Responses to Reviewer 1

Review of « Measurement Report: Differences in cloud optical and microphysical properties in the Arctic and Antarctic derived using thermal infrared spectroscopy », by Joseph Hung et al. Review by Quentin Libois.

General comments

This paper presents datasets of cloud optical and microphysical properties derived from remote sensing observations from the ground (mostly AERI and lidar) at two polar sites, namely Eureka (Nunavut) and McMurdo station (Antarctica). At Eureka the measurements cover the period 2006-2022 while at McMurdo it mostly consists of 2016. Using a similar retrieval algorithm (CLARRA) to estimate cloud optical depth, phase partition, and liquid and ice effective radii, the variability, average values and seasonal cycles of these properties are analyzed and compared for both locations. Beyond factual results, the paper discusses the differences obtained, pointing to distinct physical processes, atmospheric conditions, but also potential observation biases.

The paper presents an original dataset of cloud observations, certainly unique for such remote locations. It is overall well written, although a better distinction between the factual results and the interpretations would probably help the reading. More generally, reordering may improve the quality at many places. This paper is meant to be a "Measurement Report", hence does not have the physical insight of a Research Article, which at some occasions is a bit disappointing. Yet the presented dataset can certainly be very valuable for the wider scientific community, and probably I think the paper fits within ACP scope, and could be published after the points below have been addressed.

The authors thank Dr. Quentin Libois for their thorough review and suggestions. Point-by-point responses are provided in red to each comment below. Text that has been added and/or changed in the manuscript is given in blue. Line numbers refer to the location of changes in the revised manuscript, unless otherwise noted.

Specific comments

1) As the main material of the paper is the datasets, it would be worth better demonstrating that they have never been published before. In practice, several papers have used the same observations (in particular at Eureka), so this needs clarification, and the references to previous work should be more exhaustive.

We have added text to illustrate that the datasets presented are unique in their length (especially the nearly 20 years at Eureka) and that this work presents the first use of CLARRA beyond a case study. At Eureka, retrievals of the same/similar quantities have been performed (Cox 2013, Shupe 2005, 2015, De Boer 2009), using AERI measurements which have been cited in the introduction, but many of these studies also use data from other instruments, which have either been discontinued at Eureka, or have suspect data quality due to lack of maintenance (e.g., the microwave radiometer).

The following text has been modified in the Introduction, on lines 93 – 100:

This study has two goals. The first is to obtain a long-term record of cloud microphysical and optical properties, which are retrieved from downwelling thermal emission measurements collected since 2006 from AERI instruments in the Canadian High Arctic at PEARL, in Eureka, Canada. The second goal is to compare decadal cloud property retrievals at PEARL to similar retrievals from measurements made during the 2016 ARM AWARE program at McMurdo Station, Antarctica (Lubin et al., 2020). The first objective extends the continuous cloud observation record at PEARL and complements previous studies of Arctic cloud microphysics based on infrared emission and lidar measurements (Turner, 2005; Cox et al., 2014) by providing a nearly 20-year observational dataset, while the second objective investigates differences between Arctic and Antarctic cloud properties by comparing observations from the AWARE campaign to the multi-year cloud record from Eureka.

We have also added the three following references to previous work that were previously missing, related to cloud remote sensing at Eureka:

Blanchard, Y., Pelon, J., Eloranta, E. W., Moran, K. P., Delanoë, J., & Sèze, G. (2014). A Synergistic Analysis of Cloud Cover and Vertical Distribution from A-Train and Ground-Based Sensors over the High Arctic Station Eureka from 2006 to 2010. *Journal of Applied Meteorology and Climatology*, *53*(11), 2553–2570. https://doi.org/10.1175/JAMC-D-14-0021.1

Bourdages, L., Duck, T. J., Lesins, G., Drummond, J. R., & Eloranta, E. W. (2009). Physical properties of High Arctic tropospheric particles during winter. *Atmospheric Chemistry and Physics*, *9*(18), 6881–6897. https://doi.org/10.5194/acp-9-6881-2009

Libois, Q., Proulx, C., Ivanescu, L., Coursol, L., Pelletier, L. S., Bouzid, Y., Barbero, F., Girard, É., & Blanchet, J.-P. (2016). A microbolometer-based far infrared radiometer to study thin ice clouds in the Arctic. *Atmospheric Measurement Techniques*, *9*(4), 1817–1832. https://doi.org/10.5194/amt-9-1817-2016

2) The introduction would gain being reordered so that the transitions across the successive paragraphs would be more relevant. At present the reasoning is difficult to follow. See technical details for some suggestions.

Changes made to improve the transition between paragraphs of the introduction are noted in the point-by-point responses below.

3) It is mentioned that sampling issues (in particular, many observations are discarded), due to the limitations of the retrieval algorithm or issues with the instruments, can be critical. Yet the impact on the comparisons performed and conclusions drawn is not sufficiently discussed. Indeed, the differences reported may to some extent be largely a consequence of sampling differences between both locations.

More generally, the uncertainties of the retrievals and the impact on the interpretations, would deserve more attention.

We agree that sampling issues, both because of algorithm design and instrument sensitivity, can result in a very different representation of cloud properties and have been careful in the results/discussion to avoid implying that the presented cloud properties are representative of the respective regions, especially when comparing to retrievals from other instruments.

A robust and rigorous treatment of the gap in observations filled by AERI-lidar measurements would require full knowledge of the atmospheric state, ideally through aircraft measurements, which are scarce at such high-latitude locations. However, given that the same AERI-lidar measurements are analyzed at both locations, we expect that similar retrieval and measurement biases would apply, and thus, comparing our Arctic-Antarctic retrievals as they are, is reasonable.

Being an optimal estimation-based retrieval, CLARRA does provide a retrieval covariance matrix in the standard output product. However, due to the nonlinearity of the inversion problem, this becomes a strict underestimation of the true error (often a very significant underestimation) because it does not capture important sources of error such as cloud inhomogeneity, cloud boundaries, ice crystal habit, and sky scene subsampling. For this reason, we chose not to report the OEM covariance as an uncertainty. Instead, the manuscript discusses several of the aforementioned error sources:

- sensitivity to cloud base: Appendix B
- uncertainty associated with assumed ice crystal habit (and more generally, the lack of infrared sensitivity to different ice crystal habits)

- reduced sensitivity to thin/thick clouds
- sampling bias from precipitation, humidity, and other quality control filtering

4) Often, the physical interpretations provided in Section 3 remain superficial. It might be more appropriate to distinguish the Results section from a dedicated Discussion section. The latter might be less elaborated than in a Research Article, but merging the various physical interpretations spread across the current section 3 would certainly strengthen this aspect and avoid redundancies. Currently I find this section 3 hard to follow, with many points that would better fit in the introduction. Some reordering may help the readers. Many interpretations consist more of general statements regarding cloud microphysical processes than thorough analysis.

