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Atmospheric Chemistry and Physics Discussions

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 #2

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1 General remarks

The manuscript provides a detailed climatology of mixed-phase clouds in the Arctic. The data is derived from active satellite remote sensing (CALIPSO/CLOUDSAT) and therefore includes day and night time observations as well as information on the vertical distribution of mixed-phase clouds, which is a substantial improvement compared to data sets based on passive remote sensing. The analysis of the data characterizes the horizontal, vertical and temporal variability what was not summarized in such detail before. Specific pattern of this variability could be linked to the different meteorological

preconditions for the formation of mixed-phase clouds. Especially the differentiation for single layer mixed phase clouds helped in that way. Overall I think that in future the presented data set and potential extensions to data of recent years may help to identify how climate warming will change Arctic cloud properties what is still uncertain from climate modelling.

Thus the manuscript provides an important contribution to current and future research and is worth to be published as it substantially helps to improve our knowledge on Arctic cloud properties. However, in my opinion the manuscript lacks of two major issues which have to be reassessed in detail before publishing the manuscript. First the statistical analysis does not consider warm clouds. Second, the version of the retrieval algorithm DARDAR used for the manuscript is outdated as stated by the authors themselves.

Below, I compiled a list of comments which have to be considered in a revised version of the paper. There might be some contradictory statements resulting from my misinterpretation of the text when first reading. I am sure the authors will know how to weight in such cases and how to improve the text to avoid misinterpretations by other readers.

2 Major comments

Neglecting warm clouds

At the end of section 2.2. the authors explicitly note that warm clouds are not considered in calculating the statistics although already in the next sentences F_{CLOUD} is named "total cloud occurrence". I think neglecting warm clouds is a big mistake as it limits the interpretation of the mixed-phase cloud occurrence presented in the manuscript.

Considering climate warming, there might be an increase of warm clouds at the expense of mixed-phase clouds, especially in Arctic summer. If you want to detect any trends in occurrence of mixed-phase clouds linked to climate warming you have to include warm clouds in your statistics. Otherwise you always normalize the number of mixed-phase clouds by all clouds $< 0^{\circ}$ C which might remain a stable fraction despite a general decrease of mixed-phase clouds.

Furthermore, not considering warm clouds also makes N_{CLOUD} as presented in Fig. 2 meaningless with regard to analysis of the cloud radiative forcing. For the Earth energy budget, all clouds including warm clouds are of importance. Especially warm clouds which are likely to have a cooling effect. So presenting a cloud occurrence without warm clouds may give a wrong impression to the reader.

Some questions still remains here. How often warm clouds can be observed in Arctic at all? Do the ground based remote sensing observations presented in Fig. 2 do also neglect warm clouds? How the comparison would look like if warm clouds are included.

DARDAR product Version 2.

The authors present a section comparing DARDAR products from version 1 used in the manuscript and version 2 already published by Ceccaldi et al. (2013). Differences between both versions are quite significant ranging up to 15 % for mixed-phase cloud occurrence. Although these differences are known, the presented climatology is calculated using the old version 1 of DARDAR. Having the new version 2 availably I highly recommend to reprocess the data using the version 2. There is no need to show outdated data and draw conclusions from an obviously biased data base. Using version 2 would also be more forward-looking in case the data set will be extended to more recent years.

The comparison between V1 and V2 can still remain part of the manuscript. Just interpret differences as the improvements compared to literature using V1.

3 Minor comments

Title: "Variability of the mixed phase...". "The mixed phase" is somehow undefined and can also be related to subjects different than clouds. I suggest to reword "Variability of mixed-phase clouds...".

P23455, 12: "global net warming effect". Does that mean, that Arctic clouds warm so much, that globally all clouds are warming? Or do you refer to a "regional net warming effect"?

P23456, 29: Discriminating ice and liquid (pure ice and pure liquid clouds) is not as difficult with current instrumentation. The statement of the authors is a little exaggerated. What is still limited and challenging is the discrimination of mixed-phase clouds from pure ice clouds. E.g. from remote sensing where even a small fraction of large ice crystals in mixed-phase clouds may cause similar radiative properties like pure ice clouds have.

P23457, 12-20: This long sentence is very hard to read. Do the last references refer to SORPIC 2010? This can not be as publications are published earlier than the campaign.

P23460, 6: The criteria using the strong attenuation is not included in Fig. 1. Please add.

P23460, 11: "Physical thickness" I can not imagine that the lidar can penetrate all clouds entirely and give the cloud top and base altitude. That's why I would not call it "physical thickness". This is probably only the lidar penetration depth. Am I right? If so, this implies that there are certain limits in the cloud phase retrieval. Frequently low level clouds will be not classified. Please highlight at some point.

