Response to the reviewer comments on the manuscript:
"Variability and properties of liquid-dominated clouds over the ice-free and sea-ice-covered Arctic Ocean"
[acp-2022-848]

We thank the two anonymous reviewers for diligently reading and carefully reviewing our manuscript and providing us with useful comments and suggestions to improve the quality of the manuscript. A list of all reviewer comments and questions (written in *italics*) as well as our response (written in regular) is given below. Whenever we provide information in which line changes were made we refer to the line numbering of the revised manuscript.

Comments Reviewer 2:

Summary:

This manuscript presents results from arctic airborne campaigns (ACLOUD and AFLUX), where they measured low arctic clouds over sea ice and open sea. The manuscript is very well written and is quite pertinent to ACP, particularly with respect to advances in the Arctic low cloud, which remains highly difficult to measure.

This is great manuscript to read, however there are a few minor comments to address, mostly on some clarification of some points (see list below). After these minor comments are addressed, it is recommended for publication in ACP.

General Comments:

1. *There is combined measurements of in situ cloud drop/ice crystal sizes and remote sensing measurements. While this may be outside the scope of the paper, at least a mention on the actual shape of the size distribution should be included. It would be interesting to see how that matches the commonly expected gamma distribution, with alpha =7 that are typically used in Nakajima & King bi-spectral retrievals for quantifying the effective radius.*
   The shape of particle size distributions are shown and discussed in Moser et al. (2023), which we refer to in the manuscript. We think that a detailed examination of size distributions is not necessary for this manuscript. The size distribution shape has only a minor impact on the cloud radiative properties compared to the cloud phase and LWC.

2. *The date format for ACP is dd month yyyy, e.g., 25 July 2007. There are a few instances of a varying date format.*
   We went trough the manuscript and changed all the dates to the right format.
3. *The spectral slope in the measured snow albedo leaves to believe that there may be other factors, like haze or aerosol layer, present. While that may be not so important given much of the remote sensing is focused on the near infrared regions, at least a mention of the haze/aerosol conditions should be made, and if available more details*

on how that would impact these retrievals. A note that haze was present is found later in manuscript (line 313). Potential impact of this layer should be explored
When performing the Albedo measurements, the research aircraft was flown at an altitude of 150 meters above ground and below clouds, which makes the presence of a haze layer very unlikely. It is more probable that there was a moistening process taking place at the surface, such as melting snow, as the Albedo simulations were only conducted for dry snow conditions. This observation is consistent with the melting snow conditions shown in Figure 2 of Light et al. (2022).
We now mention that in the manuscript:
Line 149: "It is obvious that the differences between the measurements and the simulations change spectrally, which might be caused by a non-homogeneous stratification of snow with different grain sizes or a moistening process taken place at the surface, such as melting snow. The latter one seems more likely, because the albedo simulations were only done for dry snow conditions and the measurements are consistent with observations from Light et al. (2022) and Rosenburg et al. (2023)."

Specific Comments:

1. *Line 24: What is (TR 172)? If it is a reference, then it is not in the reference list.*
   It describes the project number. To keep it simple, we removed it here and just mention it in the acknowledgments.
2. *Line 38: Please add the caveat that cloud top properties is from passive remote sensing from reflectances, not all passive remote sensing techniques, see Platnick 2000. Additionally, some active (lidar) techniques are also limited to the topmost portion of the cloud. There are transmitted-light based passive remote sensing that have a more even distribution of sampling through the cloud. e.g., McBride et al., 2011, LeBlanc et al., 2015, and Smith et al., 2017*
   Thanks for pointing that out. We changed this sentence and the following sentence to:
   Line 38: "However, the information retrieved from passive remote sensing using reflectances often is dominated by the cloud top properties (Platnick, 2000). Unfortunately, passive remote sensing retrieval from reflectances of Arctic boundary-layer clouds is challenging due to the unknown vertical distribution of ice particles in the typically…"
3. *Figure 2: For that many drop sondes, one wonders how representative are these averages? What is the deviation to the median, and the standard deviation?*
   We added horizontal bars of the standard deviation to the plot, which represents the variability of the dropsonde measurements (see Figure 3, below).

