Reviewer 2

Thank you very much for taking the time and investing the effort to thoroughly review our study. The raised comments helped to further improve the manuscript. In the following, we reply (blue) to all reviewers‘ comments (black). Text passages from the manuscript are in italics. In our answers, we always refer to the line numbers of the newly revised manuscript.

line 39: I don’t think Morrison et al 2012 were exploring mixed-phase cloud streets. In their study the super-cooled liquid clouds were stratiform and long-lasting because ice depletion was low. The Murray-Watson paper would be a more topical reference here.

Due to retrieval limitations, Murray-Watson et al. (2023) only considered liquid-dominated clouds as well (see line 59 of the manuscript). Thus, we added Abel et al. (2017) as a reference, who studied airborne precipitation observations of mixed-phase clouds during a MCAO event.

Line 229: why not also correct the passive LWPs for the slant path angle and provide a vertical LWP estimate? It’s true the LWP values may not be exactly accurate, but the same can be said for the geometrically-corrected radar measurements. It is difficult for the reader to hold on to the idea that the reported LWP values are all along a slant path rather than vertical. This process of correcting radar but not passive microwave for the slant angle doesn’t make sense to me. Later on you discuss shifting the LWP values in time to bring them into closer agreement with the radar. To my mind simply correcting both the radar and microwave similarly for the same geometrical issue makes more sense.

We thank the reviewer for bringing up this important point which made us revisit our retrieval theory described in detail in Appendix B of Ruiz-Donoso et al. (2020).

A slant geometry is necessary for the MiRAC FMCW radar to avoid strong surface returns from the active component. As described in detail in Mech et al. (2019), the radar profiles are reconstructed to nadir.

For the passive component, meanwhile, it is less straight forward to correct for the slanted geometry. The measured brightness temperatures correspond to the sum of signals originating from the entire slanted atmospheric column within in the viewing cone of the instrument, including sources from surface, atmosphere, and clouds/precipitation.

Correcting the brightness temperatures to nadir would require making assumptions on the (unknown) vertical setup of the atmosphere and the ground, as well as running radiative transfer calculations to forward simulate the measured signal. We believe that this would induce even higher uncertainties compared to the present approach of shifting the retrieved LWP in time. The vertically integrated liquid water observed by MiRAC-A on slant geometry is instead derived using the following method.

We set up an atmosphere based on the dropsondes of the campaigns with many artificial cloud profiles. Based on this large set of atmospheres, we perform radiative transfer simulations to get a solid database for liquid water paths and corresponding brightness temperatures for various slant geometries, and derive cubic regression coefficients. To derive the liquid water path from the brightness temperature observations, we apply the coefficients from angles and atmospheric altitudes closest to the aircraft observations of each brightness temperature. This method is further described in Appendix B of Ruiz-Donoso et al. (2020). Moreover, a manuscript on the LWP retrieval is in preparation.

We additionally tested the validity of the applied spatial shift of the LWP retrieval obtained from geometrical considerations. We therefore compare the brightness temperatures obtained by MiRAC-A (89 GHz) with brightness temperatures from HATPRO (31.4 GHz as 89 GHz is not available), another radiometer that is installed onboard the P5 in nadir-looking geometry. Comparisons show a good accordance between the shifted and nadir measurements for both days as shown in Fig. R1 for a short time series on 04 April.