Many points in Section 3 that contain introductory material have been shortened and moved the introduction. We have also re-ordered Section 3 to better differentiate between results and discussions, which are detailed in the point-by-point comments below.

5) The partition between ice and liquid, given in this paper in terms of an optical partition (contribution to the total optical depth), can be hard to use for modelers who handle actual liquid and water contents. It would be helpful to better emphasize how despite this radiative definition the liquid/ice partition can still be valuable for people interested in the mass partition. Likewise, there is no mention of liquid or ice water paths, while these are properties commonly simulated and evaluated in atmospheric models. Addressing these points may help the paper to reach a wider community, beyond the remote sensing one.

The limit on PWV was designed to enforce a minimum sensitivity to the phase, as described on line 202 of the original manuscript:

"Large amounts of water vapour also reduce sensitivity in the far-infrared and prevent retrieval of the thermodynamic phase, so the precipitable water vapour (PWV) is restricted to a maximum of 1 cm."

The liquid/ice water path (LWP/IWP) can be estimated from the retrieved variables. We have added the LWP to the retrieval products and a new Figure 10 to show this new data, but have refrained from reporting the IWP because the IWP depends strongly on the assumed ice crystal habit (potentially by a factor of 2 or 3) and so differences in IWP when comparing between our values and model data may reflect moreso the differences in ice habit rather than the physical IWP, which can be misleading for data users.

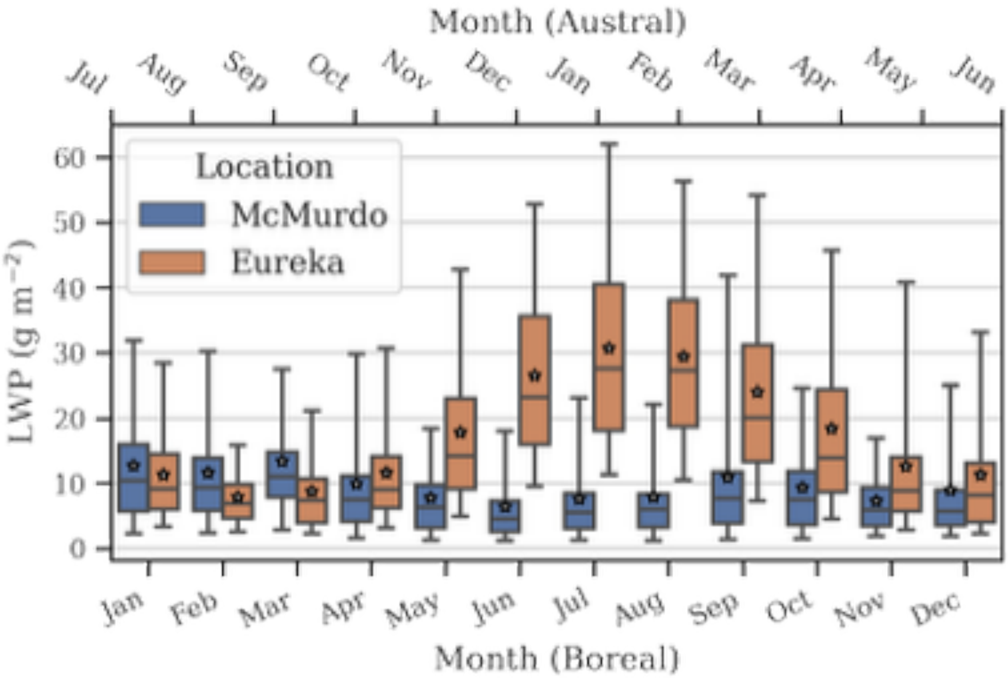
We will upload a new version of the datasets with a new DOI, including the added LWP.

The following text and Figure has been added to discuss/incorporate the newly added LWP. On line 141:

We additionally estimate the liquid water path (LWP) from the retrieved variables, which is calculated as LWP = 2/3 ∗ r_eff,liq ∗ τ_liq ∗ Q_liq, where Q_liq is the volume mass density of liquid water (1000 kgm−3).

On line 222:

The listed QC filters are roughly equivalent to restricting cloud scenes to those with a LWP less than 60 gm^{-2}.



New Figure 10. Same as Fig. 9, except for the liquid water path.

On line 375:

"The seasonal cycles of optical depth, liquid effective radius, and ice effective radius are shown in Fig. 9, and of the liquid water path in Fig. 10."

On line 379:

Furthermore, the LWP exhibits significant variability that is reflective of differences in the optical depth and liquid effective radius. At McMurdo, the LWP features little seasonal variability because the largest optical depths coincide with the smallest liquid effective radius (and vice-versa). Whereas at Eureka a clear maximum occurs during the late summer and autumn, which is a similar cycle to that observed at

Utqiagvik by Shupe et al. (2015) and Zhang et al. (2019) using a combination of infrared and microwave radiometer, radar, and lidar measurements.

In the summary/conclusions, line 436:

The LWP also does not vary strongly at McMurdo, while Eureka sees a very significant late-summer peak.

6) Often, the Figures are not sufficiently well introduced, so that they are not fully highlighted. The reader needs to go the caption to really understand what they show, while one would expect to have some high level details directly in the main text. As an illustration, the Figures are mostly referred to at the end of the sentences, in parentheses, rather than stating “Figure XX shows …”.

The following text has been added to better organize figures.

Figs. 4 and 5 have been more clearly introduced on line 239:

> The monthly distribution of the retrieved variables (optical depth, ice fraction, liquid effective radius, and ice effective radius) is plotted in Figs. 4 and 5 for the retrievals performed using AERI-derived and lidar-derived cloud boundaries, respectively.

As well as Fig. 7 on line 289:

> Figure 7 shows the time series of retrieved cloud properties, analogous to Figs. 4 and 5, with key statistics summarized in Table 3.

