Review of Imke et al. 2024
Clouds and precipitation in the initial phase of marine cold air outbreaks as observed by airborne remote sensing doi: 10.5194/egusphere-2024-850

This manuscript focuses on 2 cold-air outbreaks observed during the AC3 campaign in April 2022 and describes their characteristics. The main motivation presented for the study is that the observations provide a valuable reference for future modeling studies.

This is a useful study, I do have a fair number of comments I'd like the authors to address.

Abstract: Why these 2 CAOs in particular? Are they representative? Where do they fall within the more comprehensive assessment of Murray-Watson for example? This is addressed later in the document, but would be good to bring into the abstract. Is there a new process insight or hypothesis developed from their study that is new to their understanding? It is not clear to me why modelers should choose these examples above others, other than the opportunistic sampling. The authors also don't come back to this argument towards the end of the document.

It makes sense to me that the 2nd CAO is the weaker of the 2, as it occurred 3 days later, and likely experienced similar synoptic conditions. The authors treat the 2 CAOs as if they are independent of each other. What initial conditions did they share? A figure indicating the dominant synoptic situation and how it evolved would helpfully complement the first figure. I hypothesize a coastal low interacted with the northerly CAO flow to develop that convergence line, for example.

In addition, the English is awkward in places. I recognize English is not the first language of any of the authors. I will point out specific places in the more detailed comments below.

More detailed comments:
Abstract, line 12: "the liquid layer is wider" is unclear here. Do you mean a larger horizontal dimension of the rolls? Except the previous sentence says the width of the roll circulation is similar between the 2 cases. "Occurrence" -> occurrence of what? "are" -> "have"

Line 30: "them" what is 'them'? Define

Lines 33-36: unclear to me how the 2nd sentence relates to the first. Isn't the first sentence saying vapor depositional growth is favored when the vapor pressure> ice and liquid saturation?

Line 37: awkwardly written.

Line 73: "were" -> "was"

Line 83: "extend" -> "extent". Same for line 391. And line 520.
Line 85: awkward. Overall the sentence is vague. What are the induced changes in dynamics and clouds?

Line 160-161: how do you know most of the liquid is near the top? As written this sentence sounds presumptuous. And what is the motivation from deriving LWP from the slanted profiles? Superficially the nadir profiles would make more sense.

Line 240: wouldn't the strongest updrafts generate liquid drops that are less obvious to the radar than the radiometer? Why expect ice production and liquid production to be collocated?

Line 256: 2.9 samples = what horizontal distance?

Fig. 4: this is the first introduction of the measurement regimes, so you may want to identify them in the caption, it's hard to understand this figure otherwise. Are these the same as provided in Table 1? the legend and symbol shapes are hard to see. Besides increasing them in size, would also suggest increasing the size of the overall figure.

Line 281: what is the uncertainty in the LLT metric?

Line 290: "precipitating" -> precipitation

Section 4.1: there really should be a synoptic analysis and figure provided, this would be a good spot for it.

Line 296: was VIIRS imagery, of higher spatial resolution, not available?

Table 2 is nice. Does the BLH top correspond to the cloud top, or the mixed-layer height? Indicating the range of cloud top heights would be useful also.

Lines 301-304: fig. 4 is difficult to follow to be honest. The dropsonde and ERA5 values don't agree well, the disagreement is more than I would have expected based on Seethala et al. 2021.

Line 308: I don't see mean thermodynamic profiles provided as part of fig. 1e,f,…..these figures only indicate the locations of the dropsondes.

Line 303: One also can't see from Fig. 4 that the MCAO follows the SST (SST isn't shown) and basically we are just seeing a few values for the ERA5 dataset, because of the coarse resolution. I would suggest increasing the size of the dropsonde-derived values in fig. 4, and including SST, and reducing the size of the ERA5 values. I am confused why there are so many ERA5 values. The interesting aspect of this figure is the sharp increase in the MCAO index for 1 April (isn't it)? How much of the fetch is over the MIZ? It would be useful to include the beginning point in the figure 4.

Line 314: where are you showing the surface flux values you refer to?

Line 315: here you define the BLH. This would be useful to include in the caption of table 2 as well.

Lines 328-330: I rather think a spatial plot of the ERA5 winds and Geopotential height would be more informative here.

