Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-283-AC1, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Summertime Arctic Aircraft Measurements during ACCACIA" *by* Hazel M. Jones et al.

Hazel M. Jones et al.

hazel.jones@manchester.ac.uk

Received and published: 14 June 2018

[This document is laid out in the following format: original reviewer's comment, authors' responses, any changes that have been made to the text.]

Response to Anonymous Referee #1 (Received and published: 17 April 2018)

Comment: This paper describes airborne observations of clouds and aerosol particles made during July, 2013 as part of the Aerosol-Cloud Coupling And Climate Interactions in the Arctic (ACCACIA) campaign. The observations were conducted from a BAS Twin Otter, based out of Longyearbyen, Svalbard. The instrumentation emphasises cloud microphysics a little more than aerosol microphysics. The paper is an overview of observations from eight science flights. The results presented in this paper will likely

Printer-friendly version

Discussion paper

be of value to the modelling community in terms of data for evaluation. It may be able to offer some useful scientific perspectives, but currently it reads more like a project summary or narrative of a field study.

Response: We thank the reviewer for appreciating the value of the data presented to the modelling community. We have tried to reduce the project summary-like nature of the paper in a number of places. Firstly, we have clarified the objectives of the paper by adding text to the introduction section. Secondly, we have made amendments elsewhere in a number other places (see list below) to focus the text towards these aims and some of these changes are also listed in the responses below. The nature of this paper is based on the range of measurements available to present. The flight tracks were varied, and sampled different regions, cloud types, and altitudes, making it difficult to summarise the final results from all flights in a simple conclusion. However, we report results and make suggestions as to why differences occur between cases; such as, between flights M191 and M193.

Text changes:

End of paragraph 7 of Introduction section: "This paper aims to give an overview of the cloud microphysics and aerosol data collected during the eight science flights of the summer campaign."

Has been changed to: "The purpose of this paper is to make the full cloud microphysics and aerosol data set available to the modelling community but also to present evidence that these Arctic layer clouds consist predominantly of supercooled water with occasional glaciated regions. In these glaciated regions we discuss the potential origins of the ice phase and the role of secondary ice production in generating the more strongly glaciated regions."

Comment: It is a mix of many observations that needs more focus. I suggest emphasis on two aspects: 1) ice processes, including INP, and 2) connections of the aerosol observations with the cloud droplet number concentrations where feasible.

CC BY

Response: We agree with the reviewer regarding the requirement for the presentation of the observations to have more focus (see above response about aims). However, the range of measurements available to present were obtained from flights with a variety of objectives, flying in different regions, altitudes and conditions. Clouds were also often measured on an opportunistic basis when present, making it difficult to separate out the final results from all flights in precisely the way suggested. Instead, we have highlighted the important aspects of the cloud structure and microphysics of the clouds that were sampled that are not readily identifiable from simple time series, allowing easier assimilation by modellers.

The purpose of this paper is mainly to inform the modelling community that this dataset as in-situ cloud measurements in this region are still sparse. Furthermore, we illustrate just how complex the Arctic atmosphere can be, with different cloud properties measured in the same region at similar conditions. In contrast, we show that some consistent characteristics are observed; for example, the lifting and deepening of the cloud layers over the transition from sea-ice to ocean. These data can be used by the modelling community for model validation and, as such, we highlight microphysical differences between cases, where possible, which would provide good case studies for model evaluation and development; for example, the differences between the multi-layered stratocumulus clouds measured during M191 and M193.

Specific comments: 1. Table 1 and Figure 1 are missing the Canadian NETCARE aerosol and cloud observations conducted near Resolute Bay in the Canadian Arctic archipelago during July, 2014, as described by Leaitch et al. (ACP, 2016).

Response: We agree that there is value in including NETCARE project results in Table 1 and Figure 1, so this has now been done. Additional remarks have been added to the discussion section to reflect this.

