Review of "Observations and Comparisons of Cloud Microphysical Properties in Spring and Summertime Arctic Stratocumulus during the ACCACIA campaign." By Lloyd et al.

This paper details observations from a recent field experiment where aircraft sampled mixed phase clouds and aerosol properties in the vicinity of Svalbard. Overall, I think the results could be an important contribution to research in arctic mixed phase cloud microphysics, but some extensive revisions to the paper are needed before I would determine it to be fit for publication in ACP. In particular, I think the introduction does not pay enough attention to some studies regarding aerosol indirect effects with regards to mixed phase clouds, and I think explaining their results in the context of these studies would be of great benefit to the paper. Furthermore, the paper goes into gory detail about 4 different flights, listing off many data points that do not have a whole lot of relevance to the paper's main arguments as a whole, particularly in Sections 3 to 7 where many of the details can be cut out and either integrated into the discussion section. If an integration is not desired, then these points could be more eloquently expressed as a figure as I will show in the comments. The paper is also quite wordy, and I highly urge the authors to make the paper more concise. There is also a fundamental problem with quoting 1 Hz values of ice concentrations in that the sample statistics may be inadequate given the relatively low number of ice particles sampled over 60-100 m by the probes, so the given 0.1 Hz observations are more appropriate for use. Furthermore, the conclusion section lacks any details about what is recommended for future studies, which should be noted. Detailed comments about each section are listed below.

Section 1: A much greater amount of detail is necessary in your description of how CCN and IN can affect cloud properties. In particular, there are three different hypotheses listed by Lohmann and Feichter (2005) and in Figure 1 of Jackson et al. (2012) for how CCN and IN affect mixed phase cloud properties:

1. The thermodynamic indirect effect hypothesizes that increasing CCN leads to a decrease in droplet sizes. This decrease in droplet sizes decreases the number of drizzle drops necessary for rime-splintering to occur and hence leads to a reduction in the number of ice crystals due to suppression of secondary ice production. (Rangno and Hobbs 2001)
2. The glaciation indirect effect states that an increase in IN leads to an increase in the number of ice crystals (Lohmann et al. 2001).
3. The riming indirect effect states that increasing CCN decreases the droplet size and hence inhibits growth of ice crystals via riming, decreasing the IWC. (Borys et al. 2003)

You should mention the Lance et al. (2011) and Jackson et al. (2012) papers looking at ARCTAS and ISDAC as well. The comparisons made in the paper should also be discussed in terms of these three hypotheses and what the relative impact of each effect is for the case you are presenting.

Lines 25-29, page 28760: These lines are not referenced, although probably are not needed either since you have already demonstrated that single and multi-layer mixed phase clouds exist and have a wide variation in properties.

Objective 2: Why compare your ice concentrations against the DeMott parameterization? I don't think this was adequately explained in the introduction.

Lines 7, page 28762: Why weren't the other cases selected? Surely they have some variability in aerosol loadings that can be examined. Since the overall goal is to select two cases that have a comparable meteorological setup and surface conditions with different aerosol loadings, the selection of these two cases needs to be better justified in terms of the meteorological and surface conditions as well as the aerosol loadings. It may do some good to present the synoptic conditions that formed these clouds as well as to mention whether the clouds were over land, ice, or open water since these factors can play a role in determining the microphysical properties.

Line 16-20, page 28763: I would suggest removing these two sentences since these probes are not used in the paper.

Line 9-11, page 28764: Remove, since you mention this later.

Line 12-17, page 28764: I don't think you mention the size ranges where you use the CIP-100 in place of the 2DS data. For what size ranges do you use the CIP-100 and 2DS? The resolution of the CIP-15 and the 2DS probes is comparable, and the response time should only affect the sampling of the smallest particles, so a comparison of the CIP-15 and 2D-S concentrations in their overlapping size ranges is needed in order to justify the choices of probes for each size range and to provide the reader an idea of how different the measurements from the differing probes are.

