Author Responses to Anonymous Reviewer #2

Summary

This work using ACLOUD aircraft observations studies BL Arctic cloud microphysics profiles based on various in-situ probes. Overall, this kind of studies is needed to improve NWP model simulations and predictions. Authors looked into BL mixed phase clouds over various surfaces and found out that Ni is about less than 35 L-1 and IWC was between 0.003 and 0.08 g m-3. Cloud top T was measured between -3.8 and -8.7C and likely resulted in Ice Multip process but this finding was speculation and no indicators or metrics are given. Not clear, why ice phase had minor impact on transmissivity? Cloud phase as stated is critical for RTM and simulations but this study did not show any RID or even RHw where the supercooled droplets or mixed phase clouds exist.
In addition, intro is found to be very weak, and only certain people work indicated rather than broader knowledge supposed to be given and referenced.
No proper discussion section is provided, and conclusions are mostly speculative.
Based on the above points and issues provided below, I suggest major corrections for this study.
We acknowledge the general comment from the Reviewer. Our study found relative high ice crystal concentration (>10 L-1) in relatively warm single layer stratocumulus clouds with cloud top temperatures above -9°C. Even without simultaneous measurements of ice nucleating particles (INPs), with the current knowledge of INP concentrations it is safe to assume that these ice crystals likely resulted in ice multiplication processes. However, to make this point more clear we have added two references of INP measurements when discussing SIP.

The ice phase optical properties were found to have an insignificant effect on the radiative transfer in our simulations where the total extinction was kept fixed. Removing the ice phase would have likely led to increase in liquid phase extinction through increase in droplet number. Therefore our simulations should be considered as sensitivity study on the effect of the choice of the ice asymmetry parameter to radiative transfer. We have improved the discussion and interpretation of the results from the radiative transfer simulations to make this point more clear.

As suggested, we have improved the introduction by adding references to works, where the vertical structure of Arctic mixed-phase clouds was investigated, and added a "discussion and conclusions" section. The current conclusions section was renamed as summary section. More detailed description of the changes can be found below in our replies for the major and minor comments.

Major/minor issues:

Line 22; specifically please see Gultepe et al BL clouds/ice fog work for Arctic clouds, this work is directly related to your work here.
The following references to Gultepe et al. were added to the Introduction:

Gultepe, I., Isaac, G., Hudak, D., Nissen, R., & Strapp, J. W. (2000). Dynamical and microphysical characteristics of Arctic clouds during BASE. Journal of Climate, 13(7), 1225–1254.

Gultepe, I., Isaac, G. A., & Cober, S. G. (2001). Ice crystal number concentration versus temperature for climate studies. International Journal of Climatology, 21(10), 1281–1302.

*Gultepe, I., A. J. Heymsfield, P. R. Field, and D. Axisa, 2017: Ice-Phase Precipitation. Meteor. Monogr., **58**, 6.1–6.36.*

Ln35-40; see Ni-T for ice clouds based on various studies, and Gultepe et al showed that no trend in Ni-T (Intern J. of Climate)
This reference was added. We also added a sentence summarising these observations: "Gultepe et al. (2001) summarised ice crystal number concentration observations from two Arctic campaigns with maximum dimensions greater than 125 µm. The observed average number

concentrations measured over a wide temperature range from 0 and -45°C varied between 0.3 and 6.4 L^{-1}."

Ln50; see Gultepe et al for SIP and ice crystal splintering issues (AMS met Monographs2018; Arctic ice cloud studies)
We added this reference.

Ln58; provide a ref for Ice fog/BL clouds high Ni values found in Arctic clouds, Lawson et al and Gultepe et al (Atmos Res review, 2017)
We want to remind that in our literature study we summarise in-situ measurements covering only Arctic mixed-phase clouds, where the vertical profiles of low-level clouds were investigated. As mentioned in previous responses, we added reference to Gultepe et al. (2000) and Gultepe et al. (2001). We also added the reference Lawson et al. (2001), and added the following statement: "On the contrary, Lawson et al. (2001) reported extremely high ice particle number concentrations (exceeding 1000 L^{-1}) for another cloud system measured during the same campaign observed at -12°C, and thus not explainable by rime-splintering."

Ln68; why suddenly g became important here?
The asymmetry parameter is a key single-scattering property for radiative transfer calculations but it is not necessary to highlight it in this part of the Introduction. We modified the sentence as following:
"We also discuss the the vertical variability of liquid and ice phase optical properties and discuss the implications of ice phase for the radiative properties of the low level clouds."

Ln116; why cip not used for 15 micron bins? See Gultepe et al grey probe used for BL ice clouds and drizzle.
We use CIP only for retrieving ice phase microphysical properties and for this, we only use particles with D>200 µm, due to uncertainties related to detecting asphericity and uncertainties related to sensitive volume of smaller particles.

