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Interactive comment on “Arctic low-level boundary layer clouds: in-situ measurements and simulations of mono- and bimodal supercooled droplet size distributions at the cloud top layer” by M. Klingebiel et al.

Anonymous Referee #1

Received and published: 21 July 2014

Review of “Arctic low-level boundary layer clouds: in-situ measurements and simulations of mono- and bimodal supercooled droplet size distributions at the cloud top layer” by Klingebiel et al.

Recommendation: Requires revision before publication.

This paper reports on some microphysical and aerosol measurements that were made during 13 research flights of an instrumented Basler Bt-67 research aircraft operated out of Inuvik. The paper is technically sound and easy to understand. Further, the

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measurements reported in this paper contributes to the growing number of data sets describing Arctic clouds, about which more information is required in order to better understand the changing climate and melting sea ice in Arctic regions. The two novel contributions of the paper are the description of the spatial distribution of the droplets in the bimodal size distributions and the direct numerical simulations that are performed in an effort to understand the physical processes that are responsible for the appearance of the bimodal peaks. Given this, I highly recommend that this paper should be published in ACP.

However, there are many aspects of the manuscript that should be improved before the paper is published in order that the collected data realize their maximum potential in understanding arctic cloud processes: 1) there is an inadequate referencing and comparison against previous arctic cloud experiments, many of which have investigated many of the issues described in this paper in more detail than presented here; 2) a more comprehensive analysis of data from the 13 research flights should be presented rather than concentrating descriptions on only portions of flights flown on a couple of specific days; 3) a better description of uncertainties and limitations associated with the instrumentation, and 4) there should be an explanation of how sensitive the results of the simulations are to some of the parameter values chosen for the simulations. If these issues are adequately dealt with, I highly anticipate that the paper will be of sufficient quality to be published in ACP.

1) There is an inadequate comparison against the results of previous studies. The authors have ignored the results of two of the most detailed studies of the properties of mixed-phase and supercooled boundary layer clouds collected in the Arctic, namely the Mixed-Phase Arctic Cloud Experiment (M-PACE, Verlinde et al. 2007, Bull. Amer. Meteor. Soc.), and the Indirect and Semi-Direct Aerosol Campaign (ISDAC, McFarquhar et al. 2011, Bull. Amer. Meteor. Soc.), both conducted over the DOE ARM North Slope of Alaska Site. The authors' descriptions of the changes in cloud properties in their vertical profiles mirror many of the features that have been uncovered in the plethora of

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studies published using data from these two field campaigns. Some of the observations during those campaigns can also assist in explaining some of the phenomena seen in the VERDI data. For example, Fig. 12 in McFarquhar et al. (2011) shows the small-scale structure and inhomogeneities in the cloud microphysical characteristics using an airborne Doppler X-band radar, showing the close proximity of updrafts and downdrafts in arctic boundary-layer clouds. They also used these observations and modeling studies (Korolev and Isaac 2003; Korolev and Field 2008) to show that a quasi-steady state driven by cloud top radiative cooling could be reached under appropriate conditions, consistent with the findings in this study. They also showed that the turbulence and mixing was responsible for the observed cloud distributions and mixing. They showed that vertical mixing had a major role in determining the particle size distributions, and is a good reason why their profiles were not adiabatic.

Other analysis of the microphysical data using M-PACE and ISDAC data reached similar conclusions and should be referenced in this study. For example, McFarquhar et al. (2007, JGR) looked at 53 profiles within single-layer stratus clouds in the Arctic during M-PACE. Their analysis (e.g., Figure 11, 12 and 14 along with Table 2) showed many of the same features shown here, with increases in particle size and liquid water content with height, but with a relatively constant liquid droplet number. However, their presented size distributions do not seem to show evidence of a bimodal peak. Using ISDAC data, Jackson et al. (2012, JGR) compared cloud microphysical properties to aerosol concentrations measured above and below clouds. Correlation analysis showed that aerosols were being mixed into the cloud from above. And, Earle et al. (2011, JGR) examined correlations between aerosols measured below clouds and liquid cloud properties, with calculations from an adiabatic parcel model showing increases in aerosol concentration can limit activation to larger and more hygroscopic particles, explaining some differences. As all these studies can explain, to various extents, results obtained in the current study, they should be referenced at the appropriate place in the manuscript to show which of the findings in the current manuscript have already been seen by others. Even better, it would be nice if the current authors

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were able to explain reasons for similarities and differences between their study and the previous ones, so that our understanding of aerosol-cloud interactions in boundary layer clouds was further enhanced.