Line 339: that the clouds only reach 1km and 500m respectively is interesting too. The CAOs documented at COMBLE tend to reach higher.

Line 342: awkward writing. Line 343: you could look at the near-surface RH to examine if more evaporation is happening.

Line 347: not convinced this is the best place to mention Morrison et al 2012. That is a modeling study based on more weakly-forced clouds. If you are going to mention it, better left to a discussion later on.

Fig. 7: this is a nice figure. The caption should state clearly what the 3 groupings represent. Is is the same for all panels? The lowest one is labeled differently. How wide/coarsely resolved are these distances? I just wonder if ice production is truly colocated w liquid production, or perhaps they get lumped together through a coarseness in the grouping.

Line 367: not following this sentence. What do you mean by extreme?
Line 368: 87 -> 87%
Line 370: spell out what '0.6 of D' means (it's easy to lose track of the acronym meanings).
Line 375: is the measurement limit known to the reader somewhere earlier? I don't recall seeing it.

Line 380: I think the clouds looked at within Shupe 2008 are different enough from CAOs that this analogy isn't quite right. It could just be a coincidence. You could say in the discussion section that examining if CAO clouds are a truly unique cloud regime that behave differently from non-CAO MPC is a topic worthy of future study (I suspect the differences in surface fluxes do genuinely separate CAO and non-CAO clouds).
Lines 381-383: if you want to keep this, it would be better suited for a discussion section towards the end.

Line 398-399: vague. Might help to indicate the distances of 'large' and 'small' fetches.

Line 407: to me it makes more sense to look at how cloud microphysics depends on the surface fluxes, as opposed to the exposure to open water. You can actually estimate the surface fluxes from the dropsondes, this was done within Seethala et al 2021. It would be interesting to know those values.

Line 408-409: this sentence doesn't make sense.

Line 413: here is where I think the reader learns the LWP uncertainty. Better to say the LWP values are beneath the maximum uncertainty of 30 g/m2.

Lines 427: how do you deduce the subsidence? From satellite imagery?

Lines 434- : I appreciate the effort to place the 2 CAOs into context and not claim the noted features are universal (though they well might be). It would be nice to see this sentiment expressed in the abstract as well.

Line 445: decreaseing -> decreasing

Line 452: Khanal et al 2020 examine MPCs using MODIS, cited below.

Line 475: since updraft speeds are correlated to surface fluxes, you could examine the dependence of riming on dropsonde-derived surface fluxes to put this statement on a stronger footing. Unless you have in-situ vertical velocities from one of the planes (it doesn't seem like it though).

Line 485: the -> these, before 'conditions'

Lien 486: do you show CTH<BLH somewhere? I don't recall seeing that.

Line 508: isn't dendritic growth through vapor deposition, followed by aggregation, a different growth process than riming? I'm not sure I understand how aggregation encourages more riming.
Line 508: and how do you know SST is rising? You haven't shown SST anywhere.

Line 510: this is difficult to follow, not sure what this sentence is saying.
Line 511: I think liquid droplets are typically more readily raised, because they are smaller than ice particles. Or maybe you just need to be more precise about the particle size - but I'm not sure you have the microphysical measurements to back up this statement. All in all, this paragraph is too speculative. Please dial it back.
Line 516: you are contradicting what you said on line 508.

Conclusions: I like that you brought it back to the motivating questions.

Line 549: I don't believe temperatures have to be colder than -20C for riming to be active, this is certainly not universally true, nor do you explicitly show this.

References:

Seethala, C., P. Zuidema, J. Edson, M. Brunke, G. Chen, X.-Y Li, D. Painemal, C. Robinson, T. Shingler, M. Shook, A. Sorooshian, L. Thornhill, F. Tornow, H. Wang, X. Zeng, L. Ziemba, 2021: On assessing ERA5 and MERRA2 representations of cold-air outbreaks across the Gulf Stream. *Geophys. Res. Lett.*, **48**, doi:10.1029/2021GL094364

Sujan Khanal, Zhien Wang, Jeffrey R. French, Improving middle and high latitude cloud liquid water path measurements from MODIS, Atmospheric Research, Volume 243, 2020, 105033, ISSN 0169-8095, https://doi.org/10.1016/j.atmosres.2020.105033.