and the manuscript has been changed so that what was Paragraph 6 is now Paragraph 3.

Section 2.1, Paragraph 1: What is the minimum detectable signal of this radar? A monodisperse size distribution of liquid drops with a concentration of 100 cm-3 and maximum dimensions of 20 microns (radius of 10 microns) should result in a reflectivity of about -20 dBZ, which is quite characteristic of the tops of single-layer arctic stratocumulus. Establishing approximately how small of particles the MMCR is sensitive to is critical as liquid cloud droplets have been observed in arctic stratus at temperatures as low as -30 degrees Celsius and I fear that observations of “small ice” that are pointed out in later sections could really be liquid drops.

We agree that it was irresponsible to make strong claims about the phase of ice particles (e.g. “small ice”) at cloud top and we have altered the manuscript to reflect the uncertainty in determining cloud phase in this region of the cloud layer.

We have also made significant changes to the Discussion section that address many of the issues raised by this comment.

The MMCR has a sensitivity down to roughly -50 dBZ, and we fully expect the radar to be observing liquid (in addition to ice) in the cloud top region. While we do not have the ability to directly determine the phase of the hydrometers from the radar, we feel the IWC and fall speed profiles strongly suggest the presence of ice formation at cloud top. Both the IWC and V_f profiles have a significant and continuous increases through the cloud layer to cloud base, which is consistent the generation of ice crystals at cloud top, that fall through the liquid layer and undergo ice mass growth processes. Additionally, at cloud top, the mean Doppler velocity (MDV) does provide some insight into phase, where the observed MDV values are $\sim 0.2 ms^{-1}$, and these high downward fall speeds are evidence for ice. If the radar reflectivity signal is dominated by liquid droplets, we expect MDV to be very close to $\sim 0 ms^{-1}$.

Furthermore, if we are to assume that we are observing clouds that contain only liquid and no ice, the observed aerosol effects on the liquid drop distributions are counter to what is predicted by the first aerosol indirect effect. Traditionally, with increasing aerosol levels, one expects greater number concentrations of liquid drops with reduced effective radius. Yet, in the clouds observed in this study, the cloud top values of hydrometeor fall speed tend to be greater in the polluted cases, which implies the presence of larger hydrometeors. The microphysics of these clouds do not resemble the typical physics of liquid clouds. We feel that this is basic evidence for a more complex system that contains ice.

Drizzle adds more complexity to phase classification from the radar data because drizzle would have MDV values similar to falling ice crystals. Though, at the minimum cloud temperatures used in this study (T<-6 deg. C) we feel that there is a limited impact from drizzle events. Drizzle events will be restricted to the warmer clouds in our data set because the mean minimum cloud temperatures for all LWP bins are well below -10 degrees Celsius (see Figure 5). Moreover, studies have shown that Arctic clouds found in the lower 2km of the atmosphere during the months of December through May, frequently contain both liquid water and ice (Shupe et al., 2005). And mixed-phase clouds are more prevalent at Utqiaġvik than are liquid clouds during the months under consideration in this study (Liu et al., 2017).

Section 2.3. I think that the current criteria to eliminate as many liquid clouds as possible might be too simplistic. Liquid cloud particles can exist at temperatures as low 30 degrees Celsius, and even drizzle has been observed at temperatures as cold as -10 degrees Celsius. The authors need to better establish how sensitive the MMCR is to the smaller liquid particles, or perhaps only include regions that are subsaturated with respect to water but supersaturated with respect to ice in order to adequately ensure that they are only observing taking observations from ice in the clouds.

This comment is closely related to the previous comment where we outline our case for why we feel the radar reflectivity signal is dominated by the ice phase, and not the liquid phase of the cloud hydrometeors.

We are concerned with regions of the cloud that are saturated with respect to water and ice because we are interested in how the liquid properties of the cloud influence cloud ice production. Selecting clouds that are subsaturated with respect to water will limit the occurrences of mixed-phase clouds, which are the main subject of this study.

To avoid confusion about our motives in studying clouds containing both liquid and ice, the following sentence has been added to paragraph 1 of Sect. 2.3:
We are interested in the interaction between the liquid and ice phase hydrometeors in the cloud and therefore we investigate ice in mixed-phase clouds that are saturated with respect to both liquid and ice.

Page 8, line 14. These can easily be liquid droplets. Section 4.2. How much of an impact do you think the Hallett-Mossop process, active at temperatures from -3 to -8 degrees Celsius, would have in your higher LWP bin clouds in terms of secondary production? It may not necessarily be more riming, but there could also be more secondary ice crystals being produced by this process. Laboratory experiments have also shown that when droplets freeze they can produce spicules that then proceed to generate secondary ice crystals (see Lawson et al. 2015).

We agree that we should not be as specific in stating what physical processes are controlling ice production in clouds. In response to this and other comments, Section 4.2 has been altered to include a broader view of secondary ice production and how it may relate to the clouds observed in this study.

Regarding the Hallett-Mossop process, we expect this could play a role in ice crystal production in the warmer clouds included in this study. Though unlike the Lawson et al. paper mentioned by the reviewer, we are studying Arctic clouds which tend to be in a cleaner environment than the tropics. We expect the dearth of IN found in the Arctic to make Hallett-Mossop, and other secondary ice production mechanisms, a possible control of the ice mass budget in these clouds. This has now been addressed in the updated Discussion section:
Moreover, when riming occurs there is rime splintering, a process that generates small ice crystals when a liquid drop is collected by an existing ice crystal (Hallett and Mossop, 1974). Rime splintering increases the ice crystal number concentration and it occurs more commonly at warmer temperatures (-3 to-8℃). This may help to explain why LWP4 bin clouds have the highest observed levels of IWC, despite these clouds tendency to be warm.

Figure 4. The color scale for reflectivity needs to be adjusted.

Color scale has been altered and there is an updated figure in paper.

Figures 5, 6, 7. I found the figure legends difficult to understand with all of the entries and abbreviations. I would recommend revising the legends to make the figures easier to understand.

Legends have been reformatted.

References:

Hallett, J., and Mossop, S. C.: Production of secondary ice particles during the riming process, Nature, 249, 26-28. Doi:10.1038/249026a0, 1974.

Liu, Y., Shupe, M. D., Wang, Z., and Mace, G.: Cloud vertical distribution from combined surface and space radar-lidar observations at two Arctic atmospheric observatories, Atmos. Chem. Phys., 17, 5973-5989, doi:10.5194/acp-17-5973-2017, 2017.

Shupe, M. D., Uttal, T., and Matrosov, S. Y.: Arctic Cloud Microphysics Retrievals from Surface-Based Remote Sensors at SHEBA, J. Applied Meteorology, 44, 1544-1562, doi:10.1175/JAM2297.1, 2005.