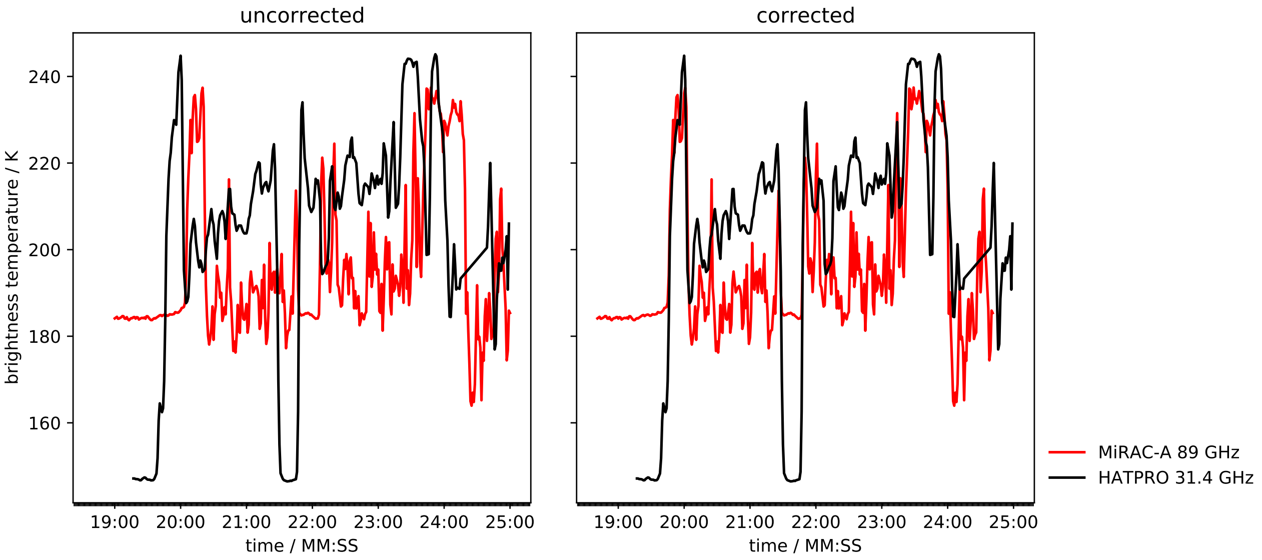

Figure R1: Time series of the brightness temperature on 04 April obtained by MiRAC at 89 GHz and HATPRO at 31.3 GHz. Left: originally obtained data that are inclined by 25° for MiRAC and nadir for HATPRO. Right: Original HATPRO data (nadir) and spatially shifted MiRAC data that mimic the nadir view.

Note that we do not take the HATPRO observations for deriving LWP here in order to be able to compare the retrieved LWP with consistent estimates from past and future campaigns within the AC^3 framework which always included MiRAC measurements.

Line 258-260: incomplete sentence here beginning with 'While'.
We corrected the sentence: *„While radar reflectivities are corrected to nadir profiles, TB and, thus, LWP measurements are measured along a slanted path (Mech et al., 2022a).*" (line 158)

Line 308: it would be worth assessing if ERA5 fluxes (which you also use, e.g line 434) match those calculated as you did from the dropsondes, building on Seethala et al 2021. The accuracy of ERA5 turbulent fluxes over the open Arctic Ocean is not well known, and this is an opportunity to opine on how well they follow those calculated from the dropsondes using the COARE 3.5 flux algorithm.
We thank the reviewer for raising this important comment. We point the reviewer to Fig. 3 e, f and line 270: „In accordance with Seethala et al. *(2021), fluxes and MCAO indices from ERA5 generally correspond to dropsonde estimates, except over sea ice where ERA5 seems to overestimate the fluxes. Finer spatial structures in both parameters are resolved in the dropsondes.*"

Line 545: how are the in-situ particle shape measurements determined? Is this a fractal dimension?
The optical in-situ particle shape measurements were obtained by the instruments CDP, CIP, and PIP. More information on the instruments and derived properties is given in lines 166 ff. A fractal shape of the particles is derived. The full complexity of the retrieval is described in Maherndl et al (2024).
For clarity, we added the following to line 170: "*Rimed mass is calculated from images of the fractal particle shapes, as well as the continuous particle size distribution derived from combining*

CDP, CIP, and PIP observations.“

Line 561: where is this suggestion of more riming -> more precip supported? It seems reasonable other than that faster-falling particles might be able to reach the surface without a phase change.
We agree with the reviewer and removed the corresponding statement from the manuscript. We additionally adapted the text which now reads (line 340): „*For 01 April, we, hence, infer that riming is mainly present within the updraft regions of cloud streets. A more detailed comparison with λ of the roll circulation detected by the remote sensing measurements is performed in Sect. 4.4*“

Line 650: remove ‘by’
Done.

Line 844: should ‘III’ be ‘II’? This is the 2nd question right? Revisit subsequent numbering also if so.
Done.

Line 852: assuming the data support the hypothesis, please try to more clearly connect the hypothesis to the supporting data.
The updated manuscript states: „*Our statistical analysis of median cloud characteristics within the roll circulation and their variability (Fig. 7) could be used to test the performance of cloud parameterizations and better understand riming effects.*“ (line 466).

Refs:
- the Maherndl et al 2023 now has a final revised paper that would be better to cite than the preprint.
Done.
- Seethala, C. Et al, 2021: On assessing ERA5 and MERRA2 representations of cold-air outbreaks across the Gulf Stream. Geophys. Res. Lett., 48, doi:10.1029/2021GL094364
Done.

Ruiz-Donoso, E., Ehrlich, A., Schäfer, M., Jäkel, E., Schemann, V., Crewell, S., Mech, M., Kulla, B. S., Kliesch, L.-L., Neuber, R., and Wendisch, M.: Small-scale structure of thermodynamic phase in Arctic mixed-phase clouds observed by airborne remote sensing during a cold air outbreak and a warm air advection event, Atmos. Chem. Phys., 20, 5487–5511, https://doi.org/10.5194/acp-20-5487-2020, 2020.

Maherndl, N., Moser, M., Lucke, J., Mech, M., Risse, N., Schirmacher, I., and Maahn, M.: Quantifying riming from airborne data during the HALO-(AC)^3 campaign, Atmos. Meas. Tech., 17, 1475–1495, https://doi.org/10.5194/amt-17-1475-2024, 2024.