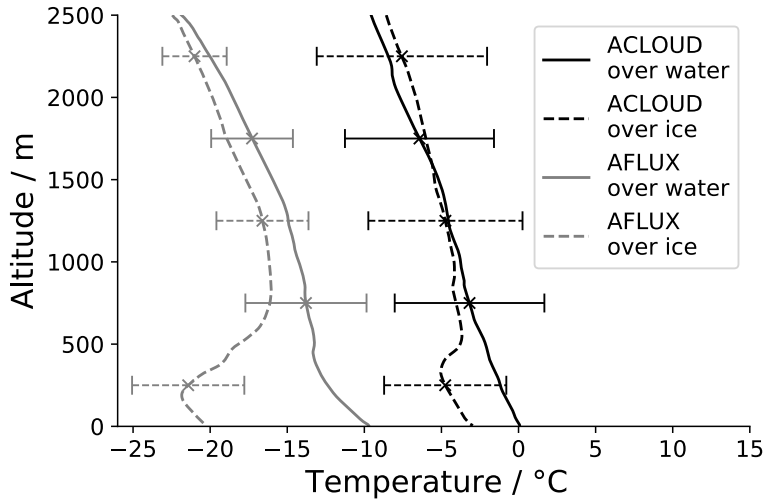

Figure 1: Averaged temperature profiles of all launched dropsondes during the ACLOUD (black) and AFLUX (gray) campaign over water (continues lines) and ice surface (dashed). The horizontal bars represent the standard deviation.

4. *Line 73: I'm not certain that the reference to the AISA Hawk instrument requires the book Pu 2017.*
   Agreed. We replaced it with Ruiz-Donoso et al., 2020.

5. *Line 77-79: Why is there missing measurements? Instrument issues, lack of cloud, or measurement quality is not sufficient?*
   The files, which the AISA Hawk instrument produces are very large and need a lot of storage capacity. Therefore, we only start recording when Polar 5 is flying above clouds. For this reason, we don't record data e.g., inside clouds or above land surface. To make that more clear we added:
   Line 80: "Due to storage capacities AISA Hawk data are only recorded when clouds are present below the aircraft."

6. *Line 84: How accurate are the simulated downwelling irradiance? Did you remove the conditions with high clouds? What were the sun angles modeled?*
   Yes, the downward simulations can and were only used when no clouds were present above the aircraft. In these cases, the accuracy of simulations is high for the downward irradiance as atmospheric conditions measured by radiosondes (Ny Alesund) and aerosol optical depth (airborne sun photometer) were implemented in the simulations. An accuracy analysis of airborne measured downward irradiances is discussed by Ehrlich et al. (2023).
   We added following sentences:
   Line 89: "According to Ehrlich et al. (2023) the accuracy of downward simulations is high as atmospheric conditions measured by radiosondes (Ny-Alesund) and aerosol optical depth (airborne sun photometer) were implemented in the simulations. Within libRadtran we used the radiative transfer solver DISORT2 (Discrete Ordinate Radiative Transfer, Stamnes et al. 2000) and performed the simulations of the upward radiance for solar zenith angles between 55° and 69°. Azimuth angles were adjusted depending on measurement time, location and attitude of the research aircraft."