Figs. 6 and 8 did not have a high-level introduction, and so the following text has been added on line 275:

> Further, Fig. 6 shows the distribution of r_eff,liq and r_eff,ice, plotted against cloud temperature for each site, and our retrievals indicate that in mixed-phase clouds, the ice effective radius is smaller than in single-phase ice clouds, where panel b illustrates that at Eureka, the mean ice effective radius is 19 μm in mixed-phase clouds, compared to 29 μm in single-phase clouds, in keeping with previous observations by Turner (2005) and Cox et al. (2014).

And on line 351:

> The variability of cloud optical depth is highly site specific. Figure 8 illustrates this by plotting optical depth against temperature at Eureka and McMurdo.

Technical comments

l.15: “microphysics” is unclear. Do you mean microphysical processes or properties?

Clarified – “properties”

l.19: having → “, that have ..., are experiencing ...”

Changed

l.22: why “high albedo snow/ice surfaces” makes it difficult to be simulated?

One such example is that the difference in albedo (and thus the energy budget) between multi-year and seasonal ice is substantial (Perovich and Polashenski, 2012) and the albedo feedback is a large contributor to inter-modal spread (Kim and Taylor, 2026). Thus, high albedo regions are of current scientific interest. For example, having been one of the focuses of the ARCSIX aircraft campaign last year (https://www-air.larc.nasa.gov/missions/arcsix/index.html).

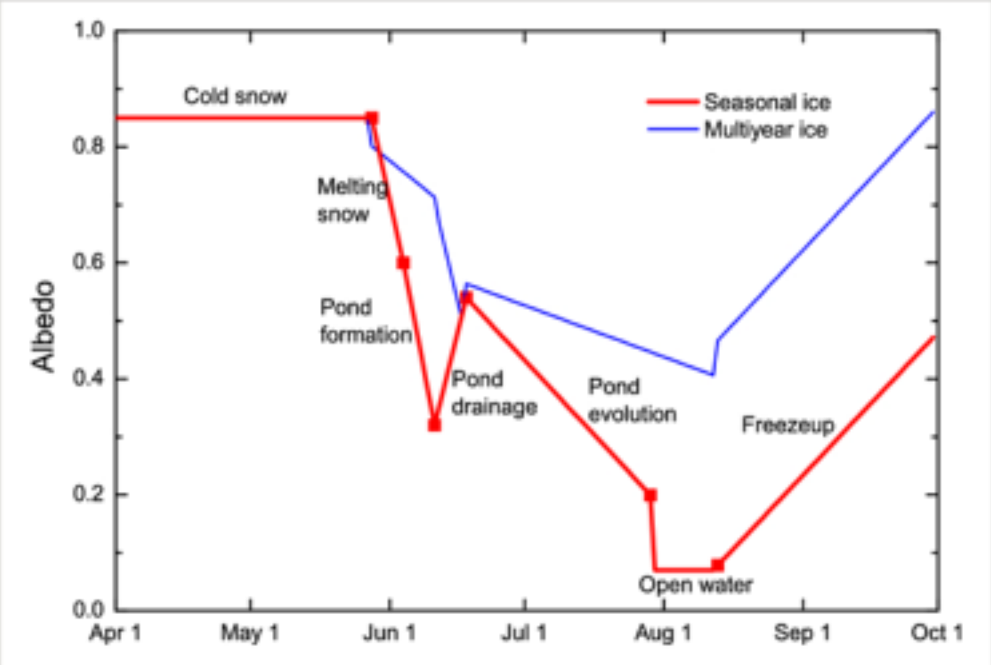


Figure 3 Open in figure viewer PowerPoint

Time series of the evolution of seasonal ice albedo. Seven phases of melt are illustrated. The evolution of multiyear ice albedo [Perovich et al., 2007] is plotted in blue and seasonal ice albedo is in red. These particular time series assume melt onset on 29 May and freezeup on 13 August.

References for this section:

Figure 3 from Perovich, D. K., and C. Polashenski (2012), *Albedo evolution of seasonal Arctic sea ice*, *Geophys. Res. Lett.*, 39, L08501, https://doi.org/10.1029/2012GL051432.

Kim, D., & Taylor, P. C. (2026). *What factors explain the current Arctic albedo and its future change? Journal of Geophysical Research: Atmospheres*, 131, e2025JD044070. https://doi.org/10.1029/2025JD044070

l.29: typo with the parenthesis, extra ";"

Corrected

l.30: microphysical interactions → microphysics or microphysical processes (?)

Clarified – "microphysical processes"

l.36: "tends to interact more strongly" can be misleading as the difference mainly comes from differences in the size of the particles (droplets being smaller than ice crystals in average), rather than intrinsic differences between liquid water and ice optical properties

Added "… due to the generally smaller sizes of liquid water droplets." to this statement, on line 38.

l.48: "differences in cloud feedback strength" suggests that the differences especially come from the representation of ice clouds. Is it the case? Please clarify

Cloud feedback here is a general term referring to many feedback effects related to clouds.

We have moved the second half of this sentence "… and Equilibrium Climate Sensitivity (ECS) differences between Coupled Model Intercomparison Project version 6 (CMIP6) models can largely be attributed to differences in cloud feedback strength (Bock and Lauer, 2024). Improving how these processes are represented through observational constraints is thus important for reducing error." to the end of the first paragraph to remove the implication that this is solely tied to the representation of ice clouds. Line 31.

l.49: the link between this paragraph (very descriptive, focusing on the sites) and the previous one (describing cloud microphysical processes and radiative impacts) is not obvious. Consider improving the transitions, or even reordering some parts of the introduction to improve the flow

The introduction has been roughly re-ordered as follows: [motivation and background] [brief summary of key cloud microphysical processes] [measurement sites/campaigns][remote sensors for cloud properties] [paper goals and summary], with improved transitions between paragraphs. Specific changes are highlighted in the responses to the following comments.

l.54: what is the altitude of "SAFIRE"? If it's nearby 0PAL please specify

SAFIRE (and the PEARL Ridge Lab) data are not used, so we have removed references to both. For reference, SAFIRE is around 200 m altitude and ~2 km from 0PAL, close to the Eureka airstrip. The new sentence reads, on line 57:

"This study focuses on measurements made at the Zero-Altitude PEARL Auxiliary Laboratory (0PAL) at 10 masl."

l.61: Again, I find the transition to the FIR quite abrupt. Especially because the FIR can be either a "tool" to probe clouds, or a way to measure spectrally resolved cloud radiative impact. Hence "The far-infrared (FIR) is difficult to study" is unclear. Maybe say that cloud observations are needed, that IR observations are relevant, and the FIR has a great added value for that (if this is the point here).

