

Interactive comment on “Variability of the mixed phase in the Arctic with a focus on the Svalbard region: a study based on spaceborne active remote sensing” by G. Mioche et al.

Anonymous Referee #3

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Review of “Variability of the mixed phase in the Arctic with a focus on the Svalbard region: a study based on spaceborne active remote sensing” by Mioche, Jourdan, Ceccaldi and Delanoe

Recommendation: Major revision.

This timely contribution addresses a topic of incredible importance, namely the coverage of clouds and mixed-phase clouds in the Arctic. In particular, the vertical, spatial and seasonal variability of clouds and of mixed-phase clouds over the entire Arctic region and of the Svalbard archipelago are examined. This study is important on mul-

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iple fronts: the climatology of such clouds over the Arctic are not well understood, and since the phase of clouds has major impacts on radiative transfer, the distribution of the clouds and phases needs to be better understood; efforts to improve our knowledge of the vertical and seasonal variability, and how it depends on factors such as the surface characteristics, need to be known if results of large-scale models can be validated, and better parameterizations of the controls of cloud properties produced; and, it is important to determine how representative statistics from specific locations that are used to provide more comprehensive measurements of cloud properties from surface stations are of the entire Arctic region in general. Thus, I think that this paper should definitely be published, and possibly highlighted if a number of problems with the current manuscript are corrected.

I found that there were a couple of areas in which the manuscript could be improved. First, I found the organization of the manuscript peculiar. Typically, I would expect discussions of the quality control and uncertainty associated with the analysis to be presented before the results of the analysis are shown. The opposite seems to be done in this paper, with Section 5 discussing the limitation of the data after the results have already been presented. It would be much easier to have this discussion up front so that one could be interpreting the results with these limitations in mind.

Second, I find it curious that the authors use Version 1 of DARDAR given that Version 2 is available, and given their suggestion that there are some rather large discrepancies between the two versions. Although I am entirely sympathetic with the desire to publish analysis that has already been performed without doing extra work, given the rather large differences I would encourage the authors to replace the Version 1 results with the Version 2 results. Presumably the codes for doing the analysis and for plotting the figures have already been written, so it would not be that much extra work to repeat the analysis. I think the end product that would come out would be that much better.

Third, I am a bit concerned about how the omission of clouds with $z < 500$ m is affecting the statistics. Past ground-based and in-situ observations have suggested that in some

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regions of the arctic mixed-phase clouds can be frequently occurring within 500 m of the surface. Hence, omission of these clouds would be biasing the statistics. Although data to evaluate whether clouds are present throughout the Arctic at $z < 500$ m, presumably this could be done for a few of the stations in the Arctic where ground-based remote sensing data are available. It would be nice if some quantitative estimate of the uncertainty could be made due to the omission of these clouds.

Fourth, I am left wondering about whether variations in aerosol concentrations or compositions could be responsible for some of the variations that are seen in the characteristics of the clouds. Whereas I don't think that the authors need to do any analysis with regards to this because the paper is already quite complete, I think that at least some comments should be made.

Fifth, the paper needs to be carefully inspected for grammatical and spelling errors that are pervasive throughout the manuscript and can be a little distracting.

Specific comments:

Page 23455, line 11: Reference Vogelmann and Lubin study for dominance of long-wave effect

Page 23456: Korolev has published a number of studies on causes of persistence of mixed-phase clouds and has developed conceptual models that explain their persistence. Some of his papers should be referenced here. Also, on line 10 the authors talk about the persistence of supercooled water droplets. I don't think it is the supercooled water droplets that are persisting, but rather the supercooled or mixed-phase clouds themselves. The water is being continually cycled through the cloud, with formation of new supercooled drops and glaciation or falling out of others. The discussion as written makes it sound like one cloud is being maintained for several days or weeks, rather than a recycling of the water in the ubiquitous mixed-phase clouds.

Page 23457, line 12: I don't think this structure is particularly peculiar and has been

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observed in many other locations in addition to the Arctic. See Fleishauer et al. (2002) and Plummer et al. (2014) as examples of mixed-phase clouds or clouds having supercooled water near the tops of clouds.

Page 23457, lines 17-21: Check that references are referring to the right field experiments.

Page 23459, line 1: Wouldn't it make sense to validate the methodology by comparing against ground-based remote sensing retrievals at some of the specific Arctic stations where such data are available? This is hinted at on page 23463 line 13 where it is stated satellite cloud fractions are smaller than surface observations by 3 to 35%, which seems like a very large amount!

Page 23460: See major comment above on difference between Version 1 and Version 2 of DARDAR. It almost reads that inclusion of Version 2 analysis was an afterthought in the paper.

Page 23462: The writing needs to be improved here. It seems that there are a number of disjoint sentences, each written as a separate paragraph.

Page 23463, line 27: I would like to see a bit more of a quantitative statement here rather than saying methodology is justified based on consistency: would also like to see comparison against ground-based retrievals.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23453, 2014.

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