This paper provides an extensive evaluation of multi-layer cloud systems in the Arctic (Svalbard region) in relation to warm air intrusions (WAIs) and cold air outbreaks (CAOs). It relies primarily on radiosonde deployments over ~20 years, while also using collocated radar retrievals to validate the presence of cloud layers. The authors find that clouds are generally thicker, and multi-layer cloud systems are more frequently observed during warm air intrusions when compared with those during cold air outbreaks. Other findings characterize differences in the radiative profiles and seeding characteristics of multi-layered cloud systems in WAIs and CAOs.

The paper demonstrates much effort put towards characterizing cloud layers and synoptic settings. Much of the authors' interpretation is consistent with the findings and all of the figures are clearly visible and well-manufactured. However, I have many concerns which I discuss below.

Major comments:

1. I understand that some studies similarly discuss multi-layer clouds in relation to the entire tropospheric profile as is done in this study, but this is problematic since the majority of studies evaluating multi-layer clouds refer to cloud layers restricted to the lower troposphere and in close proximity to each other (e.g., Herman and Goody, 1976; Tsay and Jayaweera, 1984; Verlinde et al., 2013). The latter are associated with shared ambient properties, in contrast with the former which can include cases such as low-level clouds with overlying cirrus.
    a) This distinction should be acknowledged in the introduction, and instead of using the verbiage "multi-layer clouds (MLCs)" the authors should choose something more appropriate for the entire column, such as "multi-layer cloud systems", or define the other cloud type as low-level multi-layer clouds.
    b) I also wonder if this paper warrants an evaluation of these low-level multi-layer clouds. Due to the extensive work put in by the authors, the paper is already quite extensive but I would encourage the authors to explore this avenue (the paper in its current form could be cut down without impacting major findings). This is particularly important since (I suspect) these low-level multi-layer clouds will be more susceptible to the WAIs and CAOs compared with upper tropospheric clouds. This could be a very valuable component to the study.
    c) Extending point b) further, you include clouds in the upper troposphere associated with RC3, which you do not even consider. This could be interpreted as problematic; and accounting for this might improve results.
    d) It would be nice if the authors could include a case of these low-level multi-layer clouds similar to Figure 1 to see how the authors' layer classification method performs.
2. The cloud layer methodology is extensive, and involves some previous studies' layer classification methodologies. However, it begs some questions:
    a. For the radar-validated layer classification, it states that "Radar echoes through at least 50% of the minimum layer depth of 100 m" must be met to have a radar validated layer. But how/why was this 50% threshold chosen? What dBZ threshold? These choices must be justified.

    b. I understand more flexibility is often permitted when using classification methods from previously published findings, but the difference in the number of "potential" layers (without radar verification) and layers with radar verification is strikingly different. Some minor form of validation is be warranted (see my recommendation in 5. below).
3. Parts of the methodology used to diagnose WAIs and CAOs seems relatively arbitrary. Specifically, this refers to diagnosing WAIs and CAOs using +/-σ diverging θ from the vertical θ profile. The authors do show that using this threshold roughly corresponds to these events' averaged properties on the synoptic scale (e.g., temperature, meridional vapor flux). However, this doesn't necessarily depict that these cases correspond with CAO's and WAIs. The authors should justify this σ threshold, or at least perform sensitivity tests showing results are robust.
    a. Further, the authors use multiple the rotated PCA method captures the three tropospheric levels, and plot results for three different categories of WAI and CAO (shallow, elevated, and deep). This is a lot of information. I wonder if much information is lost by just evaluating shallow+deep cases like in the latter portion of the paper.
4. The authors evaluate seeding, which is when ice particles from an overlying cloud fall into a cloud below and influence its properties. I have some questions/concerns regarding this:
    a. A major point of uncertainty is that the authors assume that the clear-sky region between the layers was not or evenly currently is impacted by the seeding of particles, however, if seeding were to occur it would moisten the underlying clear-sky region.
    b. Why do you only evaluate seeded particles up to 400 microns? Ice crystals grow much larger than this. Probably worth mentioning that (I assume) you don't need to evaluate larger particles since they all are seeding cases?
    c. More of a suggestion: are you confirming no radar reflectivity in the shaded regions? If not, could do this to improve analysis.
5. An important consideration when diagnosing cloud layers in this region is differences in cloud phase, of which little discussion is made in the study. A few points:
    a. The cloud layer classification method detects (super)saturated layers and diagnoses them as cloud. However, liquid should always be present at liquid saturation whereas high ice supersaturations have commonly been observed with the absence of ice. This distinction should be made clear. I also wonder if the wide gap between potential and radar-verified layers may be related to this. You could analyze whether layer classification differences arise when separately looking at layers with RHliq~100% and RHice~100%.
    b. One reason the authors mention for an absence of cloud is that aerosol concentrations are too low for clouds to form (e.g., line 378, 398). While this could be true for ice clouds, CCN concentrations are generally abundant in the atmosphere to varying degrees, and clouds will form when (super)saturated with respect to liquid. They cite a paper discussing low CCN concentrations (~10 cm^{-3}) over the region, which will impact cloud properties/lifetimes but it will NOT inhibit cloud formation. The authors should revisit and revise these statements.