Text changes:

Section 4.4, paragraph 3 now includes: "....Burkart et al. (2017) showed similar results

Printer-friendly version

Discussion paper

in the Canadian Arctic during the NETCARE project, whilst Croft et al (2016) also reported ammonia from seabird colonies as a source of aerosol particles. We cannot. . .”

To section 4.1, para 6, the following sentence has been added: “Leaitch et al. (2016), reported a summertime cloud droplet concentration range of 16 – 160 cm-3 for cloud sampled during NETCARE over the Canadian Arctic Archipelago.”

References have been added to the reference list.

2. Section 4.3 discusses the switch from mostly liquid to mostly ice, but fails to refer to Figure 7. According to Figure 7, M191 exhibited more glaciation than M193, yet the 2DS ice concentrations given in Table 4 are about two times higher for M191 than M193. Please discuss these differences. Are you suggesting ice multiplication for M191, M193 or both?

Response: Clouds sampled during both M191 and M193 contained some areas of mixed phase or fully glaciated cloud. Figure 7 shows that for M191 there were more in-cloud data points where almost 100% of the condensed water mass was present as ice than in M193 (please note the log scale on the y-axis), not that more ice was seen in total. In fact, there is only one very short period during M193 when the cloud was almost totally glaciated. Table 4, on the other hand, summarises microphysical parameters for the whole flights, including mean 2DS ice crystal number concentrations were higher for, M193 than M191. This is not inconsistent, since Fig. 7 relates to relative Ice mass and Table 4 presents average crystal numbers. Ice mass fractions (as often used in models), are related not only to ice crystal numbers but also their form/habit etc. The discussion in 4.3 refers to number concentrations of particles changing from being almost 100% droplets in number to being 100% ice crystals. However, a reference to Fig.7 is now included for completeness (see below). We are suggesting that it is possible that some ice multiplication processes are active within regions of cloud of almost 100% ice crystals that are columnar in form, and we are sampling within the H-M Secondary Ice Production (SIP) active temperature range. Final determination of

CC BY

this SIP is by comparison of crystal number concentrations with expected INP numbers (e.g. as from an extrapolation of the parameterisation of DeMott et al., 2010).

Text changes:

Section 4.3, paragraph 1 has been changed to: "When ice was detected by the 2D-S during the summer ACCACIA campaign, for the majority of the time, it was within a mixed-phase region where the cloud consisted of almost 100% supercooled water droplets. However, on occasion, it was noted that the phase of the cloud particles rapidly switched from being almost 100% droplets to almost 100% ice particles. Figure 7 shows that there were more in-cloud data points where almost 100% ice was noted compared to M193. When this occurred, the ice crystal habit was predominantly columns, see Fig 8a, typical of that temperature range, but in numbers enhanced above expected ice nucleation particle (INP) concentrations predicted using an extrapolation of the DeMott et al. (2010) parameterisation (the highest cloud temperature sampled for the parameterisation was -9 °C, and DeMott et al. (2010) advise further development in the temperature regime > -15 °C). These enhanced numbers are indicative of secondary ice production, likely through the Hallett-Mossop process as previously reported in Lloyd et al. 2011."

3. Section 4.4 is unable to say anything about INP sources. It brings up the observations from the Grimm OPC, but the discussion is brief and qualitative. Why is there no attempt to correlate number concentrations from the Grimm with the 2DS 'ice' cns? A comparison of Tables 3 and 4 suggests there may be some association. Whether there is or not, it would offer more information and something with a little more rigour than the current presentation. It could be linked to DeMott et al. (PNAS, 2010).