Ln152; why assumed as spheres? See Gultepe et al Ice fog review in Atmos Res 2017
The SID-3 instrument measures the differential scattering cross section at the 50° scattering angle with a detector having a half-angle of 9.28°. This measured intensity is then calibrated using spherical particles so that the resulting diameter is the equivalent diameter of a sphere that would have the same differential scattering cross section. There is not an established way how to derive IWC from single particle light scattering measurements. Here we made the assumption to use the most simplified model for the ice crystal shape, i.e. a sphere, since the IWC in the measured mixed-phase clouds is mainly dominated by particles in the CIP measurement range (D>200 µm). The IWC in the SID-3 measurement range contributed typically less than 1% to the total IWC, so any uncertainty caused by the shape assumption would not significantly affect the total IWC.

Eq. 4; what is Qeff? And what wavelength considered here? (See AMS Bull Arctic Ice Fog campaign)
We are not sure what the Reviewer means by Q_{eff}. The Mie calculations to derive the scattering cross section were performed for the wavelength of 532 nm. The text was modified as following: "The extinction coefficient for ***visible*** *wavelengths was calculated using the following equation … For spherical particles in the SID-3 size range, the $\sigma_{ext}(D)$ was calculated by multiplying the geometrical cross section with the extinction efficiency (Q_{ext}) calculated using the Mie theory for 532 nm."*

Fig. 2; where is RHw? If not saturated why we see SIP particles?
The RH corresponds to RHw. This was made clearer in the figure caption. The measured RHw does show unrealistic low values. Therefore we also added a clarification to the figure caption: "Note that the absolute value of relative humidity is not considered to be reliable and the values in panel B should only be considered to represent the trend in the relative humidity."
Since the reported cases are all mixed-phase clouds, we assume that the RHw = 100%, in which case RH_ice > 100%.

Why not show RID?
If the Reviewer means Rosemount icing detector, the instrument was not installed on P6.
Same fig; you cant just plot wind speed like this, see Gultepe et al 1990 and 1995 Arctic cirrus studies.
It is not clear what change to the presentation of the wind speed the Reviewer is suggesting. Fig. 2C shows statistical properties of vertical wind as a function of height. It is not informative to show the mean of the vertical wind as this fluctuates around 0. Variance gives information on the magnitude of the vertical wind fluctuations and thus give an indication of turbulence strength.

Fig. 3; how did you filetered data to get solid lines???? From what? Aircraft?
The solid lined show the average 10-s aircraft observations for each height bin. This is made now more clear in each figure caption.

Fig 5; how did you removed the snow from the analysis? Snow versus cloud?
We did not remove precipitation particles from the habit analysis. This is mentioned in the figure caption.

Fig 6; same issue?
How the solid lines were calculated was made clearer in the figure caption: "The mean and standard deviation were calculated based on 10-s aircraft observations for normalised altitude bins having bin edges at -0.6, -0.3, 0, 0.25, 0.45, 0.65, 0.85, 1, 1.05."

Ln280; Gultepe et al (Atmos Res review, Arctic ice particles were not irregular), they showed it clearly also.
This reference refers to Arctic ice fog studies of Gultepe et al. (2014) and Kim et al. (2014) and their findings of pristine crystals. We, however, refer to studies in mixed-phase clouds, which are more comparable to our results as the formation of ice crystals in ice fog and mixed-phase environments can be different.

Fig 09; how did you interprete data? These are just selected ones, explain it.
These are selected example crystals. The figure caption was revised as following:
"Examples of ice crystals representing three categories that were frequently observed on 2 June: large (D>500 μm) needles and columns, smaller unrimed faceted crystals and other polycrystals. Images are from the PHIPS probe."

Fig 12; why you normalize the data??? Show T also, o idea what is T?
The data is now shown as a function of actual altitude. The temperature at the cloud top is given in the text, which we consider to be sufficient, as the cloud systems were rather shallow.

Fig 13; a has some relationship but not others, why?
In the revised version, we are not discussing the trends in LWP, only in IWP.

Section 6; not clear and results are not meaningful to me, please discuss and compare with others.
Section 6 (now 5) was edited to improve its clarity. Discussion of our results in comparison with literature is not possible to our knowledge, as we are not aware of studies that investigate the sensitivity of radiative transfer in low-level mixed-phase clouds to ice crystal asymmetry parameter.

Parag 425/conclusions; very well known statements, and I see that conclusions need to be improved and referenced for the knowledge given in the paper.
We renamed the section as summary section. A detailed, referenced, discussion of the statements presented in summary can be found in the added discussion section.

Finally, how did you discriminate SIP from the prime ice crystals? What is the basis?
SIP was identified in the basis of knowledge of INP concentrations at T=-10°C, which are estimated to be well below 10^{-2} L^{-1}. Since the observed ice crystal concentrations were several order of magnitude larger than the INP estimate, we can assume with high confidence that the

observed ice crystals were the result of ice multiplication. This was made more clear in the discussion section:
"Previous observations in the Ny Ålesund have shown that springtime INP concentrations at -7°C are on average below 10^{-3} L^{-1} with 95-percentile being below 10^{-2} L^{-1} (Li et al., 2022). This is in consensus with the general knowledge of INP concentrations in this temperature range (Kanji et al., 2017). Therefore, it is highly likely that the observed ice crystal concentration in all of the presented warm and neutral period cases were results of ice multiplication."

I suggest major revisions for this work, and see it again.