7. *Section 2.4: What is the expected uncertainty in the combined in situ cloud probes for effective radius, LWC and IWC?*
   To describe the uncertainty of the in-situ instruments we adapted the paragraph to:
   Line 117: "In this study, the reff calculation is based on all observable cloud particle sizes, the LW C is calculated using particles smaller than 50 μm (CAS data) and IWC using particles larger than 50 μm (CIP and PIP), which is appropriate for Arctic mixed-phase clouds (McFarquhar et al., 2007; Korolev et al., 2017). Uncertainties of in-situ cloud measurements strongly depend on the microphysical cloud properties. In liquid clouds, the droplets are sized by the CAS, which has a range of 10-50 % uncertainty (Baumgardner et al., 2017), while in ice and mixed-phase clouds the sizing is dominated by data from the optical array probes which have an uncertainty of 20 % (Baumgardner et al., 2017; Gurganus and Lawson, 2018). In stratiform liquid and mixed phase clouds, the calculation of the LWC is subject to an error of 20 % (Faber et al., 2018) and for the IWC an error of 50 % (Heymsfield et al., 2010; Hogan et al., 2012) is assumed. For the in-situ data used here, a description of the processing methods and the derivation of microphysical cloud properties are described in detail by Mech et al. (2022) and Moser et al. (2023)."

8. *Line 121: How low were the sun angles? Arctic often suffers from sun being near the horizon which are hard to model and measure.*
   We changed this sentence to the following and added some information:
   Line 133: "Solar zenith angles (72° to 82° during AFLUX and 55° to 69° during ACLOUD, according to Wendisch et al. (2022b)) and azimuth angles were adjusted for each simulation, depending on the location, altitude, and measurement time of the airborne measurements."

9. *Line 137: The spectral shape of the measurements vs the modeled snow albedo, particularly in the visible, (shorter wavelength range), seems to indicate that there is something else ins the measurement scene that is not accounted for by the model. Is there any indication of aerosol near surface? Additionally, there may be issues with the Langley scattering in the modeled radiances. At the very least, please explain why you have solely attributed the differences to snow grain size and the stratification.*
   The aerosol conditions during the flights were rather clean as indicated by Lidar and sun photometer measurements. This makes it unlikely, that aerosol particles will have impacted the surface albedo measurements. Aerosol particles sedimented into the snow are known to have significantly smaller effect (Donth et al., 2020, Warren 2013). When performing the Albedo measurements, the research aircraft was flown at an altitude of 150 meters above ground and below clouds, which makes the presence of a haze layer very unlikely. It is more probable that there was a moistening process taking place at the surface, such as melting snow, as the Albedo simulations were only conducted for dry snow conditions. This observation is consistent with the melting snow conditions shown in Figure 2 of Light et al. (2022).
   We now mention that in the manuscript:
   Line 149 :"It is obvious that the differences between the measurements and the simulations change spectrally, which might be caused by a non-homogeneous stratification of snow with different grain sizes or a moistening process taken place at the surface, such as melting snow. The latter one seems more likely, because the

albedo simulations were only done for dry snow conditions and the measurements are consistent with observations from Light et al. (2022) and Rosenburg et al. (2023)."

10. *Line 150-152: This is good to identify potential 3D radiative transfer issues, however the abstract and other sections of the text do not make such a distinction, and presents the effective radius and LWP as equally valid. Maybe some bounding of the expected error for Ref, tau, and LWP should be mentioned. A citation might be all that is needed, like Schäfer et al., 2015.*
    Good point, we added following reference:
    Line 168: "As shown by Horvath et al. (2014) the 3D radiative effects are less pronounced in the retrieved LWP_{reff} compared to the optical thickness."
11. *Line 215: grammar error: "obtaines in"*
    We changed it to "results in".
12. *Figure 8 gives a great statement to how well the filtering process is successful.*
    Thanks, we like it too.
13. *Line 277: How many days/cases does the 2% of the data represent?*
    The two percent represent different sections over several days, from 23 March 2019 to 11 April 2019.
14. *Line 355: many question marks: bad format or is the author unsured that the document is in preparation?*
    Thanks for seeing this. The question marks were a reminder that we need to put the right reference here, what we missed to do for the initial submission. We wanted to wait to publish the dataset until we got the reviews for this manuscript, in case we needed to change anything in the processing method.
15. *Data availability: There is no link to the access of the data, but rather a list of papers that describe it.*
    The references Klingebiel et al. (2023a) and Klingebiel et al. (2023a) will be published at PANGAEA and link to the dataset.

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