Response: It was an aim of the project to link the below cloud aerosol number concentrations to the cloud microphysics; however, due to the difficulties with Arctic flying, the data available from the flight tracks does not allow this to be investigated fully. There are only two flights when the aerosol and cloud sampling were co-located, and only

CC BY

one of these flights detected ice particles, meaning that we cannot draw conclusions about any correlations between aerosol numbers and cloud microphysical parameters. Temperatures sampled are above the range over which the DeMott et al. (2010) parameterisation is valid. Therefore, to compare these aerosol and cloud results in this way would be an extrapolation of the DeMott et al. (2010) parameterisation to warmer temperatures than it covers. We will include a sentence in the manuscript to make our reasoning clear for not making this comparison.

Text changes:

The last sentence of section 4.3 now reads: "......, but in numbers enhanced above the expected ice nucleation particle (INP) concentrations predicted using an extrapolation of the DeMott et al. (2010) parameterisation (The highest cloud temperature sampled for the parameterisation was -9 °C, and DeMott et al. (2010) advise further development in the temperature regime > -15 °C). These enhanced numbers are indicative of secondary ice production, likely through the Hallett-Mossop process as previously reported in Lloyd et al. (2015)."

To the end of section 4.4, the following sentence has been added: "A direct comparison between aerosol and cloud measurements cannot be made due to below cloud aerosol sampling runs typically not being complemented with in-cloud sampling of the layer directly above."

To the Table 3 caption the following sentence has been added to the end: "Please note that only M191 (at 350 m) and M198 (at 400 m and 310 m) were co-located with cloud sampling directly above."

4. The second last paragraph of Section 4.4 that discusses CCN and new particle formation should be a separate section that draws connections between the CPC, Grimm and CDP number concentrations. Presently, the aerosol and cloud droplet number concentrations are discussed independently. The aerosol numbers with the standard deviation give us no perspective on the aerosol concentrations that influenced the clouds.

CC BY

Profiles of the CPC and Grimm number concentrations should be included with the CDP number concentrations shown in Figure 10. How is the below-cloud aerosol linked with the cloud? Were you level in cloud long enough to estimate updraft speeds from your gust measurements?

Response: The flight tracks unfortunately do not allow for a direct comparison between cloud and aerosol data; for example, below cloud aerosol sampling runs were not always complemented with in-cloud sampling of the layer directly above. As figure 12 shows, there can be significant environmental changes between different locations, particularly at different latitudes. We therefore did not present the aerosol and cloud information in the manner suggested by the reviewer due to the differences in sampling locations. Profiles of CPC and GRIMM number concentrations cannot be included in figure 10 as the profiles extended to just below and just above the cloud layer, meaning that no out-of-cloud aerosol data (where it is valid) is available for these profiles. Cloud passes for this project were typically during profiles and, as such, we have not presented updraft speeds in this paper. It is hoped that these issues can be rectified in any future planned Arctic flight projects.

5. Define ACCACIA when it is first written.

Response: Adjustment made to abstract

6. Page 1, Line 30 – which, which

Response: Adjustment made

7. Page 1, Line 32

Response: Adjustment made – cited chapter 12 from the IPCC AR5

8. Page 6, line 23 - Canadian Arctic Archipelago rather than Northern Canadian islands.

Response: Adjustment made

CC BY

9. Page 9, lines 16-20 - The comparison between the Grimm and CAS is fine, but what is the purpose of it here?

Response: This finding has implications for other aircraft studies which utilise probes in this way, and highlights the differences in data due to sampling methods. Given the key goals of this paper is to present new measurements of Arctic cloud and aerosol to the modelling community, we feel that it is important to highlight how the sampling method may affect these data and illustrate that care must be taken when comparing such internally- and externally-sampled data.

Text changes:

Section 3.2, paragraph 3, this sentence has been added at the end: "…as Fig. 4 highlights. It is important to consider this when comparing data to previous projects."

10. Page 10, lines 15-16 – Indicate where this is evident - Figure 8?

Response: We have added text to resolve the comment.

Text changes:

The following addition has been made to the text: "Also present were some irregularly shaped ice crystals; all ice particles showed evidence of riming when detected lower in the cloud, as is evident in Fig. 8."

