

Interactive comment on “Observed microphysical changes in Arctic mixed-phase clouds when transitioning from sea ice to open ocean” by G. Young et al.

Anonymous Referee #1

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Review of “Observed microphysical changes in Arctic mixed-phase clouds when transitioning from sea ice to ocean” by Young et al.

Recommendation: Should be acceptable for publication following minor revision This paper presents analysis from a case study showing how the cloud, aerosol, and thermodynamic properties changed during a flight of the BAe-146 over the Arctic, sampling the transition from sea ice to ocean. As it is well recognized that surface and meteorological conditions, in addition to aerosols, can impact cloud properties and that this is one of the first case studies showing the transition from sea ice to ocean during a single flight, I think that the paper adds to the body of literature about arctic clouds and should be published. Overall, the paper was well written and easy to understand

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and most of the conjectures were well justified by the data. Nevertheless, I have highlighted a couple of areas where the paper could be improved before it is accepted for final publication.

Major Comments

I would have liked to have seen more information about the meteorological conditions. This would provide a better context for the analysis of the latitudinal and longitudinal variation of cloud properties. It would be easier to attribute changes in cloud quantities in a N-S direction to variations in surface conditions if it was better known that there was no major system that could have also influenced the cloud conditions. Further, the aircraft only sampled in one direction at one particular time. Thus, when the authors stated that the 500 m cloud was not present at lower altitudes between some latitudes, this is only saying that the 500 m cloud was not present on the particular line being sampled. Although the surface conditions (i.e., open water versus sea ice) varied mostly as a function of latitude, it cannot be categorically assumed there were not variations in cloud properties (or location) as a function of longitude at the altitudes the plane was not sampling. While it is true that the measured microphysical properties did not vary a lot along the E-W runs, there is still a possibility there could be variation in the clouds or cirrus present at different altitudes in the E-W direction (e.g., the 500 m cloud layer). I think if some meteorological analysis (if available) were shown, it would better justify the analysis of cloud properties as a function of latitude.

I was a bit confused in the manner that some of the supplementary material was included in the manuscript. When I typically think of a supplement, I typically think it is related to arguments that are made outside the scope of the main paper. However, most of the supplementary figures are referred to in the body of the text. Thus, why not just make them regular figures and include them in the paper rather than including them as a supplement?

The authors stated that mixed-phase clouds in the arctic are characteristically topped

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with a liquid layer which facilitates ice formation below. Although some arctic clouds definitely have this structure, I would say that the majority of arctic mixed-phase clouds do not have such a simple structure. Instead, they occur in many layers with also many layers of supercooled water sometimes embedded within them. This was discussed in Verlinde et al. (2007) and there was a study by Morrison et al. (2009) where various models characterizing such multilayer clouds were compared. This distinction should be made in the manuscript at the relevant places. There has been a greater emphasis on the single-layer clouds in the literature because they are conceptually easier to understand and study.

The authors state that their study represents the first investigation using in-situ airborne instrumentation on how cloud microphysical properties change with varying sea ice cover. I would agree that their study is the first detailed case study where such an issue was examined, with samples over varying surface characteristics on the same flight. But, I think it would be important to reference the study of Jackson et al. (2012) as they compared data from pristine and polluted conditions from two different field projects (ISDAC and M-PACE), noting that differences between the cases were caused not only by varying aerosol amounts but also by differences in the sea ice cover (open ocean versus ice). Hence, by referencing this paper, their study could be put in the proper context.

I think some details about the microphysical instrumentation are lacking. I was surprised that they did not mention any bulk measurements of liquid water content or any measures of supercooled water. Were any available? I would think the bulk measures of liquid water would be on the aircraft as a way of testing the stability of the CDP calibration. In addition, how was the phase of the particles identified? The authors note that there were mixed-phase clouds where the CDP was measuring cloud droplets and the 2DS/CIP measuring ice crystals. Was there any detailed investigation to show that the small particles were indeed droplets? McFarquhar et al. (2013) showed that in mixed-phase clouds some of the smaller particles could also be ice. Looking at the

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shape of the CDP size distributions could also give some information on the likelihood that the small particles are either water or ice. Finally, was reconstruction used to extend the size ranges of either the CIP or 2DS? This should be mentioned.

The authors make the point that the ocean cloud would likely reflect incoming short-wave radiation more efficiently than a sea ice cloud. I think it is important to place this statement in an appropriate context. The sea ice will be much more efficient at reflecting the radiation than the ocean, and the role of the cloud should be considered in the context of the surface.

My final (optional) suggestion is that it would be nice to include some sort of schematic diagram in the conclusion on how the cloud, aerosol, and thermodynamic properties varied across the ocean, sea ice and transition region. I think this would be a nice way of summarizing their findings and would be a diagram that could also be well referenced in future studies.

Detailed Comments

Page 3, line 15 replace semi-colon after Crosier et al. (2015) with a comma

Page 12, line 7: I would prefer dramatically to be replaced by something more quantitative.

Page 14, line 17, is it appropriate to call this cloud cirrus if it has LWC? Maybe this comment is more directed at the Verlinde et al. (2007) paper, but I think it is also relevant for this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-409, 2016.

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