P23460, 15: First reading it was not obvious that cloud classification by DARDAR provides vertical information and not only a single cloud class for each pixel. It would be helpful for the reader to highlight that more clearly at the begin of this section.

P23460, Sec. 2.1: Overall there are a lot of assumptions and limitations in the cloud classification. To better understand the results please give a summary or definition what "Mixed-Phase Cloud" means in the context of the manuscript. What clouds are included, which potential mixed-phase clouds are not?

P23461, 4: Is there a justification why this area was chosen? Comparison with ground-based measurements at Ny Alesund? What I'm not sure is, if choosing such an area with a mixture of open water and land is a good choice. My first idea would have been to pick an open water and an ice or land covered area for comparison. Based on the theoretical processed behind the dynamics of mixed-phase clouds which are different for clouds above land compared to clouds above open water.

P23462, 15: Why 3 pixel were chosen as criteria? How many meters in vertical thickness are 3 pixel? The 3 pixel have to be consecutive or randomly distributed? What is the typical total number of all altitudes? (to have an impression what fraction mixed-phase clouds have to cover)

P23463, 5-23: This part reads like an introduction text and causes a break in reading fluency. Some parts are even repeated from section 1. I suggest to shorten and move sentences into section 1.

P23464, 9: DARDAR has a 500 m threshold? What about the ground based observations? Are they also limited below 500 m altitude? Please give the vertical range of the ground based observations at all sites. That is important to draw any conclusions from the comparison in Fig. 2.

P23464, 26: The authors present a similar cloud classification by NASA and show that there are only small differences. Why then using your own product when there is

already another similar available? Differences and a justification to apply a new own algorithm should be given in Section 2.

P23465, 16-26: Again this part reads like a kind of introduction and partly repeats what was written earlier in the manuscript. I suggest to shorten and move sentences into section 1.

P23466, 20: Summarizing open water and sea ice into one category can be a good but also a bad choice. Consider climate change and sea ice retreat this is a good choice as I would assume to find a signal of ice retreat in the cloud statistics. Otherwise cloud formation above open water and sea ice can be very different. These differences will be hidden when averaging over both surface types. Think about adding single statistics for sea ice and open water surfaces. Cloud formation above sea ice areas is much closer related to land surfaces. So land and sea ice might also be one combined category.

P23467, 5: "cloud" change to "clouds"

P23467, 16: Is the decrease in summer due to less low level MPC or an increase of mid-level MPCs?

P23469, 19: May a digital elevation model help to identify ground returns?

P23471, 20: Results for altitudes below 500 m have been considered to by contaminated by ground returns anyway and were before rejected from the data analysis. So I see no reason why data below 500 m is shown here. The discussion is meaningless if the data below 500 m is bad.

P23474, 15-18: This decrease of low-level clouds might also be observed in the data because warm clouds are neglected at all.

P23476, 22: Isn't it the mixture of cold (not warm) air and warm water over the North Atlantic which causes many of the low-level MPC?

P23477, 9: I would not agree to this conclusion! Only because there are similar occur-

rence patterns it does not mean, that the clouds are similar. Therefore the definition of mixed-phase applied in this study is to general.

MPC can form under quite different dynamic conditions and thus may have completely different microphysical and optical properties. The definition of MPC in this paper is just based on the coexistence of ice and liquid water and does not tell anything about the formation mechanisms of the clouds and their detailed microphysical and optical properties. Only because clouds have in average the same phase it does not mean, they are similar. E.g. mixed-phase clouds above ice and above open water have completely different dynamics!

So in-situ measurements of clouds above open water close to Svalbard can not be related to mixed-phase clouds above sea ice in any other Arctic area.

Figure 1: Decision on the very left side (Layer thickness). I would have assumed that it is the other way around. Thin clouds in my view are more likely to be pure ice clouds and mixed-phase clouds are in general thicker.

Figure 1: How vertically extended clouds are treated here? When the lidar signal is attenuated, all cloud parts below can not be classified anymore. How this is accounted for?

Figure 2: Legend of the plot is quite small and hard to read.

Figure 3: A similar projection for total cloud amount is needed for the interpretation. As F_{MPC} is only a relative number, figure 3 does not tell about the total number of occurrence. A higher fraction of MPC does not necessarily mean a higher total number of occurrence.

Figure 4: Single lines are very hard to distinguish.

Figure 4: If the retrieval at altitudes below 500 m is not trusted, you can't show results here.

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