11. Page 10, lines 18-19 – To what do you attribute the difference between M191 and M193 in Figure 7?

Response: As the data we have are limited, the question of why were there more transitions to 100% ice for M191 compared to M193 most likely comes down to the environmental conditions during M193 (e.g. being on the edge of the conditions required for secondary ice processes to occur) and the stage of evolution of the different clouds. The example images included in the paper are from when temperatures were -3 °C (M191) and -3.8 °C (M193) – however, the images for M191 show larger crystals so

CC BY

these were likely falling from higher and colder in the cloud.

Text changes:

In Section 3.3.1, paragraph 3, the following has been added: “Their size suggests they are falling from colder regions higher up in the cloud”

12. Page 10, Lines 20-28 – What is the importance of these details?

Response: As the main goal of this paper is to present this dataset to the modelling community, the presence of stellar crystals is highlighted so that modellers will be aware that it is possible for these types and size of ice crystals to be present in the Arctic atmosphere. In particular, information on cloud habit (and size) is important for modellers as this has the potential to greatly affect the cloud radiative properties.

Text changes:

To Section 3.3.1, Paragraph 6 “The presence of large stellar crystals has the potential to impact on any simulated cloud radiative properties in the Arctic and this information is included so that modellers can be informed on the range of possible arctic ice crystal habits.”

13. Rather than leaving it until the caption of Figure 9, mention in Section 2 that the flights were based out of Longyearbyen.

Response: Changes made as suggested.

Text changes:

The following sentence has been added to the end of the paragraph in section 2.1: “The aircraft was based at Longyearbyen in Svalbard, Norway.”

14. I don’t find the 3D aspect of Figure 9 to be helpful. If all you are trying to say is that “the stellar crystal regions were co-located at different heights”, is not a 2D representation sufficient and clearer?

CC BY

Response: This figure has been replaced by a 2D version as requested.

15. Page 15, line 14 – “INP”

Response: The heading for 4.4 has been extended to read ‘ice nuclei’. INP is now defined in the introduction on page 3, where it first occurs in the main text.

16. Page 16, lines 16-18 - “Cloud droplet diameters. . .” Add why you think the droplets are smaller than for other studies? Is it because of the presence of ice?

Response: The second part of the conclusion point has been removed as it only refers to the Mioche et al. 2017 study that reports ASTAR, POLARCAT and SORPIC and is in reference to the size of particles at cloud base. The original wording is not a true reflection of all Arctic measurements, so to avoid confusion, we have removed this sentence.

17. Page 16, lines 19-20 – What are the potential implications for “no consistent relationship of ice crystal number concentration with altitude”?

Response: These results support previous studies of Arctic mixed-phase stratocumulus layers - i.e. ice particles are spread throughout the cloud layer and are not confined to specific altitudes within the clouds. The placement of ice within a cloud layer will affect the radiative impacts of the cloud layer, and it is therefore important to capture it correctly to produce realistic simulations of these clouds. We present these data in support of previous studies that found a similar vertical distribution of ice particles within Arctic stratocumulus, and provide more data for model evaluation. By showing that there is no clear relationship between ice crystal number concentrations and altitude within the clouds, we can infer that dynamical mixing may be occurring within these clouds or that ice crystals are nucleated at any point within the cloud. Discussion of these implications have been included in the manuscript.

Text changes:

Section 4.1, paragraph 2, the following text: “Ice when present was typically spread

CC BY

throughout the cloud vertically with no altitude maxima."

Has been replaced with: "When ice was present, there was no consistent relationship identified between ice crystal number concentration and altitude. This finding supports previous work on Arctic mixed-phase stratocumulus (e.g. Hobbs and Rangno 1998; Shupe et al., 2006; Verlinde et al., 2007; Shupe et al., 2008; McFarquhar et al., 2011;Young et al. 2016a), suggesting that ice crystals may be being mixed throughout the cloud by dynamical processes or that the ice crystals are nucleating homogeneously throughout the depth of the cloud layer. The placement of ice within a cloud layer will affect the radiative impacts of the cloud layer, and it is therefore important to capture it correctly to produce realistic simulations of these clouds."

