Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-918-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterisation and surface radiative impact of Arctic low clouds from the IAOOS field experiment" *by* Julia Maillard et al.

Anonymous Referee #1

Received and published: 18 October 2020

The authors present results of the IAOOS field experiment which took place from 2014 and 2019 in the central Arctic. They focus on lidar measurements which were performed on drifting buoys and analyse cloud occurrence and further cloud properties based on this data set. They also look into the radiative fluxes measured during the N-ICE campaign, where collocated buoy observations were made as well, and analyse the different radiative modes observed in this time period. Here, they compare the observations to ERA5 reanalysis data.

Enhancing cloud observations in the Arctic is crucial to better understand Arctic clouds, they radiative impact and their impact on the Arctic climate system. Especially in the harsh Arctic environment and especially in the Central Arctic, it is quite challenging to acquire such data. Using drifting buoys with such instrumentation is quite impres-

Printer-friendly version

Discussion paper

sive. The authors discuss the challenges of such observations and also the limitations. However, also the retrieved data is limited and lacks spatial (as the authors mention) and temporal coverage. This needs to be kept in mind when analysing and discussing the data. A maximum of 4 lidar profiles per day cannot provide robust cloud statistics. However, this is how the authors sell the results. In particular, monthly statistics based on this data, in particular for those months where the number of profiles is even less than 100, don't seem to be reliable. At ground-based surface observatories, continuous observations can be performed. Assuming a typical 1-min resolution of lidar measurement, this would result in at least 1440 profiles per day. What if only 4 measurements, i.e. 4 random snapshots of clouds at one day, were available instead? Would they capture the cloud statistics based on the high-resolution data? Don't get me wrong: I think that this data set is of high value but the representativity needs to be critically discussed. This is partly done in the manuscript but needs to be enhanced. During the N-ICE campaign, a micropulse lidar (MPL) is available. I strongly suggest to also include a section showing the comparison between the results of the buoy lidar and the MPL. This would provide more insight in the representativeness of the buoy lidar cloud observations. Please find in addition my specific comments below.

Specific comments:

l 5: "Cloud frequency is globally at 75%...": unclear what globally means. please be more specific (which time period exactly, region).

l7: "On the whole, the cloud cover is very low. . .". Misleading. Could be read as: Cloud cover (=cloud fraction) is low (=small). Rather use "Cloud base height is very low. . ."

ll 26 ff: You could also mention the results of Mioche, G., Jourdan, O., Ceccaldi, M., and Delanoë, J.: Variability of mixed-phase clouds in the Arctic with a focus on the Svalbard region: a study based on spaceborne active remote sensing, Atmos. Chem. Phys., 15, 2445–2461, https://doi.org/10.5194/acp-15-2445-2015, 2015

ll 41: Concerning ground-based cloud observations, you could mention the studies by

Shupe, M. D., V. P. Walden, E. Eloranta, T. Uttal, J. R. Campbell, S. M. Starkweather, and M. Shiobara, 2011: Clouds at Arctic Atmospheric Observatories. Part I: Occurrence and Macrophysical Properties. J. Appl. Meteor. Climatol., 50, 626–644, https://doi.org/10.1175/2010JAMC2467.1.

and

Shupe, M. D., 2011: Clouds at Arctic Atmospheric Observatories. Part II: Thermodynamic Phase Characteristics. J. Appl. Meteor. Climatol., 50, 645–661, https://doi.org/10.1175/2010JAMC2468.1.

Also, be aware of the enhanced cloud observations at Ny-Ålesund:

Nomokonova, T., Ebell, K., Löhnert, U., Maturilli, M., Ritter, C., and O'Connor, E.: Statistics on clouds and their relation to thermodynamic conditions at Ny-Ålesund using ground-based sensor synergy, Atmos. Chem. Phys., 19, 4105–4126, https://doi.org/10.5194/acp-19-4105-2019, 2019.

Ebell, K., T. Nomokonova, M. Maturilli, and C. Ritter, 2020: Radiative Effect of Clouds at Ny-Ålesund, Svalbard, as Inferred from Ground-Based Remote Sensing Observations. J. Appl. Meteor. Climatol., 59, 3–22, https://doi.org/10.1175/JAMC-D-19-0080.1.

ll 45 ff: concerning shipborne and airborne observations it is also worth mentioning the ACLOUD and PASCAL campaigns:

Wendisch, M., and Coauthors, 2019: The Arctic Cloud Puzzle: Using ACLOUD/PASCAL Multiplatform Observations to Unravel the Role of Clouds and Aerosol Particles in Arctic Amplification. Bull. Amer. Meteor. Soc., 100, 841–871, https://doi.org/10.1175/BAMS-D-18-0072.1.

l 64: "extract a 5-year statistics of the Arctic cloud cover": This is overstated. It has to be made clear that this is not a robust statistic with respect to spatial and temporal coverage. Be more precise here: e.g. "cloud cover along the track of the drifting buoys in the central Arctic for the months of. . ."

l 89: As mentioned before, having only a maximum of four lidar measurements per day is a very, very low number. The representativity needs to be critically discussed. Not only once, but also when presenting the results.

l 101: “red line”: ambiguous, better “red circle”

ll 106: information on N-ICE campaign:

Please include more information about the campaign data set and the auxiliary instrumentation, e.g. detailed information about the radiation sensors (“four component radiometer”). What kind of instruments exactly? What are the instrument specifications? I assume they both down- and upward radiative fluxes are provided, right? Where is the instrumentation exactly installed? Distance of the instruments to each other?