The introduction to the infrared spectrum for cloud remote sensing has been worded as follows to smooth the transitions between the measurement sites and measurement techniques, on line 67 :

"Measurements of cloud properties at these sites are important in filling in the high-latitude data gap. Cloud properties may be derived by ground-based instruments, which take advantage of the interactions between radiation and clouds with different microphysical properties. The infrared (IR) spectrum is one such tool, as this spectral region is sensitive to optical depth ...".

l.65: "is responsive" is unclear → sensitive, bears the signature?

Changed to "... and bears the signature of emission from liquid water and ice crystals..." on line 71

l.80: IR measurements are not well introduced. The idea should be to explain right here why IR has some advantages compared to other techniques. The introduction could more explicitly feature the common scheme "context, motivation, objective, approach"

We have removed this sentence entirely to smooth the transition. See response to **l.61**.

l.83: "from downwelling thermal emission measurements" should not be the objective, but rather the strategy used to answer the objective, namely "obtain a long-term record of cloud microphysical and optical properties". Unless the focus of the paper is on the retrieval algorithm.

The first objective has been reworded as "The first is to obtain a long-term record of cloud microphysical and optical properties, *[which are]* retrieved from downwelling thermal emission measurements ..." to emphasize the data record as the objective. Line 93.

l.84: "is highly complementary..." does not seem to be at the right place in the introduction. Can it be a perspective, or instead should it be in the previous paragraphs in the context, to specify why ground-based measurements are used in the present study, beyond existing airborne measurements?

We have moved this perspective to the preceding paragraph to highlight the dataset. Line 87.

"However, this requires a suite of instruments to be operated continuously and concurrently for extended periods, which is not always possible at remote field sites, such as at PEARL. Thus, analysis of infrared-derived cloud properties is highly complementary to lidar-radar surveys of polar clouds, as well as data from sparse aircraft campaigns (e.g., Verlinde et al., 2007; Wendisch et al., 2019; Zhao et al., 2023; Xia and McFarquhar, 2024) by providing baseline knowledge on cloud properties and variability, particularly if differences between in-situ and remote measurements can be reduced (Di Natale et al., 2026). Moreover, the dataset will be beneficial for model validation, and can be used for case studies. In addition, ongoing measurements will allow for detection of trends in Arctic cloud properties with climate change."

l.87-88: "will be beneficial", "will allow" → this should be in the perspectives rather than in the introduction

This sentence has been moved to the previous paragraph; see previous comment.

Figure 1: why showing other points than PEARL on A? On B specify which facility is shown.

As we compare Eureka measurements to those from other Arctic sites it can be useful for the reader to have a reference for the various geographical locations. The caption of Figure 1 has also been amended to state the PEARL 0PAL, as well as the coordinates for PEARL and McMurdo, as suggested by Reviewer 2.

Updated Fig. 1 Caption: "Site locations and photos for the PEARL 0PAL at Eureka, Canada (80 N, 86 W) (a,b), ..."

l.90: "Previous work has suggested" → this should be in the context, not in the objectives

This sentence has been moved to the paragraph where PEARL and McMurdo were introduced. Line 64.

"In the Antarctic, the Atmospheric Radiation Measurement (ARM) West Antarctic Radiation Experiment (AWARE) program (Lubin et al., 2020) provided a comprehensive suite of measurements of atmospheric radiation and meteorological processes at McMurdo Station on Ross Island (77°S 166°E) in 2016 (Fig. 1c and 1d). Previous work has suggested that there are important differences between Arctic and Antarctic cloud properties, with Lubin et al. (2020) finding that clouds at McMurdo were higher, optically thinner, and typically had smaller effective radii than at some Arctic sites such as the ARM North Slope of Alaska (NSA) site."

l.95: it may be more relevant to mention first the instruments and data, and then the retrieval technique

This summary for Section 2 has been updated to reflect the order the material is presented, line 101:

"Section 2 describes the instruments and data recorded at PEARL and during the AWARE campaign, along with the physical inversion scheme used to retrieve cloud properties (cloud boundaries, optical depth, ice fraction, and effective radius of liquid droplets and ice crystals) from these measurements."

l.116: some information about the frequency of AERI measurements would be useful in this paragraph

We have added that information to the manuscript on line 119.

The E-AERI acquires one spectrum every 7 minutes, while the P-AERI operated on a 40 second sampling and its spectra were averaged to the same 7-minute resolution as the E-AERI.

l.122: is it useful to detail what products (if not only backscatter profile) of the HSRL are used?

We have used the lidar backscatter and depolarization ratio and have added this info to Table 1.

l.128: maybe explicit [t]hat CLARRA only works for single layer clouds (if correct). What happens in the case of multi-layer?

We have not explicitly restricted retrievals for single-layer clouds. If a multi-layer cloud is present (e.g., as prescribed by lidar data), CLARRA will attempt to fit parameters for each layer.

l.130: is there any assumption in CLARRA regarding the vertical distribution of cloud mass or extinction? More generally, slightly more information about the assumptions made in CLARRA may be useful.

No such assumptions were made. The water profile is prescribed from radiosonde measurements and is not retrieved.

l.131: are there a single liquid and ice effective radius for the whole cloud?

Yes, the assumption is made that clouds are vertically homogeneous. However, the boundaries for ice and liquid clouds are independently fit (i.e., a mixed phase cloud is determined to exist when ice and liquid layers intersect).

l.134: “equivalent”

Fixed

l.136: the database of Yang et al. (2013) contains many shapes. Specify which habit is used, and what assumptions are used regarding the size distributions

The scattering parameters are compiled assuming a lognormal distribution of surface-roughened ice prisms.

The information in comments **l. 128, 130, 131, 136** have been added to the manuscript as follows on line 169:

Thermodynamic profile information is interpolated from the two nearest radiosonde launches (at <= 12-hour intervals) and is not separately retrieved. The assumption is also made for vertically homogeneous clouds, whereby a single R_eff_liq and R_eff_ice are fit, and a mixed-phase cloud is determined to exist when ice and liquid layers intersect. In the case of multi-layer clouds (i.e., if detected by a lidar), CLARRA will attempt to fit a separate optical depth for each layer.

l.136-139: I think this sentence is hard to understand and should be clarified. I guess the point is the inconsistency between the shape assumed in CLARRA and the actual ones, such that a different reff may just mean a different ice habit (with actually similar reff)?

