

A review of "Characterization of the cloud microphysical and optical properties and aerosol-cloud interaction in the Arctic from in situ ground-based measurements during the CLIMSLIP-3 NyA campaign, Svalbard" by Guyot et al. (acp-2017-672)

General Comments:

This paper examines cloud microphysical and optical characterization encountered during the ground based CLIMSLIP-NyA campaign performed in Ny-Alesund, Svalbard. Three different scenarios are identified: the Mixed Phase Cloud (MPC), snow precipitation and Blowing Snow (BS) events. Aerosol cloud interaction is also investigated to assess the influence of anthropogenic pollution transported into the Arctic. The results presented in this study are found to be generally consistent with previous studies.

Overall, I found that while the study is reasonably well presented, the English needs to be improved. There are many grammar errors throughout the manuscript (I point out a few in my specific comments below), which warrant a very careful proofread. Also, I have difficulties in finding real new insights in this study. The authors reiterate a few times that their results are (qualitatively) consistent with previous studies, but what is the new contribution that this study makes? This is not very clear to me. In addition, although the study is primarily interested in ground-based in-situ measurements, a description of the synoptic meteorology (perhaps with the help of synoptic charts and some satellite observations) during the events is needed. This will provide necessary information on the synoptic backgrounds under which these events occurred, and will help explain some of the differences observed in the microphysical and optical characteristics. Finally, the uncertainties of some of the analyses need to be better explained.

In my view, a major revision is needed before the manuscript can be considered for publication in ACP.

Specific Comments:

Abstract

1. Line 33: "presents" should be "present".
2. Line 34: "mostly of" should be "mostly".

Introduction

3. Line 56: maybe change "feedbacks" to "feedback".
4. Line 57: "circulation" should be "circulations".
5. Line 65: You say that "The Svalbard region is an exception where MPC are the most frequent cloud independent of season...". But are the clouds over this region representative of the Arctic clouds? I note you comment later on that the aerosols are representative of the aerosol properties in the Arctic (line 93).
6. Line 65 and 69: "MPC" should be "MPCs".
7. Line 75: what do you mean by "umbrella effect"?
8. Line 77: should "suspects" be "suggests"?
9. Line 105: "complexified" should be "complicated".
10. Line 107: "ice content" and "liquid content" should be "ice water content" and "liquid water content".
11. Line 108-109: perhaps replace "cloud cover in lifetime" by "cloud lifetime".

Site & instrumentation

This section is good but some clarifications are needed.

12. Line 135: the measurements were made at a Mountain station – are there any orographic effects that could potentially affect the representativeness of the observations? Also, a map showing the location of the stations as well as topography would be useful.

13. Line 179-183: The uncertainties of the classification of particle morphology need to be discussed. You said that a manual classification was also performed but no details are provided. Also, how does this affect your results? No discussion referring to this issue is presented later on.

14. Line 194: “relied related...”???

15. Line 195: “Due to high discrepancies...” high discrepancies of what?

16. Line 229: “...not possible for two reasons not developed further.”?? I don’t understand this sentence.

17. Line 245: “has” should be “have”.

18. Line 246: how does the sampling rate affect the accuracy of the measurements? Please provide some context.

Identification and characterization of the study cases

A description of the synoptic meteorology during the events can be presented here (see my general comments). Synoptic charts and some satellite observations (such as MODIS and CloudSat/CALIPSO if available) would be very useful. These satellite observations would provide complementary information that cannot be derived from the ground-based measurements, such as cloud-top temperature/height/phase, etc.

19. Line 278: “On the same time” should be “At the same time”.

20. Line 287: “solidification point”. Do you mean “freezing point”? Also, you say here that “liquid particles were always supercooled droplet” but previously you say that “No droplets were sampled”? (line 282).

21. Line 305 and Figure 4: it’d be useful to also present the results as a function of temperature to appreciate if the pattern persists across the temperature range. This result can also be compared with previous studies (e.g. Korolev et al. 2003; Ahn et al. 2017).

22. Line 313: suggest insert “for Arctic clouds” after “scientific literature”.

23. Line 327 and Figure 5: what causes the large discrepancies between the FSSP and CPI measurements in the overlapping range?

24. Line 334: “in expense” should be “at the expense”.

25. Line 334-336: what other mechanisms could potentially explain the ice crystal growth? Are there characteristics of secondary ice production?

26. Line 355: insert “in” before “Figure 6.a.”

27. Line 362: “accurately measured”?? But you mentioned before (line 182-183) that a manual classification was also performed due to some malfunctions of the automatic classification?

28. Line 395-399: were these cases sampling different precipitating systems or cloud types (associated with the temperature differences)? Again, synoptic charts and satellite observations could be useful here.

29. Line 399-403: isn’t the dynamics (e.g. deep convection vs. shallow convection, convective vs stratiform precipitation) supposed to play an important role here? To better understand the temperature effect, as argued by the authors, it is necessary to present Figure 8 as a function of temperature, too. Once again, what are the synoptic processes associated with these events?

Aerosol-cloud interaction in the Arctic

30. Line 449: the authors should explain why only the LMPC cases are examined in this section.

31. Line 456-462: did you use a reanalysis dataset to drive the WRF model? What is the vertical resolution of the model? How many levels are within the boundary layer? Quite often the representation of the boundary layer in the model is questionable (partly due to the low resolution). How does this affect the trajectories?

32. Line 619: the assumption of a constant LWP can hardly be valid, especially “the station is in the liquid layer or the mixed phase layer” as you mentioned in line 279-280.

Line 648-653: I’m not sure if the glaciation and riming indirect effect can be evaluated simply using the statistics in Figure 14, as the results can be complicated by scavenging effect, too. Also, how reliable is the IWC measured by the CPI? Further, aerosols may also serve as ice nucleation particles which could facilitate precipitation and ultimately decrease droplet number concentration. This effect is not addressed in the analysis.

Summary and conclusion

33. Line 719: insert “for two of the LMPC events” after “was performed”.

34. Line 732-733: again, I don’t think this conclusion is valid based on the analysis presented.

Annex: Characterization of the Blowing Snow (BS) cases

Is there a reason why this analysis can’t be a part of the main study?

35. Line 766-767: I don’t understand why it is possible. Can’t snow develop in clouds or near cloud base?

Ahn E., Huang Y., Chubb TH., Baumgardner D., Isaac P., de Hoog M., Siems ST., Manton MJ. 2017. In situ observations of wintertime low-altitude clouds over the Southern Ocean. *Q. J. R. Meteorol. Soc.* 143:1381–1394.

Korolev AV, Isaac GA, Cober SG, Strapp JW, Hallett J. 2003. Microphysical characterization of mixed-phase clouds. *Q. J. R. Meteorol. Soc.* 129: 39–65.