Why is the information of the MPL not used in addition? The measurements of the MPL should be set in to context to the buoy lidar observations.

l 111: “April to June”, please add 2015

l 114: please provide a reference for ERA-5

l 116: “L1”? Please be more specific

ll 122 ff: How does the lidar window frost impact cloud detection? You could state at the end of section 3.1.1 what this means for the accuracy of the cloud observations?

l 160: Equation 1: please introduce all variables!

l 174: How is the threshold of 1.1 chosen? What is the impact on cloud detection?

l 190 Tc has not been introduced

l 192: Equation 2: make sure that all variables are introduced, e.g. alpha_p

l 224: “Global” is misleading: Why not simply name it as it is: “average monthly cloud cover from March to December”

ll 228 ff: just a comment here: low clouds frequently occur in the Arctic and it is especially difficult for satellites to capture these clouds also from active instrumentation, e.g. due to blind zone, ground clutter. Ground-based observations are thus crucial to capture these low clouds. It is true that for ground-based observations the sensitivity is highest in the lower atmosphere but the combination of cloud radar and lidar can very well capture the whole atmospheric column! → (ll 235-236).

ll 236: which instruments were used in the Hahn et al study?

ll 240-251 and Fig.2: You really need to discuss your results in conjunction with the number of measurements (as seen also in Table 2).The very low cloud occurrence in March, April, November and December is very suspicious. I would not overinterpret the results here. Please consider the representativity of the data. Discussion of "Interannual variability": I would also be careful here. I am not convinced that based on the number of data, any conclusions can be drawn here.

l 256: "lacks spatial coverage": but also temporal coverage! Again, a comparison between MPL data and the buoy lidar data are crucial to give more confidence in the temporal representativity of the results. You really need to draw your conclusions more carefully.

Table 2: Are the numbers in each months are for all buoy drifts/years?

l 270: "non-significant": did you perform a significance test?

l 277: This is a too strong statement. Please rewrite.

l 281: "(note however. . .)". Thank you that you mentioned that point here but not sufficiently discussed and highlighted.

l 286: "significant difference": tested?

l 287: "shoulder months": unclear, please do not use "shoulder months" throughout the manuscript and mention the months explicitly.

l 300: Again, how representative are the 222 profiles?

Fig. 4. “a” and “b” missing in plot Do you calculate the median and percentiles from e.g. 6 values? See for example March

l 337 ff: I am not convinced that you can simply set the COD to 2 for high-IAB cloud layers. You simply do not know the COD in these cases. You state that this is helpful for examining the seasonal trend. But also this trend has large uncertainties then.

ll 382-383: Why was the information from the MPL not exploited as well?

l 389: “. . .due to the higher surface temperatures in spring/summer.” Please elaborate on that.

Fig.5 b) and c) Remove “Measured” in xlabel since also ERA5 data are shown Explain RC1, RC2, OC in figure caption. Rather provide a detailed section on where the radiation sensors are installed in the text than mentioning it in the figure caption.

l 404, Equation 5 What about the surface emissivity? Should be included in equation 5.

l 420: “. . .partly compensated by a 14 Wm-2 error in LWu in April/May. . .” Where can I see this? It would be interesting to include a plot of LWu.

Figure 6: Please use different line styles for the different cloud optical depth. Remove “Evolution” from figure caption: “Longwave downward radiative flux as a function of . . .”

ll 425: Which kind of satellite data are assimilated in ERA5 exactly? Please provide more details here which underline your hypothesis.

ll 441 ff: It is totally unclear why you need to come up with parameterizations or estimates of the downward radiative flux components. These are measured, aren’t they? What is the intention of this part?

l 473: “shortwave cloud albedo effect”

ll 476-478: “In contrast, the longwave warming effect,. . ..” Maybe it would be good to remind the reader that this is the difference between the dashed and solid line in Fig.6 (as far as I understood).

ll 482-484: “This explains that. . .” Can you elaborate on that a little bit more? Unclear to me.

ll 489-491: “Equations 6 and 7 were inverted to calculate. . .” Can you explain in detail how you did it? In Eq. 7, COD is not directly included. It might be good to remind the reader how this is connected to transmittance. Do you take F0 from your fitted function?

Table 4: What are the uncertainties of the derived COD values?

ll 521-522 and this section: “The results show a significant seasonal variation. . .”. Overstated due to the reasons mentioned before. Also “Monthly cloud frequency is minimum in March/April and November/December. . .” A discussion of measurement and sampling uncertainties is needed here! I doubt that the results a robust for these months.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-918, 2020.

Printer-friendly version

Discussion paper