Reviewer 3

Thank you very much for taking the time and investing the effort to thoroughly review our study. The raised comments helped to further improve the manuscript. In the following, we reply (blue) to all reviewers‘ comments (black). Text passages from the manuscript are in italics. In our answers, we always refer to the line numbers of the newly revised manuscript.

Major Concern

Sections 4.3 and 4.4 contain a few portions where the authors discuss their findings, and it is difficult to separate actual findings from speculations. The authors should either use more nuanced language (especially in the portions immediately following a hypothesis) or prepare a separate discussion section:
- ll. 367-370 The authors first suggest (“seems to”) the role of MCAO strength and then imply (“thus suppress”) with further causal implications after.
- ll. 379-381 This sentence contains mere speculation.
- ll. 356-359 After suggesting a relationship (“We hypothesize”), the authors imply certain properties (“thus increases”)
- ll. 408-415 This paragraph is filled with discussion elements.

We thank the reviewer for this comment and thoroughly went through the manuscript, therein especially Sec 4.3 and 4.4, to adjust the language as needed. In more detail, we updated the following sentences, and revised the corresponding sections to account for language changes.

„*The smaller MCAO strength on 04 April seems to weaken the updraft motion and might, thus, suppress the rise of CTH and the lifting of ice into the liquid layer in updrafts. In updrafts, this might prevent riming, likely hampering an increase in S and mean Ze as well as a lifting of the height level with highest ice occurrence.*“ (line 371).

„*A potential reason for this reduction might be a reduced buoyancy in the ABL, and warm air being advected above the boundary layer. Future modeling experiments could test this hypothesis, including lee effects on air mass development caused by the Svalbard archipelago.* “ (line 385).

„*We speculate that, here, updrafts carry ice particles to higher cloud regions. If so, the mixed-phase region would expand at the expense of the liquid layer and would enhance riming (Fig. 4.2). Potential riming occurrence would increase ice particle size, Ze, and S in updrafts. The observed slight LWP increase in updrafts (Fig. 7e, f) could indicate that, in our study, condensation is more favored than depletion of liquid.*“ (line 359).

„*While the evolution of cloud microphysics with fetch is similar on both days, thermodynamic conditions modify the intensity of the parameters.*
On 04 April, characterized by overall warmer temperatures, clouds are more shallow. On this day, 90% of the profiles containing liquid-topped cloud streets have LLT of smaller than 100m, which is more than on 01 April (70%). Less supercooled liquid may reduce the amount of liquid-topped cloud profiles (Table 2), LWP (Fig. 8k) and LLT (Fig. 8i). A potential mechanism could be that the warmer temperature, low amount of supercooled liquid, and weak MCAO index prevent riming, reducing snowfall rate and mean Ze. This could potentially explain why snowfall occurs less frequently on 04 April. Moreover, the lack of riming in updrafts would reduce the variability in snowfall rate within each fetch bin. Lacking preconditioning by riming might delay the precipitation onset on 04 April by more than 10km (Fig. 8p)which starts forming at fetches of 26 and 39 km on 01 and 04 April, respectively. “ (line 419).

Minor Concerns

ll. 22-23 Thinking of quasi-Lagrangian simulations that could be evaluated, it would be helpful to provide information regarding the boundary layer windspeed as well as the spatial and temporal gap between subsequent crosswind flight legs. Are flight legs really revisiting an air mass or are stationarity assumption needed here?
We thank the reviewer for the clarifying question. The impact of wind speed is discussed in Fig. 4, and flight legs are discussed in Sect. 2. The flight legs probe the same locations several times, they resemble each other, and have roughly the same length. No spatial gaps between adjacent flight legs exist. Hence, flight legs revisit the same location but not the same air mass.
In the abstract, we specified: „*The evolution and structure were assessed by flight legs crossing Fram Strait multiple times at the same location, sampling perpendicularly to the cloud streets.*“ (line 10).

l. 25 Maybe remove “accompanied by” or I’m missing the point and more explanation is needed here.
Done.

ll. 54 I tend to disagree and would soften this statement. There have been earlier and simultaneous efforts that explore MCAOs using satellite data in a quasi-Lagrangian manner (Wu and Ovchinnikov, 2022, Tornow et al., 2023).
We softened the statement: „*While cloud reflectance measurements by satellites have provided important insights into the geometrical appearance of MCAOs since their beginning, recent studies such as Murray-Watson et al. (2023), Wu and Ovchinnikov (2022), and Tornow et al, 2023 quantitatively studied cloud development in a quasi-Lagrangian way.*“ (line 52)

l. 222 Please specify “undisturbed”.
We dropped undisturbed.

l. 223 Please specify the panel(s) within Fig. 1 that the color can be found in.
Done.

ll. 282-283 Please specify where to find the low-level jet.
We modified the sentence: „*The ABL is capped by a low-level jet at 250m height (Fig. 4e)*“ (line 285).

