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)

These three hypothesis have been stated in the introduction *(lines 60-69)* and discussed in relation to our work in the discussion (*line 483-488;600-603;615-617*). We didn't find evidence that increased CCN was leading to a suppression of secondary ice production. However comparing spring case 1 and 2 (low and high aerosol loadings respectively) there is support for the riming indirect effect. In case 1 IWC values were higher than in the second spring case (approximately a factor of 2 or 3).

Although we didn't make direct IN measurements we infer that ice number concentrations in both Antarctic and Arctic clouds outside the HM temperature zone were controlled by primary heterogeneous ice nucleation. Concentrations were lower in the Antarctic when compared to the Arctic and this is likely to be a manifestation of the glaciation indirect effect where increased IN availability in the Arctic has led to higher concentrations of ice here when compared to the Antarctic.

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.

These papers have now been cited and discussed in the paper *(lines 70-91)*

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.

The lines refer to work discussed in the Verlinde et al. (2007) paper, however I've removed these lines as suggeted.

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

The aim is to compare predicted ice nuclei concentrations in these clouds with in-situ measurements from the microphysics probes used in this study. Primary ice nucleation parameterisations are an important aspect of cloud modelling and we think it's useful to compare these with in-situ observations of cloud ice concentrations. A paragraph has been added in the introduction to describe this. *(lines 94-99)*

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.

The two spring cases represented this variability in aerosol loadings and were selected to see if this impacted on the cloud microphysics. The rational for selecting each case is described in the manuscript *(pages 150-159).* One case had much higher concentrations compared to the other, and the most notable impact this had on cloud properties involved the liquid phase, with no significant changes in the ice phase between the two cases. Presumably the aerosol in the increased loadings case were not IN active, or at least not IN active in the temperature range these clouds spanned.

The summer cases were selected specifically to address the impact of secondary ice production on the cloud layers. Other cases were found to be less conducive for secondary ice production through rime-splintering due to the temperature of the cloud layers.

Spring case one and two took place mainly over ocean and mainly over the ice or marginal ice zone respectively. The summer cases were conducted over the ocean. Although the aims of the flight were to fly over ice and over water the eventual outcome was actually that the surface below was generally similar for each case (either over water or over ice). For this reason the paper does not aim to address the differences in microphysical structure depending on whether the clouds are over the ice or over the ocean. In the case introductions I've removed the actual aims of the flight and described only what was carried out as this can be confusing.

Referee 2 also requested more detail about the synoptic conditions, we have added more detail about this at the beginning of each case study.

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

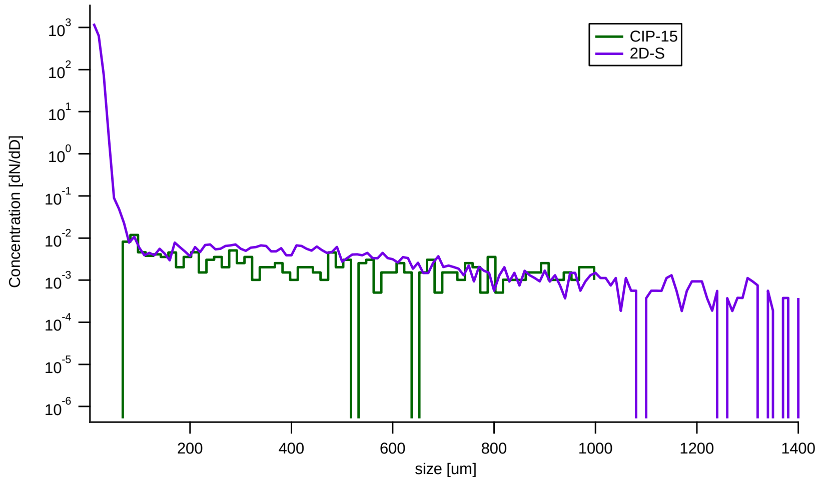
These lines have been removed.