References have been added to the reference list.

18. Page 16, lines 23-25 - "The exact sources. . ." This is not a conclusion. Essentially, it says that nothing has been determined and everything is possible.

Response: Whilst we have not determined the sources of CCN/INP, we have made suggestions of potential sources; this is something that could be addressed during future Arctic campaigns in this region. The nature of the flying during ACCACIA meant that we were not able to draw any further conclusions.

Text changes:

We have replaced the text with the following: "CCN/INP sources have not been determined due to a lack of instrumentation present to directly measure these species. However, back trajectory analyses suggest that potential sources of these particles include: boreal burning in the surrounding region; summertime exposed surface; new particle formation from recent sea-ice melt; or long-range transport from Eurasia."

Response to Anonymous Referee #2 (Received and published: 1 May 2018)

Summary: The objective of this paper appears to be tabulation of some of the aerosol and cloud measurements made during each flight of the ACCACIA campaign. The au-

CC BY

thors list the instrumentation (section 2), describe the meteorology, aerosol and cloud conditions encountered during each flight (section 3), and offer some quite vague discussion relative to some other literature (section 4). Any tabulation of in situ cloud microphysics measurements over the Arctic and Antarctic is valuable. However, the manner in which measurements are partially shown without experimental uncertainty falls below minimum methodological requirements and claims as to active processes are insufficiently supported.

Response: We thank the reviewer for listing the measurements that are presented in the paper, and for highlighting the parts of the paper requiring some improvement or clarification. The specific changes made are summarised in the responses below.

Comments: 1. Re methodology, basic limitations of the measurements reported here such as detection limits and experimental uncertainty are never mentioned, which is not acceptable. For instance, the abstract reports that ice crystals "when present" were at concentrations of "0.42 - 0.88 L-1". This is a very narrow range of ice number concentrations. There is never any value between 0 and 0.42 L-1? Depending on the size range to which the authors are referring here, I would expect the uncertainty in the number concentrations to be much higher than a factor of two. What is the uncertainty in total ice number concentration reported? To give another example, what is the uncertainty in the number concentration of 10-micron aerosol measured by the CAS shown in Figure 4 without error bars? This is a persistent omission throughout the manuscript. One option is to report the experimental uncertainty in section 2 with the instrumentation (I think this should be mandatory). Another option is to report it in Table 3. Another option is to show it in figures. Ideally all should be done.

Response: Regarding the reported ice concentrations: we think this has been misunderstood... as the values mentioned in the comment are the range of mean in-cloud ice concentration reported from flights where ice was detected. As such, there are ice number concentration values lower and higher than these mean values seen across the flights. Standard deviation error bars have been added to Figure 4 for both

CC BY

the Grimm and CAS, this shows the variability in the measurements over the averaging time.

Text changes:

In section 4.1 at the start of paragraph 5, the following has been added: “CDP cloud particle number concentration, LWC and Re are reported in Table 4 along with 2-DS concentrations. For each flight they are presented as the means calculated from regions in-cloud for all times within the science area, and are henceforth referred to as in-cloud means.”

The first point in the conclusion has been adapted to: “The range of cloud microphysics parameters measured for the summertime ACCACIA project were found to be consistent with previous Arctic studies: in-cloud mean CDP droplet number concentration ranged from 21.7 – 132 cm-3, mean CDP LWC ranged from 0.12 – 0.48 gm-3, mean CDP Re ranged from 6.45 – 13.3 μm. In-cloud mean 2D-S total number concentrations ranged from 3.6 – 56.0 cm-3, with in-cloud mean 2D-S ice concentrations (when present), ranging from 0.42 – 0.88 L-1 across all flights. Clouds were sampled across the temperature range 262 – 283 K.”