This sentence has been rewritten to clarify the meaning. In addition to the suggested clarification, the point is also that there are some ice habits that yield very similar infrared signatures in the windows chosen despite (possibly very) different r_{eff} and so regardless of the shape assumed in CLARRA, cannot be distinguished with infrared measurements. Line 145:

"It is also important to note that while the retrieved $r_{eff,ice}$ would be reflective of the size distribution for spherical ice crystals, the true ice habit is unknown. In addition, the infrared signatures of different ice crystal habits may be similar, which would make it challenging to distinguish the true ice habit without additional synergistic measurements. In practice, this means that the uncertainty of the $r_{eff,ice}$ retrieval would be the combination of several factors, including the uncertainty associated with assumed ice habit and limitations in the sensitivity of the retrieval windows, and accounting for these will be important for interpreting the retrieved $r_{eff,ice}$.

l.151: are the profiles used as such, or included somehow in the state vector? As they may vary along the day. If the actual temperature is not well captured by the RS (which can be performed with some delay), what are the consequences on the retrieved properties?

The two nearest radiosonde profiles are interpolated (in time) to each retrieval time, but we do not include the temperature profile in the state vector. Biases in the temperature profile would result in the opposite bias in the optical depth retrieval because a temperature bias would result in broad shifts in the infrared spectrum, which was clarified in the response to comment **l.229.**

Instead, we supplement the radiosonde temperature profile with the 2 m and 10 m temperature from surface sensors (availably hourly) as AERIs are most sensitive to emission from the near-surface atmosphere. This information has been added to the table of data sources (Table 1):

“Surface Meteorology

(2 m and 10 m temperature)”

l.153: remove water vapour (H2O) (I assume)

We do consider emission from water vapour (and do not retrieve it), so this is intended.

l.165: what about the interpolation from Yang database to AERI resolution for cloud properties?

The database is linearly interpolated to effective AERI resolution.

Figure 2: consider using a and b instead of A and B to label the figures (the same holds for Fig. 1). The labels in panel a are too small

Figure panel labels have been made larger; and labels have been adjusted to lowercase letters for both Figures 1 and 2.

Figure 3: CO2-slicing is not introduced. Does it give the upper boundary of the cloud?

CO2 slicing is introduced in Appendix A (while it does give an upper boundary, there is little sensitivity), and the caption has been modified to point the reader to Appendix A.

Figure 3 caption has been updated to “Diagram of the CLARRA retrieval flow. The dashed boxes indicate required data input. See Appendix A for an introduction to CO_2 slicing and an evaluation for using this technique in retrieving an effective cloud base height.”

l.174: with so many references, hard to know where the actually used database comes from

The final parameters are from Rowe et al. (2020), which has been made clearer in the text as follows, line 188:

“For modeling scattering, we use temperature-dependent scattering properties of liquid water droplets by Rowe et al. (2020), based on previous work by Downing and Williams (1975), Bertie and Lan (1996) and Rowe et al. (2013, 2019).”

l.177: the bias issue is not clear. Did you test on simulated spectra where another shape is assumed for the forward computation?

This was done in the CLARRA description paper by Rowe et al. (2019), who had tested the error resultant from assuming spherical ice crystals against a variety of other habits.

We have slightly changed the text on line 190 to make this clear:

"Although this assumption of homogeneous ice crystal shapes is inaccurate (e.g., Walden et al., 2003; McFarquhar et al., 2017) and leads to a bias in CLARRA based on testing retrievals on simulated spectra, as shown by Rowe et al. (2019)."

l.178: "inhomogeneity" seems inappropriate here. Diversity?

Changed "inhomogeneity" to "diversity" on line 192.

l.183: "best-knowledge thermodynamic state" is not clear. Does it mean spectra taken close to the soundings?

We have interpolated the nearest two soundings to each spectra time and calculated the clear-sky radiative transfer with the interpolated data and ensure that each analyzed spectrum does not differ too significantly from what is expected, given the sampled T/Q profile.

The text on line 196 has also been updated to "radiative consistency" rather than "radiative closure" to reflect that this is primarily a quality control measure and we are only examining these metrics for narrow windows of the spectrum.

l.203: total column PWV or below cloud?

This is total column, so our filter is conservative and will remove some borderline cases. However, above the typical cloud altitude, there is very little moisture. For example, at Eureka, about 90% of the water column is below 4 km (Weaver et al., 2017).

Table 3: consider swapping McMurdo and Eureka, as in the text Eureka is generally mentioned first **and l.216**: the information regarding hatch closure is redundant, can be removed here

Done

l.226: it is not clear if "cloud boundaries" in the paper refer to bottom and top or only bottom. Does CLARRA need both boundaries?

Cloud boundaries refer to both bottom and top, and CLARRA does require both. We have only evaluated the CO_2 slicing-derived cloud bottom because even for moderately thick clouds, there is very little sensitivity to the cloud top already.

l.229: "may manifest in the retrievals as warmer clouds". I don't understand. If the cloud bottom is considered lower (hence warmer most of the time) it means the emission signal is attributed to temperature rather than to optical thickness, so I would imagine a compensation of biases in the retrievals, which might be critical.

Yes, we would expect the typically lower temperature at 1.8 km vs. 1.4 km would mean higher temperature at cloud layer, and correspondingly, lower optical depth and negligible effects on the other retrieved variables due to the spectrally flat errors induced by biases in temperature. However, Appendix B demonstrates how CLARRA optical depth retrievals performed using the AERI-derived vs. lidar-prescribed cloud heights show minimal differences in results between the two sources, so this bias is small.

The sentence on line 243 has been amended as such for clarity:

"…may manifest in the retrievals as warmer clouds, which leads to low bias in the retrieved optical depth, however, the overall effect of this low-cloud bias is insignificant (see Appendix B)."

Fig. 4: is "Incidence" a standard name for the probability of occurrence?

Updated colourbar label for Figs. 4, 5, and 7 to "Frequency of Occurrence".

l.240: why not showing in Figs. 4 and 5 the monthly mean ice fraction, which would help match the 0.1 and 0.6 values given here (which currently cannot really be seen in Fig. 5)

Monthly mean ice fraction has been added to Figs. 4, 5, and 7.

l.241: where can the reader see that mixed-phase clouds are present all year long? In Table 3 only?