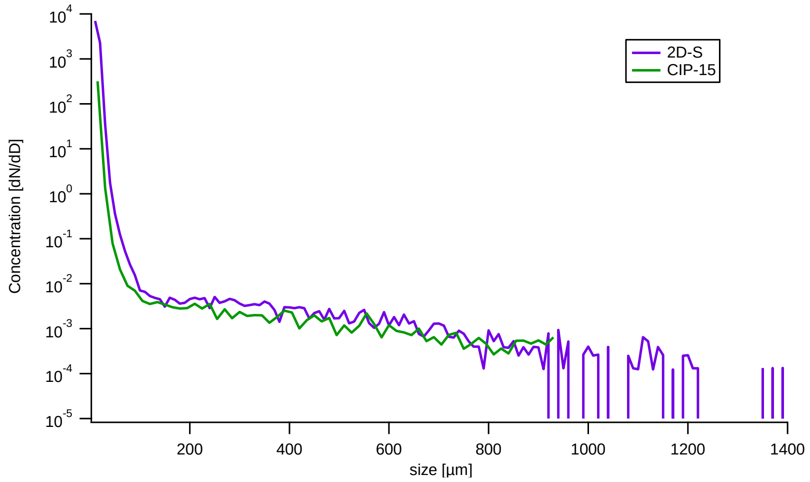
Line 9-11, page 28764: Remove, since you mention this later.

This has been removed.

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.

We had the ability to compare the 2D-S and CIP-15 instruments during the spring only, and found good agreement in their size distributions. We haven't included a new figure in the paper showing this but have added text to state this. *(lines 203-204)* We also include an example figure in this response (below) showing the comparison of the two instruments for a period during Spring Case 1 and Spring Case 2 respectively.





In the spring cases we used the 2D-S to 1050 microns and then extended this range using the CIP-100 (upto 6200 microns) to capture the larger particles that could contribute significantly to the ice water content.

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.

Brown and Francis is still widely used in the literature to estimate ice water mass in mixed phase clouds eg Crosier et al (2011). Other studies such as Baker and Lawson referred to be the referee have found discrepancies between their treatments of the data and Brown and Francis when crystals are large and have low aspect ratio with relatively good agreement for smaller crystals with larger aspect ratio. In most of the clouds studied where the ice water mass is large it is dominated by crystals smaller than 100 µm by particles with a high aspect ratio in which good agreement is found between Brown and Francis compared to Baker and Lawson. In view of the crystal habits and size observed in this work and for consistency with previous studies we have used Brown and Francis.

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

This citation has been added to the text.

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

The IAT thresholds were chosen by looking at the IAT histograms for different regions of microphysics. The majority means that the selected IAT threshold value would likely remove the vast majority of shattered particles as the shattering mode was well separated from the mode of good particles centred at higher IAT time values.

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

This section has been removed.

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.

Direct IN measurements were not made, and information about this has been included and explain the use of DeMott et al. (2010). (*lines 94-99 and line 254*)

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?

The aerosol was measured during out of cloud periods containing no hydrometeors together with suitably low RH values. The maximum RH values for each measurement period are given in Table 3. The PCASP and CAS measurements were used independently for input into the ice nucleation scheme.

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.