To Section 2.3 paragraph 1: “The CDP has a nominal sample area of 0.24 mm2, resulting in a sample volume of 14.4 cm3s-1 when travelling at a typical MASIN speed of 60 ms-1: the lowest measurable concentration from the 1Hz CDP data is therefore 0.035 cm-3.”

To section 2.3 paragraph 3: “Assuming a typical MASIN aircraft speed of 60 ms-1, the 2D-S sample volume for particles >110 μm is 4.8 Ls-1, giving a detection limit of 0.21 L-1 for 1 Hz data. For particles of diameter <110 μm, the sample volume is size dependent; for 80 μm particles it is 2.5 Ls-1, giving a 1 Hz detection limit of 0.4 L-1.”

To Section 2.4 paragraph 2: “The CAS has a lower detection limit of 0.07 cm3s-1 for 1Hz data on board the MASIN.”

cc BY

2. I don't believe that sufficient evidence is provided to support the contention about glaciation (last conclusion and last sentence of second paragraph of the abstract). What is the evidence for this? If the liquid phase cloud is not fully overcast and the boundary layer is suffused with slow-falling ice, that doesn't mean that the ice glaciated what the authors refer to as "pockets." If the liquid cloud base or top are variable, and sedimenting or detrained ice is present, that also doesn't mean that any ice present is controlling the cloud parcel phase. I basically just don't believe that the authors have shown support for their last conclusion: "However, intermittently there are sufficient IN to initiate secondary ice processes which then dominate the glaciation process, sometimes producing a totally glaciated cloud in small pockets." IN were not measured. Uncertainties are not reported. The cloud structure is not well illustrated or sampled. I'm open to being convinced, but what is shown here does not convince me.

Response: Our intention for the last point in the Conclusion - and running narrative throughout the manuscript - was to demonstrate that these summer clouds are rarely truly mixed phase and, instead, the clouds contained either predominantly liquid or ice regions. In the relatively few glaciated regions with only ice measured (within the detection limits of the instrumentation used), we often observed high number concentrations of ice crystals indicative of secondary ice processes. In these cases, the cloud particle phase transition from ∼100% supercooled droplets to ∼100% columnar ice occurs abruptly, such that we termed these regions of 100% ice as 'pockets'. At these moderately supercooled temperatures, we suggest that this enhancement is due to the Hallett-Mossop rime-splintering process. These number concentrations were orders of magnitude (increasing to approximately 10 L-1 during M191) greater than expected through primary ice nucleation alone. However, these secondary processes require a baseline number concentration of ice to be present before this runaway enhancement process can be initiated. This is our primary hypothesis for why these ice patches occur - secondary ice production requires enough INP to be available to produce enough primary ice crystals to initiate the process. The low number concentration of ice measured elsewhere throughout the cloud supports the conclusion that low numbers of INP

CC BY

are active and available at these warm supercooled temperatures; however, some factor, whether it is absolute number of primary ice available or environmental conditions we cannot say, is allowing for efficient secondary ice production to occur in isolated patches of cloud, producing glaciated regions. INP were not measured, but their presence is inferred based on no evidence of seeding from higher clouds in the area during M191. Collecting in-situ aircraft measurements in the Arctic region is a complicated task and, as such, we are unable to present data where clouds were sampled to the extent that we can fully present the cloud structure within this paper.

Text changes:

The following text has been changed in the last paragraph of the conclusions: "These results suggest that there are generally a small number of INP present that are active at the cloud top temperatures observed. However, intermittently there is sufficient primary ice to initiate secondary ice processes which then dominate the glaciation process, sometimes producing a totally glaciated cloud in small pockets. This is expected to play a critical role in the water budget of the cloud by increasing the efficiency of the precipitation processes via the ice phase, and hence the lifetime of the cloud locally."