Fig.3 (and also Fig. 4): I highly recommend showing subsidence at a level that is aligned with the cloud-top (and its free tropospheric entrainment). By virtue of looking at the surface layer, subsidence is essentially zero and can have no practical meaning here.
We thank the reviewer for this important remark and updated Figure 3. The updated version shows the subsidence at the median cloud top height of each day, i.e., 925 and 975 hPa on 01 and 04 April, respectively. Since we only retrieved the cloud top height along the P5 track and are not able to do so for all locations shown on the map, we decided to show subsidence at a constant pressure level for each day.
We rewrote parts of the analysis to clarify this (line 275):
„*At the height of the median CTH, here 925 hPa, air subsides within the ’prior to cloud streets’ and ’cloud streets’ regime, respectively (Fig. 3g, green and blue track). Over ocean, subsidence is generally reduced compared to over sea ice. The area of fetches between 75 and 120km around 7°E longitude is characterized by strong subsidence (Fig. 3a, c) throughout the entire atmospheric column (not shown) despite increasing SST and MCAO indices. This wave-like pattern is likely induced by wave effects originating from Svalbard archipelago (Shestakova et al, 2022).*

„*Compared to 01 April, the air mass at CTH (975 hPa) ascends for fetches larger than 60 km (Fig. 3h). A wave effect is notable within the region affected by the lee effect but not for the analyzed data west of the convergence line.*“ (line 293).

Fig. 3g and h (and also Fig. 1 e and f): I'm confused as to why only part of the flight track is shown (that evidently looks more complex as for example displayed in Fig. 1g). Perhaps the authors should mention why they only show a subset.
The mentioned Figures show the complete flight track of the Polar5 aircraft. The blue line in Fig. 1g depicts the longer and more complex track of HALO instead.

Fig. 4: I recommend a different set of colors here as they are hard to tell apart.
For clarity, we use the same color coding as in the plots before. To not have two blue colors in the first row, we changed the color of the cloud street observations with large fetches to gray.

Fig. 7 and 8: I recommend adding cloud droplet number concentration that is expected to decrease where riming and precipitation are active.
Cloud droplet number concentration was only observed onboard the aircraft P6 and radar observations, from which we retrieve up- and downdraft information, are only availabe for P5. Since we do not know the up- and downdraft regions for P6 observations, the composit shown in Fig. 7 cannot be generated for cloud number concentration. Moreover, a closer focus on in situ observations would be beyond the scope of this study.

ll. 357-358 Could the reverse be true, too (that is, greater ice particle size increases riming)?
The reverse might be also true as long as the rimed particles do not grow too large and thus do not sediment away from the liquid layer, which we can not prove. We did not add this speculative statement.

ll. 361-362 Why is most ice expected at Zemax = 0.6? More information seems needed here.
We explain why we assume that most ice is located at the height of the maximum *Ze* per profile in Sect. 3.2. According to Fig. 7k, this height is at 0.6 of the hydrometeor depth for updraft positions on 01 April.

ll. 379-381 Could CTH also be affected by a change in subsidence (e.g., Tornow et al., 2023) ?
At the pressure level closest to cloud top height, air generally subsides on 01 April and ascends on 04 April, respectively, as illustrated in Fig 3g, h. This subsidence pattern does not explain why CTH are on average shallower on 04 April. Thus, we assume that the general differences in CTH between the days originate from other factors discussed in more detail in line 385. Local variations of CTH in different magnitudes of fetch and their dependence on subsidence are discussed in manuscript line 389.

ll. 382-384 Again, I recommend using subsidence at CTH altitude to obtain a meaningful assessment.
See answer above.

Typos

l. 22 Maybe change to "These detailed cloud metrics are particularly well suited…"
Done.

l. 229 Please change to "80% < SIC < 100 %".

Done.

There are punctuation errors throughout (e.g., l. 20, ll. 232-234, l. 148). I recommend asking a native English speaker for their input.
Done.

References

Tornow, F., A. S. Ackerman, A. M. Fridlind, G. Tselioudis, B. Cairns, D. Painemal, and G. Elsaesser (2023), On the Impact of a Dry Intrusion Driving Cloud-Regime Transitions in a Midlatitude Cold-Air Outbreak. J. Atmos. Sci., 80, 2881–2896, https://doi.org/10.1175/JAS-D-23-0040.1.

Wu, P., & Ovchinnikov, M. (2022). Cloud morphology evolution in Arctic cold-air outbreak: Two cases during COMBLE period. Journal of Geophysical Research: Atmospheres, 127, e2021JD035966. https://doi.org/10.1029/2021JD035966