Minor comments:

- Make sure all figures are put in the sections in which they are introduced
- As you did with most but not all of the figures, it's helpful to introduce them with some basic description (e.g., fig. 7).
- Numerous times throughout the paper you spell out "multi-layer clouds" although "MLCs" had been introduced.
- Lines 8-9: Is this difference including all the years without radar verification?
- Lines 11-13: The dependence on layer distance is important for all particle sizes correct?
- Lines 13,14: "modulates … most effectively" doesn't make sense. It either increases or decreases the cooling
- Line 55: "turbulent heat"?
- Line 62: "High-Arctic"?
- Line 98: These are the radiosonde instruments? Is spelling out the acronym important?
- Line 103-104: Is the manufacturer GRUAN? Can you find a source for this?
- Line 108: is the "Cloudnet target classification product" obtaining cloud boundaries?
- Lines 124-126: This is true by definition.
- Lines 131-3: This sentence is confusing. I'm unsure what this exactly means.
- Line 136: "a layer may be classified as subsaturated even when it is actually a cloud" This isn't true for liquid clouds correct (their decay is on the order of seconds)?
- Line 137-8: This is confusing because you mention the seeding calc before even discussing your plans to do this.
- Line 140: So is RH* simply 95%?
- Line 140: Rather than say it's reclassified as supersaturated, just say it isn't considered in the analysis (we don't know how it should be classified)
- Line 158: Do you know this is recently activated?
- Line 160: "cold enough to drive ice nucleation." This is wrong. Do you mean homogeneous nucleation?
- Line 165-6: You should quickly give an explanation, especially if you're combining two approaches.
- Line 177: Delete "begin"
- Section 2/3: It would be nice for readability to rename RC1 and RC2 sooner, just for readability (maybe something like +RC2 = shallow WAO-test?).
- Figure 4: These are not thick and thin lines, but solid and lightly shaded.
- Figure 4: Is it necessary to show the sigma's? this is a busy plot with a lot of lines.
- Page 14 is blank (maybe journal with the formatting error)
- Line 363&369: you have a long and short dash
- Line 404: "Arctic-gateway"?
- Line 415: you didn't define SLC as single-layered cloud
- Line 422-3: This is obviously true in all cases.
- Line 438: not a particularly notable difference. Test for significance?

- Line 476: ‘low-level clouds’
- Line 505: Is this possibly related to CAO clouds at lower altitudes, within well mixed boundary layers for example with high moisture content?
- Line 528-9: Source?
- Line 553-4: These temperatures aren’t shown in the figure correct? Specify when findings are not shown.
- Line 668-670: Why? Couldn’t it just be related to macro-scale differences? Should also be careful about interpreting p-values.
- Perhaps the journal’s formatting error, but multiple sections including author contribution and acknowledges are located within the appendix.
- Figure 1: y-axis label is inconsistent with y-axis tick values

Bibliography

Herman, G. and Goody, R.: Formation and Persistence of Summertime Arctic Stratus Clouds., Journal of the Atmospheric Sciences, 33, 1537–1553, https://doi.org/10.1175/1520-0469(1976)033%3C1537:FAPOSA%3E2.0.CO;2, 1976.

Tsay, S. C. and Jayaweera, K.: Physical characteristics of Arctic stratus clouds., Journal of Climate & Applied Meteorology, 23, 584–596, https://doi.org/10.1175/1520-0450(1984)023%3C0584:PCOASC%3E2.0.CO;2, 1984.

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