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Interactive comment on "Characterization of Arctic mixed-phase cloud properties at small scale and coupling with satellite remote sensing" *by* Guillaume Mioche et al.

Anonymous Referee #2

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1 General remarks

The manuscript provides a broad analysis of airborne in situ measurements of microphysical and optical properties of Arctic low-lewel mixed-phase clouds. Data of four field campaigns are merged and presented with respect to different aspects in order to improve our understanding of this specific but most relevant Arctic cloud type. In a second part in situ measurements and satellite observations are compared. This analyze focus on the question if the spatial resolution of satellite to resolve all small scale aspects of mixed-phase clouds or if averaging may bias our view on these clouds.

The large data bases presented and analyzed in the manuscript and the derived pa-

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rameterisations are of high value for Arctic climate research. Also the case study on the performance of active satellite remote sensing will help to improve our understanding of the retrieval products and the interpretation of derived cloud properties. In this regard, the manuscript provides an important contribution to current and future research and is worth to be published.

However, in my opinion the manuscript lacks of some major issues which have to be reassessed in detail before publishing the manuscript. Especially the focus of the manuscript is not sufficiently clear to the reader. Two topics are presented but not combined in the manuscript. I therefore recommend to split the manuscript into two single contributions that strongly focus on the individual subjects. This will certainly enhance the visibility of both studies as both will suffer when presented in a single manuscript.

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 which result from the sometimes not well clarified differentiation of the two separate studies. I still kept most of my thoughts in the reviews as they might help to each of the single studies. I also have to admit, that I did not review the second study on the satellite comparison with the same depth as the first part of the manuscript, as I soon had the feeling that both parts should be split.

2 Major comments

Two individual studies

The manuscript presents two different parts with different application of the measured cloud properties. First all in situ cloud data are systematically analyzed and new parameterizations are presented. In the second part a completely different topic, namely a case study of satellite-airborne comparison is presented.

I do not see a large overlap, connection or synergy of both parts, except that measurements of the same instruments and field campaigns (only two out of four) are used. The approach of both parts is different: statistical analysis of a large data set versus comparison in single case studies. The satellite comparison study does not use the full data set described before and is a completely stand alone study. This is already obvious as the second part starts with an additional and separate introduction trying to justify the step from the statistical analysis to the case studies.

I was also confused by the misleading title of the paper which I guess is more related to the title of the satellite comparison in the second part of the paper. I don't see a characterization on small scales in the first part of the manuscript. Cloud profiles which are averaged over a relatively large area were analysed. Therefore, the title neglects the major part of the manuscript and is misleading.

Overall, I therefore recommend to split both parts of the manuscript and publish separately. The statistical analysis of the large in situ data set seems to be more mature and provides interesting results. That would be my first choice of the focus for this paper.

Nevertheless, I like the idea and the results of the second part, satellite comparison. The two plots show quite nicely how averaging may effect the cloud properties in terms of separating ice and liquid parts of the cloud. Therefore, I recommend to publish this in a separate publication. This would also enhance the visibility of this study. Keeping the comparison study here as a second part following the extended statistical analysis of the whole data set is remains kind of hidden.

"Small scale"

Section 3 is called "Small scale properties of liquid droplets and ice crystal particles within MPC" but averaged profiles are analyzed. These profiles can not resolve small scale horizontal variability. Also 100 m vertical resolution is not really "small" scale considering that typical mixed-phase clouds have a quite small vertical extend of a couple of 100 meters. Remove "small scale" for this section.

The title of the manuscript also refers to "small scale". This can not be attributed to the statistical analysis and only holds for the comparison with the coarser satellite data.

Focus of the data analysis

After reading the first section, I feel that the focus of the manuscript was not clearly defined in the beginning. The analysis of the data set looks slightly arbitrary. Some dependencies are analyzed campaign wise, some with the entire data set. Also the discussion or analysis with other parameters that might influence some of the found dependencies short.

To improve the outcome of the analysis, a stronger focus and master plan of how to analyse the data in a consistent way is needed. A more systematic and logic analysis of the data is necessary.

My general suggestion is to use the data set in order to decouple dependencies of different cloud microphysical properties to different environmental parameters. This is what the data set can be used for. Here, the separation between the single campaigns as often done in the manuscript is misleading. Why there are differences between the campaigns? E.g., because of temperatures. But, even when the microphysical properties may differ between the campaigns, the variability between different flights might be larger as the duration of the campaigns is likely longer than the synoptical time scale and conditions will change during a campaign. It is not the campaign name and year what matters it is rather the synoptic conditions and these might differ also within a campaign. Therefore, I suggest to not separate the data into different campaigns. Rather separate in different synoptic situation, e.g., temperature regimes, wind direction, vertical wind speed, etc.