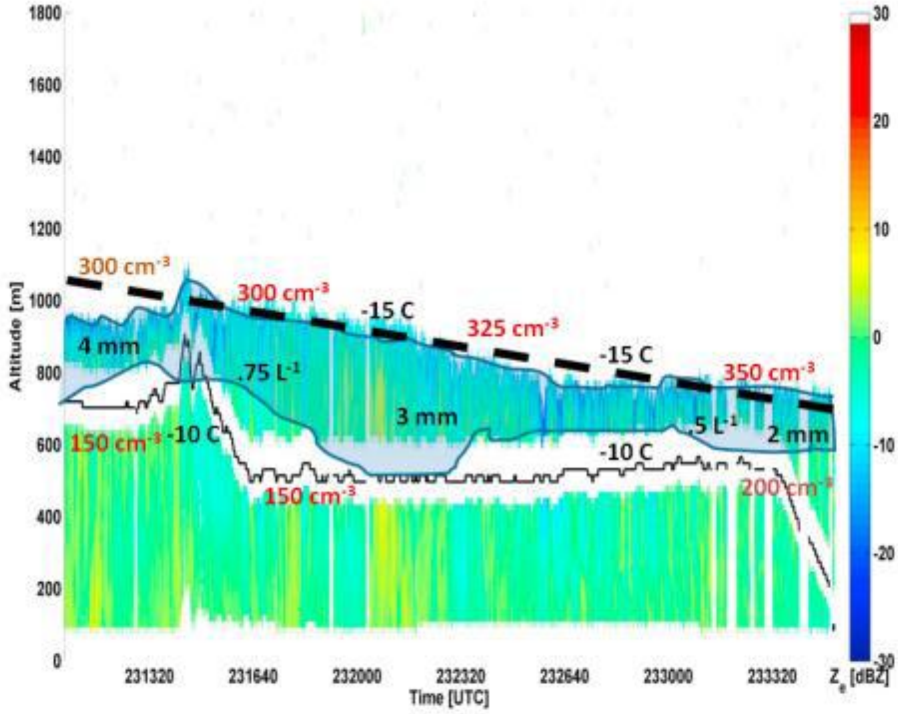


Figure 9. Vertical cross section of Z_e from the W-band radar for a cloud deck observed on the second flight of April 8. The blue shaded regions denote the approximate location of the liquid layer derived from the in situ profiles of *LWC*. Maroon values denote PCASP concentration measured above and 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.

These sections have been made more concise, but we feel some description of the microphysical structure during a single profile is useful. The beginning of each case now includes a description of the overall structure of the stratocumulus cloud layers, in an attempt to make it much clearer to the reader. The sections describing the microphysics have also been shortened where possible, with the detail about measurements from each probe (e.g LWC, IWC, *Nice, Ndrop).*

We will remove the further profile descriptions from the Appendix and include these in the supplementary material.

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.

We have calculated the uncertainty in the Grosvenor IN predictions for regions not influenced by secondary and included them in the table.

Line 8, 28777: New paragraph.

New paragraph inserted.

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.

The number of peak value figures has been reduced, but the sampling error is likely to be acceptable for the regions of secondary ice production where counts are higher, so some of these have been kept. The lines here also refer to transitions from one state to the other, for example predominantly liquid conditions very quickly replaced by glaciated cloud due to the HM process. This is distinct from repeated fluctuations in the 1Hz data that may be subject to significant error due to poor counting statistics.

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.

The importance of each hypothesis has now been included in the discussion section. (*line 483-488;600-603;615-617*). We have also added a new conclusion based on the possibility that the riming indirect effect played a role in reducing ice water contents in the spring case with higher aerosol loadings.

Interactive comment on "Observations and comparisons of cloud microphysical properties in spring and summertime Arctic stratocumulus during the ACCACIA campaign" by G. Lloyd et al.

Anonymous Referee #2

Received and published: 5 January 2015

This paper reports on some interesting microphysical observations from a set of flights during spring and summer through arctic stratocumulus near Svalbard. The authors point out that few in-situ measurements of ice and aerosol have been made in arctic stratocumulus and this is still largely true. However, the measurements that have been made over the years are tending to converge (see Morrison et al., 2011, Nature Geo-science). The authors note substantial seasonal differences in the microphysical, and glaciation, of mixed-phase arctic clouds. The observed summertime clouds appear to be more heterogeneous with pockets of ice formed apparently by rime splintering. Spring-time clouds generally had lower ice concentrations than summer. Comparisons of the observed ice concentrations with predictions using the Demott et al. (2010, PNAS) were also discussed in the paper. I found the paper easy to read and the observations are quite interesting.

While I generally find the paper to be a useful contribution to the literature on the measured microphysical properties of arctic mixed-phase stratocumulus, I also think that the paper is missing some elements, I list them below.

(1) I think the paper needs a section that provides some meteorological context for the cloud cases and the observations. Since the larger scale synoptic flow can set the stage for a given

microphysical response of the cloud system to aerosol/IN, providing an overview of the general flow along with the vertical thermal and moisture structure would be very helpful.