Line 19-20, page 28764: You need to justify why you are using the Brown and Francis (1996) relationship here. Since the appropriate relationship depends on particle habit, you need to justify your choice based on the particle habits that were observed. Many studies use an automated habit identification scheme to determine what percentage of particles in a given size range are of a particular habit and then calculate the total mass of particles in a habit category. The final IWC is then the sum of the mass of particles over all categories. Another method that takes particle habit into account is in Baker and Lawson (2006). In any case, further justification of your choice of m-D relationship is required.

Line 121, 28764: Probably should cite Korolev et al. (2013).

Line 9, 28765: Could you define "majority" 50%, 80%?

Lines 10-215, 28765: You do not need to mention this here.

Line 17-18, 28765: Was there a Continuous Flow Diffusion Chamber or similar instrument to directly measure IN? I think you need to mention that the parameterization is used in place of direct measurements of IN direct measurements if they are not available.

Line 220-24, 28765: What relative humidity thresholds were used? Plus, shattering of ice crystals on the sample tubes/inlets could potentially contaminate PCASP+CAS measurements at the large end of the size range. Did you take care to not include concentrations in time periods where there were ice crystals present in the 2DS/CIP data to help reduce this contamination? Furthermore, how were the PCASP and CAS measurements combined together?

Sections 3 to 6 and appendices: These sections give an extensive list of small details of several flights that do not add much to the overarching conclusions of the paper. I recommend that either this section be condensed to only mention the overall structure of the cases encountered, or that the details needed from this section to support your conclusions be mentioned in the discussion. It may even help to simply create figures that give an approximate picture of the cloud, like for example, Figure 9 of Jackson et al. (2012) (below) in place of the 4 time series figures. This would be easier for the reader to interpret. This would greatly reduce the number of words in the section and make the overall microphysical picture clearer. There are just too many small, insignificant details stated for me to try and see what the overall picture of each case is.

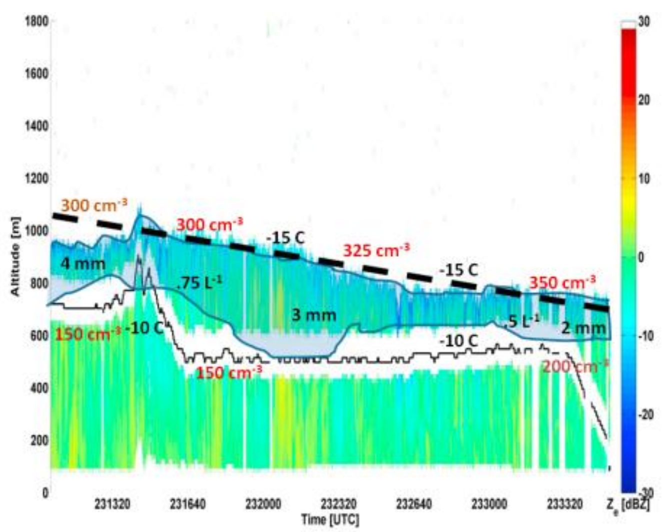

Figure 9. Vertical cross section of Z_e from the W-band radar for a cloud deck observed on the second flight of April 8. The blue shaded regions denote the approximate location of the liquid layer derived from the in situ profiles of *LWC*. Maroon values denote PCASP concentration measured above and below cloud, black values in mm are median mass diameter of ice crystals, and values in L^{-1} denote $N_{ice}(D > 50\ \mu m)$. Values in °C denote temperature. The solid black line denotes flight track altitude. The dashed black line denotes the approximate location of the temperature inversion.

Line 14-18: I think it would be better to state the variation in predicted IN in your Table rather than what Grosvenor et al. (2012) stated.

Line 8, 28777: New paragraph.

Line 22-23, 28777: These rapid fluctuations can also be due to noise from inadequate sampling statistics. In particular, for your larger dendrites, there may only be 4 or less dendrites being sampled per second, which makes this sampling error to be 1/sqrt(4) = 50% just due to the low number of particles being sampled. You should really be quoting the 0.1 Hz observations when talking about variability in cloud properties for this reason, as the uncertainty due to sampling statistics is likely to be a lot less when the averaging interval is increased.

Paragraph at line 25, 28779: This discussion needs to be expanded factoring in the relative impact of the three aerosol indirect effects in determining the microphysical properties of these clouds. The same follows for the following paragraph comparing your observations against the Grosvenor study.