And in the last line of the 2nd paragraph in the Abstract: "Results suggest a small number of ice nucleating particles were active in the region, with sufficient primary ice number concentrations and environmental conditions intermittently present such that secondary ice processes were able to glaciate small portions of the cloud."

3. There is a lack of quantitative analysis. For instance, the authors report that CIP-25 and 2D-S "generally compare well" but sometimes "2D-S numbers were much higher." There is no way for a reader to make a robust comparison of such terms with their own analyses. I will not try to find all such occurrences; please avoid such imprecise quantifications throughout.

Response: In response to the reviewer's constructive criticism, we have removed cases of imprecision in our discussion. For example, in the comparison of 2D-S to CIP-25 data, for number concentrations of particles >100 μm, the agreement was consistent for all flights where both probes were working. However, the CIP-25, developed a fault from flight M193 onwards so these data were not included in this comparison. The agreement for the fit between CIP25 and 2DS for flights M191 and M192 (where both probes were working) was found to be: CIP-25 = 0.22*2DS + 0.12, R2 = 0.82. We have changed the text to include this information.

Examples of text changes made include the following. Others are as provided in the other responses to specific comments:

To section 2.3, paragraph 3: “In this study, measurements from the 2D-S probe have been shown in preference to those from the CIP-25 due to the significantly faster response time and higher resolution of the former, and because the CIP-25 developed a fault from flight M193 onwards. (For earlier flights, CIP-25 and 2D-S measurements showed consistent agreement, [CIP25] = 0.22[2DS] +0.12 with a correlation of R2 = 0.82).”

To section 3.3.1, paragraph 6, we have added: “A small number (<30) of stellar. . ..”

4. Some figures appear not to be called out (5 an 6?). It’s not clear to me what Figure 6 is intended to illustrate. Similarly, why is Figure 7 shown?

Response: Thank you to the reviewer for pointing out these important omissions. Figure 5 is the back trajectories; these are now referred to in the first paragraph of section 3.2. Figure 6 is the examples of columnar crystals. This is now referred to in the third paragraph of section 3.3.1. Figure 6 is to illustrate that when we see the 100% glaciation in M191, the ice seen is columnar, supporting the suggestions that ice multiplication processes, namely HM, are occurring. Figure 7 is shown to illustrate that for M191, the occurrence of almost fully glaciated cloud was much more frequent than in M193 when this type of observation was also made. These ice mass fraction data, shown in Figure 7, will be of use to modellers as they illustrate how the cloud phase distribution can be so different between different flights performed over a similar region

CC BY

and temperature range. Given the similarities (in e.g. temperature and region sampled) between M191 and M193, these cases would provide a good opportunity for a cloud modelling comparison study. It is interesting to note how these cases differ in terms of their cloud thermodynamic phase distribution, and Figures 6-10 provide ideas of differences between these flights which may act as a first step in testing these cases in a modelling capacity. For example, we show that the particle habits measured, and locality of these measurements, are different, while the temperature range sampled is similar. Furthermore, we show, in Table 4, that the characteristics of the liquid phase (e.g number concentration, droplet effective radius) is also very different between these cases.

5. Averaging times are never discussed. For instance, what is the time frequency and rough flight length used to generate Figure 7? Is it the same for all aerosol and cloud measurements shown in the paper?

Response: Figure 7 is generated from all in-cloud times for each flight when in the science area (i.e. does not include transit times where other cloud types may have been sampled). This is ∼1hr 33 mins of cloud data from M191 and ∼3hr 12 min from M193 (can be calculated from Table 2). This has now been made clearer in the figure caption. As is indicated in the caption for Table 3 – aerosol data is taken from the available cloud free periods during straight and levels runs either below or above cloud layers. Mean and standard deviation values were calculated over the longest times possible in cloud-free air (over a minimum of 5 minutes). As is indicated in the caption for Table 4 – cloud data is reported as the mean (standard deviation) and the (25th percentile) median (75th percentile) from all in-cloud data points within the science area. The approach of reporting the mean/standard deviation and median/interquartile ranges was chosen to convey the most information to modellers regarding realistic ranges of cloud microphysics parameters that are reported here.