We have added or improved upon the description of the synoptic conditions at the start of each case description. This aims to provide some context to the large-scale forcing in the region. We have looked at the vertical thermal and moisture structure. Fig. 11 for example shows the temperature profile of the atmosphere measured by the aircraft. When looking at dew points these showed a marked dry layers above the cloud in the inversion layer. We haven't presented this in any new figure. We have mentioned this dry layer in relation to dew point measurements *(lines 491-494)*

(2) The authors do a very nice job of comparing their results to results from an Antarctic study. I think the paper would be enriched if the authors could cast their results in the context of the other papers published on ice concentrations/IN in arctic clouds. For in-stance, Rangno and Hobbs published a paper in 2001 (J. Geophys. Res., pg 15,065) in which they also discuss the importance of rime-splintering for high ice concentrations in arctic mixed-phase stratocumulus. In addition to pointing out that there is no clear temperature dependence to ice concentrations in arctic clouds, Rangno and Hobbs also indicated that a possible threshold droplet size exists that relates to maximum ice concentration. Do your observations show similar results? Other articles have dis-cussed ice concentrations and the vertical thermal structure of the atmosphere (Curry et al., 1997, JGR; Pinto, 1998, JAS; Rogers et al., 2001; JGR; Prenni et al., 2007; etc.); results from these papers may help place your results into a broader context.

Although we haven't done habit classification on our 2D-S dataset from looking at the images we generally observed that columnar crystals dominated the imagery, despite the presence of some less pristine ice that could simply be described as irregular. For this reason we believe the enhanced concentrations in the spring cases was very likely due to secondary ice production through rime-splintering. In the manuscript the presence of temperature inversions has been discussed, as this is a common finding at the top of stratocumulus cloud layers in this region. During the spring cases these inversions were stronger and interestingly the cloud penetrated some distance into the inversion layer.

We have added a paragraph discussing the Rangno and Hobbs (2001) and the relevance of their work to our results. (*lines 586-598)*

Rogers et al. (2001) found similar ice concentrations and evidence for a few IN in stratus clouds they studied. Their findings are consistent with the cases presented in this paper. A sentence has been added to describe this in the discussion. *(lines 581-583)*

(3) As I understand it, the IN parameterization of Demott provides an estimate of the local (in space) ice concentration based on temperature and the number of aerosol beyond a certain size. However, the ice concentration measured in clouds is a conse-quence of not only local ice nucleation processes, but also of convergence and diver- gence due to vertical sedimentation and advection. Since not all ice particles grow at the same rate, one might

imagine larger ice particles, for example, sedimenting away from a nucleation zone and therefore leading to a lower measured ice concentration. I wonder if these sorts of effects are important or if they are negligible.

These processes can change the concentrations of the crystals observed. We have noted this in paper. However, the range of crystal concentrations observed can be explained by the uncertainty in the DeMott parameterisation discussed below.

(4) In Demott's paper, the observed data are quite scattered about the 1:1 line in com-parison to the parameterization. For your observed cases, does the scatter in the points shown in Fig.3b cover the range of your observed ice concentrations? For instance, your case 1c produces IN concentrations of 1.24 or 2.05 but the scatter in Demott's Fig. 3b indicate that observed IN concentrations at these predicted values can be up to 10 per liter or as low as a few tenths per liter. I'm primarily curious about this because if the ice concentrations sit within the range of scatter Demott shows, it might provide a small amount of evidence that IN could have been responsible for the ice. (Whereas in your rime-splintering observations, this is clearly not the case.)

A section has been added to discuss the variation in the D10 parameterisation and we find that the spread in our ice concentrations is within the variability of the points in fig. 3b of the DeMott et al. (2010) paper.

Interactive comment on "Observations and comparisons of cloud microphysical properties in spring and summertime Arctic stratocumulus during the ACCACIA campaign" by G. Lloyd et al.

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The affiliation for A. Kirchgaessner and T. Lachlan-Cope is not correct. They both are affiliated with the British Antarctic Survey, NERC, High Cross, Madingley Rd, Cambridge CB3 0ET, UK. Thanks.

This affiliation has now been added.