2) I am a bit puzzled as to why such a limited set of data was presented in this study. The authors state that there were 13 research flights conducted during VERDI, but they only seem to present flights from the 14 May and 15 May 2012. I think their results and conclusions would be more convincing if they were to incorporate a more complete analysis of their data, showing relationships between cloud and aerosol concentrations, doing HYSPLIT trajectory analysis to identify where the aerosols were coming from on different days, and showing a more complete data set of how often, when and under what conditions the bimodal distributions actually occurred. This is important because the April-May season is typically a transition season in the Arctic, where both pristine and polluted conditions can occur. The authors might want to review papers of Glenn Shaw and Patricia Quinn that have examined the seasonal distributions of aerosols in the Arctic to further explore this topic. Thus, if aerosols are having an influence on the measurements, it is possible that different results will be obtained on different days. I should also mention that I found the description of the flight profiles unsatisfying. Can you present a 3-d flight track or give some description of the different profiles that were executed on the different days? I do not quite understand how the vertical profiles were obtained. Were the legs shown in Figure 1 flown sequentially at different altitudes, or was the altitude varied throughout the flight leg indicated in red? If it is the later case, this could be a bit of a concern. See Figure 13 in McFarquhar et al. (2011) that gives an example of how much a flight leg can vary during an individual leg. For that matter, I am also curious how the location of cloud top was identified? I don't think the lidar would be giving good observations of cloud top when the aircraft was ascending through cloud top? And, with respect to analysis of observations from several different flight days, McFarquhar et al. (2007) combined data from several different flights by plotting cloud parameters as a function of a normalized altitude (zero at cloud base, one at cloud top) so that patterns from multiple flights could be seen on the same plot. Such type

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of analysis would be helpful for doing a more comprehensive analysis of the complete set of data collected.

With regards to Figure 6, a classic example of a single-layer arctic boundary layer cloud is shown. Were such patterns observed on all 13 flights? This is certainly worthy of a paragraph discussion. In the ISDAC and M-PACE field campaigns, the single-layer nature of boundary clouds was a rarity, with clouds typically occurring in multiple layers. Was this ever seen in VERDI?

3) There are a couple of issues with regards to the instrumentation that could be described better. How good of an overlap is obtained between the CDP and CIP? This could be shown in an Appendix to give more confidence in the quality of the data. There is a potentially big problem with the quality of the number distribution functions estimated from the CIP data in the size range 50-125 microns due to the small and poorly defined depth of field that has been noted by Baumgardner and Korolev (1985). What correction was used for the depth of field. The authors do comment that the modified Korolev-tips were used on the probes, which is important given their note that ice crystals could occur in the clouds. However, Korolev et al. (2013) has shown that such tips do not remove all shattered artifacts. Were the Field et al. (2003, 2006) artifact rejection algorithms, based on interarrival times, also applied? This should be discussed. Also, what algorithm was used to identify the phase of the clouds? The authors mention that needles were sometimes seen, but I do not know whether this was frequent enough that all clouds were mixed-phase, or there were time periods with only liquid. What fraction of the 1 s times were liquid as opposed to mixed-phase? Could there be small non-spherical ice particles that are affecting the forward scatter signal from the CDP probe? For example, McFarquhar et al. (2013, JAOT) found that even in mixed-phase clouds, some of the small quasi-spherical particles could actually be ice particles.