Also, in Figs. 4 and 5, which now have the monthly mean ice fraction plotted. This has been added to the text on line 271:

"… in Table 3 and in Figs. 4 and 5."

l.241: shouldn't 40% rather be 44% ?

Yes, this has been corrected, on line 256:

(~44% of all analyzed clouds are mixed-phase)

l.243: not clear what "interaction" means here. Does it refer to the emission, to the extinction, to the size of the particles (hence optical depth)? Why not assuming a seasonal cycle in the total column of condensed water? Do you have any information of these integrated quantities? Also, "cloud liquid" is awkward.

"Interaction" here is referring to the size of particles and associated change in optical depth.

We do not have information from other instruments on condensed water except for brief periods during the Eureka measurement time series but have calculated and reported the liquid water path from the AERI retrieval. This has been added to the text as follows on line 258:

"Because of the strong interaction between longwave radiation and cloud liquid content, it is assumed that the variation observed in optical depth is driven, in part, by the associated seasonal variability of cloud phase and condensed water (see Section 3.3)."

l.245: I fully agree with this warning, which questions the assertion just before, as ice fraction is not really an ice fraction…

This is true, however, because we filter retrievals to maximize sensitivity in the 500-600 wavenumber region, where the information on cloud phase is contained, we expect that there is sufficient sensitivity in the retrieval windows that the optical phase classification (along with the additional QC requirements that are imposed) would be realistic for the subset of sky scenes that are analyzed. We also see that (see comment **l.294**) the optical classification satisfies some expected physical constraints.

l.254: this statement regarding mixed-phase clouds is redundant with l.240. Consider merging the information

We would prefer to keep this short statement to highlight the importance of mixed-phase clouds in the following discussion.

l.262: why is this observation counterintuitive?

We would expect that in mixed-phase clouds, ice crystals to grow at the expense of liquid droplets (e.g., via the WBF process), and thus, grow larger than in single-phase clouds. However, we see the opposite in our retrievals, and hence, have added notes that explain some contributing reasons and that for this reason, infrared cloud retrievals are more accurate for analyzing liquid in clouds as Reviewer 2 has noted (although additional information on cloud phase and ice crystal habit can be found by examining windows further into the far-infrared).

l.269: “parameterization” is unclear, maybe clarify the meaning here

Rephrased line 284 as “Furthermore, this also means that ice effective radius retrieved from infrared spectra are not directly comparable with in-situ measurements, because the retrieved values are not necessarily representative of the physical dimension.”

l.273: again, where do you read that there are no purely liquid clouds? Because the ice fraction is never 0? Because the mean occurrence is low? It deserves some clarification

Yes, this is based on the ice/liquid optical depth classification. As discussed in a previous comment, we would expect that given the restricted regime of retrievals analyzed, the optical phase classification would differ somewhat, but not significantly from the true phase classification.

l.275: the question of ice nucleation is indeed central, it deserves more discussion

This is true, but a detailed discussion of ice nucleation (and especially resultant crystal habits and how these are reflected in infrared retrievals) is really beyond the scope of this paper.

l.277: which “bias” do you refer to?

This was a poor choice of wording, and incorrect sentence order. We have reordered the text on line 292 to say:

“Liquid-only clouds were also almost always supercooled. This may be significant as smaller, supercooled, liquid droplets will generally have larger albedo (at solar wavelengths). Note that references to seasons at McMurdo are offset by 6 months against seasons at Eureka and are referred to hereafter as austral seasons."

l.281: is "feature" appropriate? "strongly dominate"?

Changed line 298 to "Single-phase ice clouds feature strongly during the winter..."

l.284: does this "60%" value appear in one figure? If not, would it be useful to complete Fig. 7?

Monthly means for the optical ice fraction have been added to Fig. 7.

l.285: redundancy with l.275

removed "When liquid is present in clouds, it is usually supercooled [...]". The paragraph beginning on line 302 now begins with:

"The mean cloud temperature is 252 K for liquid..."

l.287: "adjacent" does not seem an appropriate term here

changed "... adjacent cloud interactions" to "... related cloud interactions" on line 303.

l.288-290: I don't understand this sentence, and the conclusion drawn from the temperature similarity

The temperature range and distribution for mixed and liquid phase clouds at McMurdo essentially overlap, with the vast majority of these clouds < 250 K and many approaching the homogeneous freezing temperature limit. So, the temperature at which ice forms in the observed liquid-containing clouds should be within this temperature range (which is quite low).

l.292: I don't understand this statement. Shouldn't you look at the low tail of the mixed phase clouds to derive such a conclusion?

Yes, and the tail end of the liquid clouds. We also note that there is a small peak around 238 K in the ice cloud temperature distribution, which is indeed around the temperature range for homogeneous nucleation.

This sentence has been amended for clarity on line 308 of the revision to:

“The low-tail of the mixed and liquid cloud temperature being near the temperature at which homogeneous freezing may begin to occur …”

l.294: not sure it is needed to remind that, might be counterproductive’

We have adjusted the text on line 311 to point out that the optical phase classification satisfies a physical constraint.

“To reiterate, for our retrieval, liquid is only forced to ice at 233 K, so this transition is a signal from the data, and lends confidence to the quality of the infrared optical phase classification.”

l.296: liquid effective radius actually seems to feature some seasonal cycle

l.297: you mention seasonal differences, but no seasonality, this is unclear

l.298: please explicit the expected variations in atmospheric conditions and aerosol load. Actually the explanation comes later, consider reversing the sentences or merging them to help the reader

Comments l.296, l.297, l.298:

These lines have been reordered and clarified on line 313 of the revision:

“Although only one complete seasonal cycle was observed in Fig. 7, the small seasonal differences are consistent with variation in atmospheric conditions and are likely related to the atmospheric aerosol load whereby higher aerosol loads provide additional CCN, serving to reduce the liquid effective radius by distributing the total available water between more droplets (commonly referred to as the Twomey effect). The ice R_eff retrievals reflect a lack of seasonality at McMurdo while the liquid R_eff are strongly clustered around the monthly means.”

l.303: it is not clear what would be an “interesting avenue”. Why mentioning this potential source of INP? Does it help interpreting the results?