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Aerosol Effect

The aerosol effect discussed in Section 3.4 is not really convincing. Even the authors rise doubt on their on conclusions. There is a lot of speculations in this paragraph, "*cloud*", "*may*", no airborne aerosol sampling is available and only few cases are used to testify the potential influence of the aerosol concentration. Finally, the authors kill their arguments by themselves in stating, that "*the aerosol concentration measurements have to be taken with care*", and "*Measurements of key parameters are obviously missing in the present study to accurately quantify the mechanisms responsible for the formation and growth of droplets and ice crystals within MPC*".

With this, I only can recommend to remove this highly uncertain and speculative analysis. You may provide the information on the aerosol background for completeness but not try to draw any conclusion on the aerosol effect.

3 Minor comments

L11: Title: You may add that the study is only on low-level mixed-phase clouds.

L12: Title: What is a "small scale"? 10 km or 1 m?

L18: Which season the campaigns have been conducted?

L21: "The ice phase is found everywhere within the MPC layers..." this is a somehow trivial statement, as a mixed-phase layer is defined by having both liquid and ice particles.

L58: Give a range of the scales for typical model grid boxes and the isolated pockets discussed by Korolev and Isaac (2003). This is important to know with respect to your investigation of small scale variability.

L64: After this paragraph I would expect a discussion on the different scales of observation. What are the scales of the cloud phase distribution inhomogeneities and what instruments can resolve it. I'm not sure, if this is the correct place, but somewhere this discussion should be added.

L67: Again, specify the "regional" scale.

L81: What scales are covered by the "microphysical scale"? mm? μm?

L89: "in situ" is sometimes written italic sometimes not. Be consistent.

L92: This means you implicitly assume differences in cloud properties for the different regions, western Arctic and Svalbard? State that directly and give a reference.

L110: I'm missing a summary of available similar data sets. In situ cloud properties of mixed-phase clouds have been reported since many years (e.g., Fleishauer, Lawson, Korolev, McFarquhar, Fridlind,...). What data is available? What is their limitation? What is missing on the data? And why this new data set is needed? What can it do better?

L125: It is a little irritating that you spend three numbers for four campaigns.

L128: Does the flight speed of the aircraft used in the different campaigns significantly differ? Would this have any impact on the spatial resolution if the measurements and according results?

L138: The areas have a slightly different latitude and therefore distance to sea ice edge. Could this change the cloud properties between the single campaigns?

L142: What is the horizontal range needed to sample a single profile? Additionally, you averaged profiles. When merging these data, for what area the measurements are representative then if you consider the horizontal inhomogeneities of mixed-phase clouds as discussed by Korolev and Isaac (2006)? (Korolev, A. and G.A. Isaac, 2006: Relative Humidity in Liquid, Mixed-Phase, and Ice Clouds. J. Atmos. Sci., 63, 2865–2880, doi: 10.1175/JAS3784.1.)

L165: Operated with or without Korolev tips? Is shattering a problem?

L176: "Liquid Water Content" - no capital letters.

L192: Give at least a conclusion of the Appendix here in the main text. I also would suggest to place Table A1 in the main part of the manuscript and not in the appendix. The uncertainty estimates are essential to interpret the measurements and should not be hidden in an Appendix.

L210: I was totally lost because the equations are not consistent. Until I realized that the equations are used for different altitudes. Indicate here for which range of z ($z > zb$ and $z < zb$) the equations are applied.

L220: How you can guaranty the number of 2000 observations? Can you tell a little about flight strategy? Continuous profiles or stair cases? What ascent/descent rates were flown?

L221: Typo: "ofbtained"

L229: remove "very". That's always relative.

L231: The numbers given in Table 2 are not the normalized altitudes, right? This is a little confusing, as you first introduced the normalized altitudes and the temperature profiles and then use the geometric altitudes again. I would recommend to reorder this.

L231: If the temperatures did differ that strongly, do the cloud base and top altitudes also vary between the single campaigns? I would also split this up and somehow combine the numbers with Figure 2 instead of given the numbers in Table 2. That would be more illustrative.

L232: Well, for cloud base the observations and literature are not consistent anymore. The mean value of your study is off the range observed by McFarquhar et al. (2007).

L246: Is there a way to give a number from which fraction of IWC/TWC the influence of ice might be visible in the scattering data?

L250: These two thresholds of g differ. Do I understand correctly, that for the range 0.8-0.83 both, liquid and ice crystal properties were derived? Please add this shortly to clarify to the reader.

L253: I don't think, that Table 3 is needed, when you properly describe the matrix in the text here. Except of the range g=0.8-0.83 everything was given in the text.

L254: I would suggest to place this essential sentence a bit earlier. Maybe at line 248.

L265-267: You don't have to repeat this. Remove sentence.

L276: "droplet number size distribution" is the correct name of the quantity.

L297: Here only particle larger 100 μm are considered. But all other plots and analyzed parameters include particles less than 100 μm, e.g., number size distributions and D_{eff}. Be consistent or give a reasons why not to be consistent.

L305: Introduce acronyms ISDAC and MPACE.

L305: A more detailed discussion with this available data would be helpful.