Text changes:

CC BY

The text at the start of paragraph 5 in section 4.1 has been changed to make our approach clearer. It now reads: “CDP cloud particle number concentration, LWC and Re are reported in Table 4 along with 2-DS number concentrations. For each flight data are presented as the means calculated from regions in-cloud for all times within the science area, and are henceforth referred to as in-cloud means.”

6. I don’t understand the point of Figures 8 and 9. What is the authors’ hypothesis for the presence and absence of stellar crystals and why does it matter? If they occur at cloud top and did not sublimate between cloud top and surface, why aren’t they observed in between?

Response: These are included so that modellers will know that it is possible that these habits and size of ice crystals are present in the Arctic environment. These crystals were observed at temperatures where these habits are not expected; therefore, in a model, these may be assumed to be the predominant habit at that temperature (e.g. columns at -5 $^{\circ}$C). In fact, ice crystals are often assumed to be spherical in cloud-resolving models, thus the stark differences in crystal habits observed here, at similar temperatures, is an important finding for modellers to be aware of. Furthermore, these images are included, alongside those shown in Figure 6, to illustrate that the typical modelling assumption that ice crystals are treated as spheres is inaccurate. Whilst this is often the case across all mixed-phase or ice clouds, we are able to show which crystal habits are present in these Arctic single- and multi-layer stratocumulus clouds for use by the modelling community to improve ice crystal habit representations in numerical models. These dendritic crystals were detected throughout the cloud and were co-located across various sampled altitudes: this is what Figure 9 is illustrating (this has now been reverted to a 2D representation). The hypothesis is that there was seeding of ice crystals from above that exhibit a stellar structure due to their environmental history prior to entering the cloud in which they were sampled. Seeding is especially difficult to replicate in models due to the requirement to get inter-cloud humidity and temperature profiles accurate enough to slow sublimation. By presenting these obser-

Printer-friendly version

Discussion paper

CC BY

vations, we illustrate that seeding does occur in these multi-layer stratocumulus and is an important physical process to represent correctly in models. To do so, it is likely that ice crystal habits will need to be explicitly considered, as these will affect ice mass and sublimation speed. Furthermore, using ice habits in models will provide more accurate estimates of cloud radiative forcing to be made, given a dendritic crystal will interact with solar radiation differently than the typically assumed spherical ice crystal.

Text changes:

Section 3.3.1, paragraph 5 has been changed to: “These crystals were seen on three separate occasions, initially at 12:22 for approximately 3 minutes between 3200 m to 3600 m during a profile ascent (see Fig. 8) then 7 minutes later at 12:33 for approximately a minute between 4300 and 4100 m at the start of a descending profile, then finally 12 minutes later (12:46) around 1800m for less than a minute during the same profile. From Fig. 9, we can see that the stellar crystal regions were co-located at different heights, with the degree of riming and aggregation increasing with descent through the cloud (Fig. 8). Heavily rimed ice crystals co-located at a lower altitude earlier in the flight (12:10), also show recognisable stellar characteristics. The presence of large stellar crystals at cloud top is indicative of seeding from aloft, and such habits are not expected at the warm supercooled temperatures measured. Ice crystal seeding is typically not considered in cloud microphysical modelling studies in the Arctic (e.g. Morrison et al. 2005), however, the presence of these crystals (at sizes > 1200um) illustrates that cloud seeding is a factor that must be considered in multi-layer Arctic stratocumulus, and is a process which requires addressing in cloud-resolving models.”

7. Figure 1: colors in legend and figure don’t match.

Response: Figure 1 has been amended.

8. The yellow lines in many figures are illegible (e.g., Figure 4h).

Response: Yellow lines have been addressed in Figures 4, 5 and 12.

CC BY

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-283, 2018.