We have rephrased this for clarity on line 319:

As the Antarctic coast of the Southern Ocean is a biologically productive region, evaluating the efficacy of biogenic INP and resultant cloud properties may be an interesting avenue for future work (e.g., Wilson et al., 2015).

l.308: it might be interesting to give some information regarding the differences of INP, beyond the references. Also, would it be more appropriate to mention that in the introduction rather than here?

l.310: the mention of ice number concentration comes without any quantitative information. It is not clear whether this section tries to interpret the differences or presents general statements regarding ice clouds

l.314: why mentioning models here while the study focuses on observations?

l.316: this looks more like a motivation for the study, than a discussion of the results

> Comments l.308 to l.316:

These lines have been shortened, and some content has been moved to the second paragraph of the introduction as follows (paragraph beginning line 35 of the revision):

"Cloud microphysics is influenced by numerous factors related to the total available water and aerosol load, local dynamics, and the temperature profile. Aerosols contribute seasonally variable sources of cloud condensation nuclei (CCN) and ice nucleating particles (INP), which affect cloud formation and ice initiation – thermodynamic phase partitioning is especially significant because radiation tends to interact more strongly with populations of cloud liquid droplets than ice crystals due to the generally smaller sizes of water droplets (e.g., Sun and Shine, 1994). In addition, differences in the type and concentrations of INP between the Arctic and Southern Ocean (e.g., Tan et al., 2014; Wilson et al., 2015; Vergara-Temprado et al., 2018; McCluskey et al., 2018; Wex et al., 2019; Filonchyk et al., 2025) mean that initial ice formation will differ between the two regions. Mixed-phase clouds (i.e., containing both supercooled liquid and ice) introduce complex additional microphysical interactions that remain poorly understood (Verlinde et al., 2007; Korolev et al., 2017).

The following short intro has been kept in Section 3 on line 322 to motivate the comparisons (and to remind readers on the limitation of the ice retrieval):

"Given the large differences in the type and concentration of INP and active SIP between the Arctic and Antarctic environments, it can be expected that the varied cloud nucleation and microphysical processes at the two locations will result in differing cloud properties. Thus, baseline datasets of key cloud parameters are important for understanding differences in cloud processes between the two polar regions, although it is important to note that the extent to which infrared-derived cloud properties are sensitive to ice-phase microphysics and secondary ice production is unclear..."

l.317-318: not clear what the meaning of this sentence is. It deserves more explanation

We have added the following text to clarify this point, continued from the response to the previous comment (line 325):

"...although it is important to note that the extent to which infrared-derived cloud properties are sensitive to ice-phase microphysics and secondary ice production is unclear. For example, expected ice growth from the WBF process in mixed-phase clouds is not captured by the retrieval."

l.322: and so, what are the consequences of removing a certain amount of observations due to this threshold?

More observations at Eureka are filtered out than at McMurdo in the summer (see Table 3), which would introduce some bias in comparisons for warmer liquid and mixed phase clouds. This has been added to the text as follows, on line 330 :

"This means that the limit on PWV (1 cm) results in some measurements during the warmer and more humid summer being filtered out, particularly at Eureka, which would introduce some bias against warmer liquid and mixed-phase clouds."

l.325: is Fig. 9 mentioned before Fig. 8? [If] that is the case, figures probably need to be swapped. Also, just reading the text, we have no idea what Fig. 9 contains.

Figures 4, 5, 6, 7, 8, and 9 have had new text added to introduce them / reorganized so that they appear in the order of reference, which is outlined in point (6) in the Specific Comments.

The caption for Fig. 8 in the submitted manuscript was also mistakenly reversed – Eureka is panel (a) and McMurdo is (b), which has been corrected in the updated version.

"Optical depth vs. temperature for (a) Eureka and (b) McMurdo..."

l.327: not clear what you mean by "may have been anomalous in some way"

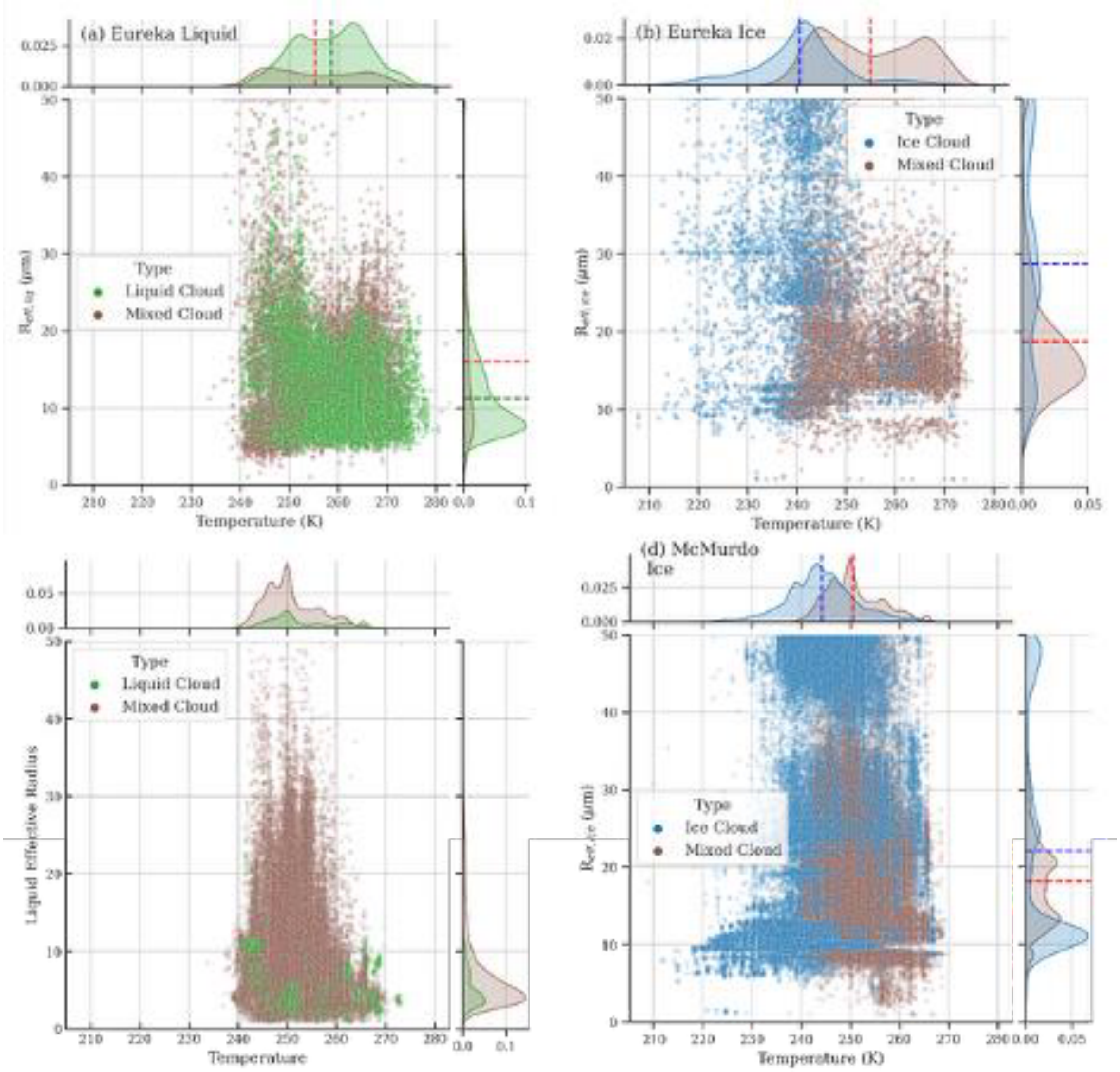
This statement points to that we are unable to determine if AWARE measurements were representative of the McMurdo climate from the single year of observations. For clarity, this has been rephrased, on line 336, to:

"... under the caveat that the 2016 McMurdo conditions may not have been representative of typical climatological norms."

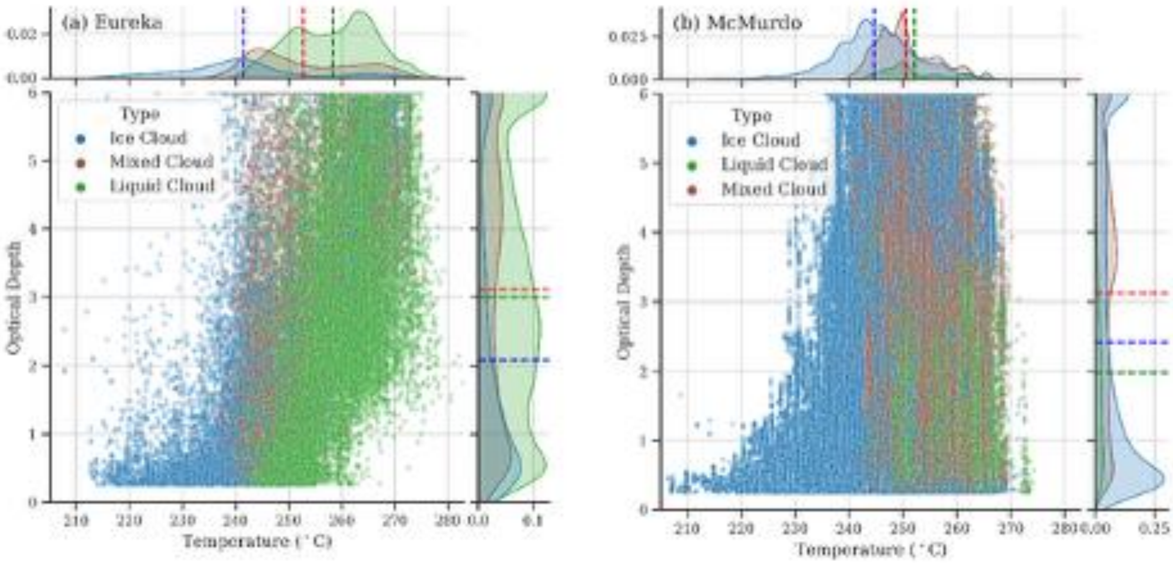
Figure 8: would it be easier to see correlations if the colored points were shaded, so that a more intense color highlights a higher density of points? Currently dense regions do not pop out. The same holds for Fig. 6.

Figs. 6 and 8 have been re-made as suggested:

The updated Figure 6 is shown below (see updated manuscript for full quality version):



And the updated Figure 8:



l.343: "due to the prevalence of thin ice clouds". I don't understand why this explains the absence of correlation with temperature

This statement points to the fact that for the subset of clouds that are cold, optical depth and temperature do not co-vary much because most cold clouds are also thin (and ice), so any linear regression would be nearly horizontal.

l.354: this conclusion is not straightforward. You may have liquid clouds at Eureka at low temperatures as well, but warmer clouds (not present at McMurdo) may drive the mean to higher values there

That is true, and we have added the additional quantifier in the discussion to account for this observation on line 363:

"…, and significantly cooler at McMurdo for liquid clouds (252 K at McMurdo vs. 259 K at Eureka). This final point, along with the observation that the temperature distribution of liquid clouds at McMurdo is heavily skewed towards the low end of the range, suggests that either ice formation generally occurred at a lower temperature at McMurdo, or that ice formed more readily at higher temperature at McMurdo."

l.357: this is interesting, but does it contribute to explain the differences between McMurdo and Eureka?

No, but this observation ties these results into the wider literature.

l.363: The figure is well introduced, which is appreciated, but it has already been referred to earlier

See response to (6) in Specific Comments for our re-organized figures.

Fig. 9: specify that the offset is 6 months, otherwise it's hard to understand.

Fig. 9 caption has been updated to:

"... The McMurdo data have been offset by 6 months to align the boreal and austral seasons, with the top axis denoting the original McMurdo time series..."

l.386: remove one "during"

Fixed

l.404: what is specifically "unique" to Eureka?

Eureka is (generally) drier, somewhat warmer, and features optically thinner clouds than at other Arctic research sites. E.g. deBoer et al. (2009), Shupe (2011), and Cox et al. (2012) references. These points were mentioned in the introduction, so we have neglected to repeat it again here.

l.419: it would be easier to compare the min or max, but not the min of McMurdo with the max of Eureka

The comparison has been updated on line 435:

"... at McMurdo, the liquid effective radius is largest during winter, while the Eureka it is largest in the spring and summer."

l.425: could you elaborate on these implications as it seems to be an interesting result

One could argue that regardless of the retrieval error associated with uncertain ice habits, a radiative parameterization is valuable in and of itself because it represents what a surface infrared sensor is able to see.

l.433: the decadal time series is only for Eureka.

Text has been updated on line 452:

“… provides a valuable decadal time series of key cloud microphysical and optical properties at Eureka, and highlights …”

Figure B1: “Rayleigh”

Fixed