L319: In this analysis the data of the four campaigns are merged. Is there evidence for differences in the particle shapes considering the strong temperature difference during ASTAR 2007?

L337: add "at low altitudes" for "...MPC in the Svalbard region at low latitudes."

L345: This section analyses phase function and g and introduced these quantities. However, g values were already extensively used in the sections before to discriminate the cloud particle phase. The presentation of g and discussion has to be given first.

By presenting g and phase function at this position of the manuscript, the analysis does not give any further new results and insight. The general location of ice and liquid particles was already discussed before. I therefore, recommend to shift this discussion somewhere earlier, before the first g values are used.

L374: I would also consider the sampling method to cause a mixing or averaging at cloud top. Assuming a cloud with variable cloud top altitude (even if only a couple of meters) and the aircraft ascending/descending through cloud top with a certain horizontal speed. If the ascending/descending rate is to low, then different parts of the horizontal inhomogeneous cloud top are averaged. This automatically will lead to lower LWC when averaging cloudy and cloud free patches.

L376: Typo: "an" ice crystal growth...

L382: What about vertical wind speed? Was this measured and can this be included in the analysis. Especially for such microphysical processes, the updraft velocity is important (Korolev, A. and P.R. Field, 2008: The Effect of Dynamics on Mixed-Phase Clouds: Theoretical Considerations. J. Atmos. Sci., 65, 66–86, doi: 10.1175/2007JAS2355.1.)

L383: LWC: Shortly explain how this was calculated. On what measured data the calculation is based?

L392: In what there is coherence with previous work? Such unspecific statements occur more often throughout the manuscript. It is always mentioned, that there is agreement, but it is not discussed in detail, what i particular agrees. Please look though the manuscript and be more specific.

L466: This is a repetition. It was just written before.

remove the whole introduction of 4.

from a modeling perspective also other parameter are needed to be correlated with the cloud properties. But these are not adressed here because no measurements are available. So I would not start with this motivation...

L4:

L4:

L484: How does this compare to standard models of the relationships between optical thickness and LWP. Actually these standard model equations include the particle size, which was not done here.

L489: The upper range is missing for the temperature range of this study.

L492: "very few previous studies": There are plenty of studies analyzing the properties in Arctic MPC although many use ground based remote sensing (Shupe, De Boer, etc). Some of them are also given in the test below.

L503: What is the hypothesis in linking cloud top temperature and IWP, LWP?

L516-525: This paragraph is rather part for the motivation. Here in the main text results should be presented and discussed. Add this discussion (state of the art microphysical processes...) to the introduction.

L529: Give the uncertainties for the fit parameter. There are statistical means to calculate these. Only if the uncertainty of the fit parameter covers the parametrization by Meyers et al. (1992) you can judge if both data sets agree with each other.

L531: Remove the last sentence. This is more useful for the conclusions and outlook.

L537: rewrite: "...liquid fraction of individual measurements can be...". The liquid fraction of an entire MPC would be LWP/TWP. Differentiate both parameters of liquid fraction for indivudual measurements and for the entire cloud. Spend symbols for both and introduce carefully.

L570: That first has to be shown! Especially with the good agrement with available parametrization, what is in particular the benefit of the new data set?

L571-575: This fits better into the motivation. Move to introduction.

L760: Conclusions: Of course it is a question of style. But if you have such long conclusions I see no benefit in putting these into a numeration. It could also be presented by regular paragraphs.

L762-765: The first two sentences are not a conclusion. Try to shorten and focus your conclusions.

L792: This first has to be shown. You only presented the parameterizations but did not apply to remote sensing nor modelling.

L802: "Globally"? You just looked at 4 flights.

L1030-1033: Reference is duplicated.

Figure 1: It would help if you indicate where the single profiles have been collected. The total flight paths are more or less irrelevant as they do not show where the profiles are measured.

Figure 3 and 4: Figures and labels are way to small! Even zooming in to 400%, I can not read the label. Spend more space for these figures. Maybe a 2x2 arrangement would help.

Figure 5: The liquid droplets shown here are measurements from the FSSP? Or do you found liquid droplets larger than 100 μm with the CPI?

Figure 5: Between 0 and -3°C almost only liquid droplets are found. Is this liquid also the liquid droplets at cloud top in the upper panels? May be only measured above the inversions? Or is this smaller fraction of liquid and cloud top compared to high temperatures only due to bad statistics?

Figure 6: This plot shows an average over all campaigns. Are there differences between the campaigns? I would assume similar differences compared ot the LWC and IWC profiles.

Figure 9: I'm not sure, if the log-scales somehow automatically gives a "good" fit. In linear space larger differences might be visible.

Figure 9: Typo: Write "LWC" for the lower equation, liquid droplets.

Figure 9: x-title: write "extinction coefficient"

Figure 10: In Fig. 8 cloud top temperature was given on the x-axis, here on y-axis. Make this consistent.

Figure 11: Panel a: Here the campaigns are not separately displayed. Why?

Figure 11: Panel b and c: How there can be error bars beyond 100%?

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