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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Microphysical and optical properties of Arctic mixed-phase clouds – the 9 April 2007 case study" *by* J.-F. Gayet et al.

Anonymous Referee #1

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Review of "Microphysical and optical properties of Arctic mixed-phase clouds – the 9 April 2007 case study" by J.-F. Gayet, G. Mioche, A. Dornbrack, A. Ehrlich, A. Lampert, and M. Wendisch, submitted for publication in Atmos. Chem. Phys. Disc.

Recommendation: This paper is acceptable for publication subject to minor revision.

This well-written paper presents airborne observations of a mixed-phase arctic boundary layer cloud observed during the ASTAR campaign. Similar to previous studies of arctic mixed-phase clouds, a cloud top layer dominated by liquid water is seen with low concentrations of large ice crystals throughout the clouds. The main way in which this paper extends existing literature in this field is through the analysis of co-located in-situ measurements and CALIPSO profiles, and through comparison against the ECWMF operational analysis of cloud properties. For these reasons, this paper represents an

original contribution and should be published. However, there are a couple of items that should be addressed before the manuscript is finalized. These items are documented below:

Major Comments

1. I feel that the introduction could do a better job of placing the observations acquired during this case study in ASTAR in the context of the existing body of literature that has already been published characterizing the properties of arctic mixed-phase clouds during projects such as FIRE-ACE, SHEBA, and M-PACE. For example, there have been in-situ measurements on arctic clouds published by Hobbs and Rangno (1998), Lawson et al. (2001), Rangno and Hobbs (2001), Boudala et al. (2002), Korolev et al. (1999, 2003), McFarquhar and Cober (2004) and McFarquhar et al. (2007), and remote sensing observations of arctic clouds by authors such as Shupe et al. (2001), Intrieri et al. (2002), Dong and Mace (2003) and Zuidema et al. (2005). There are also a number of modeling studies published in the last year through analysis of a couple of case studies of single- and multi-layer mixed-phase clouds during M-PACE. How does the work presented here fit in with the conclusions and physical understanding from these studies? I know that some of these issues are somewhat dealt with later in the paper, but I think more about the context of this study should be given in the introduction.

2. Some more information about how the CPI size distributions are derived should be given on page 11338. For example, did you use the Connolly et al. (2007, JAOT) correction factor for deriving the size distributions? I think that there is a lot of uncertainty in calculating the concentrations from the CPI, especially for particle sizes less than 150 micrometers, and this could seemingly have a big effect on the analysis presented in this paper. In addition, on this (or any other project) have you compared your CPI size distributions against those measured by other probes, such as either a Cloud Imaging Probe (CIP) or two-dimensional cloud probe (2DC)?

Minor Comments

1. Page 11335, line 22: data from field experiments are used to “evaluate” satellite retrievals rather than “validate” them.

2. Page 11336, line 26: How do you get particles with sizes of several mms from listed probes? With the small sample volume of the CPI and the size of the CCD array, I don’t think you typically get information on particle sizes of several millimeters.

3. Page 11337, line 3: The paper of McFarquhar et al. (2007, GRL) directly shows scattering on forward scattering spectrometer type probes through comparison of a CAS against a CDP and should be referenced here.

4. Page 11341, line 3: Was there a King probe or other bulk water probe (e.g., Gerber) for comparing against the liquid water from the FSSP-100? This would give a good consistency check.

5. Page 11341, line 14: McFarquhar et al. (2007) used normalized altitude when comparing profiles obtained from different clouds to accommodate the changes in cloud top altitude. Using this technique might be a good way of comparing your different profiles.

Page 11343, line 10-11: First, what definition of effective diameter are you using (e.g., see McFarquhar and Heymsfield 1998 JAS for a list of the different definitions that have been used). Also, I think you need to be careful with what you are stating here. A large effective diameter does not necessarily reveal the existence of large ice crystals, especially if you are using a definition that is proportional to the volume/area ratio of ice crystals (e.g., definition of Fu 1996 or Francis et al. 1994).

Page 11343, page 24: It would be nice to have some comments about how the findings here compare against those of previous studies. For example, Korolev et al. (1999) found that 98% of the ice crystals they measured in the arctic were irregular. McFarquhar et al. (2007) also presented some summary figures that showed their shape distributions in the arctic boundary layer stratocumulus.

Page 11344, page 1-4: McFarquhar and Cober (2004) showed that there was a substantial difference in the shape of the FSSP size distributions depending on whether observations were made in liquid or glaciated clouds. I think you could indeed verify that your FSSP size distributions are peaked like those of McFarquhar and Cober (2004), which would offer more evidence that the FSSP is responding to water. Also, it might be interesting to compare your results here against those of McFarquhar and Cober (2004) who showed that their results for the scattering properties of mixed-phase clouds were heavily dominated by contributions of liquid drops, which seems to be a bit different than the conclusion you are presenting here.

Page 11344, line 26: See my comments above about determining the CPI concentrations. I am worried that this comparison between the 2DC measurements of McFarquhar et al. (2007) and these CPI measurements is potentially affected by instrument issues.

Page 11344, line 27. I think this would be good to compare against the results of Cober et al. (2001), Korolev et al. (2003) and McFarquhar et al. (2007). All these aforementioned studies found that the liquid fraction had a parabola type shape in mixed-phase clouds, where clouds typically had fl < 0.2 or fl > 0.8, with very few values in between (i.e., clouds were either dominated by liquid or ice, but few clouds had relatively equal contributions of ice and water). Your results showing fl varying between 0.80 and 0.43 indeed show less dominance of liquid water phase and the lack of this parabolic shape. I think this discrepancy should be mentioned even if the reasons for the discrepancy cannot be explained.

Page 11346, line 8: I think you should quote the sizes of the large ice crystals that are appearing rather than an effective diameter (see my previous comments on effective diameter).

Page 11346, line 13. Is it possible to include some more details of the Mioche et al. (2009) algorithm in this paper? An EGU preprint is not readily accessible, and I

think there would be a lot of interest in the community on how these calculations are performed.

Page 11348, Line 14-15: What about the difference in the spatial scale between the ECMWF analyses and the observations? Could that not cause some of the differences? In any event, some comments should be made about the differences in spatial scale and how this might affect the comparison. Another question that could be raised is the impacts of the small size (and representativeness) of the small amount of data that went into the observations.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11333, 2009.