4) The modeling study is one of the strengths of this paper, as it gives a better explanation of the observed bimodal size distributions than I have previously seen. However,

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in order to make this section more convincing I would recommend that the authors perform some sensitivity studies to determine how sensitive their results are to some of the assumed values. Are the similar qualitative conclusions reached as long as reasonable values are chosen for LWC, temperature, relative humidity, turbulence and mixing parameters? Also, some non-standard terminology is used when describing the simulation parameters. The authors state a LWC of 0.25 and TWC of 3.15 g/kg is used. Typically, I think of $TWC = LWC + IWC$, where IWC is ice water content of cloud. I think in this context the authors are thinking of TWC as liquid water plus vapor?? If so, I recommend rewording. I think it also might be appropriate to reference the studies of Korolev and Mazin and Korolev and Field here, as well as some other studies. For example, on p. 14614 the authors state that they neglect microphysics, surface fluxes, shear or rain. But, there have been many studies showing the nature of the circulations and boundary layer clouds in the Arctic, and whether or not they can be maintained, depend critically on some of the parameters chosen to represent these processes. This issue should be discussed in more detail.

Specific Comments:

p. 14602, line 29: dominates rather than dominated

p. 14603: Sentence beginning on line 6 is awkward. Reword

p. 14606, line 9: what probes are used to characterize the size distributions in which size ranges?

p. 14606, line 18: Are humidification effects of aerosols dealt with in deriving the size distributions?

p. 14607, line 11: See McFarquhar et al. (2011), Figure 13, which shows there can be substantial changes of the cloud top height during the course of a flight. Was any change of cloud top height occurring during this flight?

p. 14608, line 13: How did the sum of the cloud and aerosol concentration change with

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height? Might be interesting to compare with Fig. 13 in McFarquhar et al. (2011) who showed this sum was constant both within and below cloud?

p. 14608, line 28: Do you have any information about the strengths of updrafts in these clouds? Although your stated growth times of 7-37 minutes are well within the lifetimes of such clouds, in 10 minutes with a 1 m/s updraft, the particles would ascend 600 m, which is greater than the depth of your cloud. Thus, there must be some sort of circulation like that in the Korolev and Field model to support this growth as well.

p. 14609, line 19: Can this argument be made more quantitative? Given the number of particles in each bin needed to have about 10% uncertainty (something Hallett (2003) lists as statistical significance), I would think that not a very large averaging time would be required to get adequate statistics at cloud top. How thick is the relevant cloud level? If you only need 1 s or so to get adequate statistics, I'm having trouble visualizing that the airplane would be ascending or descending so fast that you would not get adequate statistics.

p. 14610, line 12: The spring is the transition season in the Arctic and there can be a lot of day-to-day variation in the aerosol amount. Further, a lot of inhomogeneity in aerosol amount in the vertical and horizontal was noted during ISDAC aircraft missions. Were similar uniform distributions and assumption of arctic background aerosols also valid on other days?

p. 14610, line 18. Was the HYSPLIT analysis done on the other days as well to show where the aerosols were coming from on those days?

p. 14610, line 22: See comments above. Can more observations to be shown to confirm that clear arctic atmosphere with low number densities were consistently seen?

p. 14611, lines 12-13: Recommend removing this. Given the large variations in the transition season noted in the Arctic in other regions (e.g., Alaska), I think comparisons of measurements 4 years apart are too uncertain to be included in the paper. The

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paper is sufficiently strong if that data analysis section is expanded without doing such a comparison.

p. 14613, lines 11-15: Might also be worth comparing with the findings of Jackson et al. (2012) who looked at role of entrainment of aerosols from the top of the cloud on ice nucleation, and also comparing with the study of Earle et al. (2011).

p. 14615, line 11: Add “the” between “as” and “next”

p. 14614, line 15: Other studies have found processes like microphysics and surface fluxes are very important in determining the mixing dynamics. See aforementioned papers by Korolev and collaborators.

p. 14615: what does TWC mean? In cloud microphysics this frequently refers to sum of ice water content and liquid water content. Clarify and possibly use alternate terminology for consistency with previous papers.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 14599, 2014.

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