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# *Interactive comment on* "Processes contributing to Arctic cloud dissipation and formation events that bookend clear sky periods" *by* Joseph Sedlar et al.

**Anonymous Referee #3**

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In this study the authors attempt to explore reasons for dissipation and formation of low clouds in the Arctic, using a multitude of data from the ARM site in Utqiagvik (Barrow). They first isolate clear-sky periods using a ceilometer and refine these with additional data. They then proceed to analyze data from lidar aerosol backscatter and from in- situ surface measurements of aerosols, radiation and basic meteorology as well as indicators of atmospheric tendencies from soundings. They do this using composites of data for four years.

Their effort is ungrateful in the sense that it turns out to be very difficult to tease out any solid relationships. This is, while of course frustrating, in itself not a reason to reject a paper; a negative result is also a result, and it all rests with how this is handled. However, the paper could be better organized and more clearly written. I recommend that the paper is accepted after major revision focusing more on the structure and language of the paper, more than on the results themselves.

We thank the reviewer for their detailed review of our manuscript. We have considered each suggestion, comment and criticism and have provided detailed responses to each below (in red).

Major comments: This is an original way to analyze data, and the approach is interest- ing. I commend the use of more than cases studies; while this is likely a reason for the lack of clear results, it represents a way to obtain more general results. Anyone can dig out a single case and speculate about reasons for a given outcome, but this is close to useless in a more general sense unless it can be shown that results are more general.

We appreciate the reviewer's commendation of the methodology of our paper. Our intent was to avoid the "case study pitfall", allowing us to more generally characterize the first order processes critical to cloud lifecycle changes.

While this is a strong case for this paper it is also a bit of a weakness in the present manuscript. The background to the problem and the motivation for the method is pre- sented in a very hand-waiving fashion; the current introduction reads more like a list of previous studies and suggestions than an organized argument. Many examples of suggested aerosol influence is listed, but isn't it quite clear why. While aerosols are certainly important, different clouds form mainly because of dynamics than by aerosol constraints. Different types of clouds form in different situations and differently at dif- ferent locations because of different predominant dynamics; low clouds in the Arctic Ocean, frontal clouds in extratropical cyclones and deep convection in the tropics. All of this is modified but not determined by aerosols.

We agree with the reviewer's concern. We have revised the introduction to more appropriately frame the research question that, as of now, the role of aerosol versus meteorology in its contribution to cloud dissipation or formation on the North Slope of Alaska (NSA) is unknown. We use this as a motivation to provide two hypotheses to this research question: 1) clouds are responsive to aerosol presence, or lack thereof, as has been reported over the central Arctic sea ice. 2) General meteorological processes, such as near surface thermodynamic modification or active transient synoptic forcing, are crucial in determining the lifecycle of low level clouds on the NSA.

Hence, I wish that the authors more deeply criticize and discuss the problem of rep- resentatively, as a motivation to stay away from case studies, and then present more clearly the hypotheses they are attempting to test including potential effects of atmo- spheric dynamics. As it stands, I get the impression they throw whatever data they can lay their hands on, on this problem in the hope that something might show up. I also miss the motivation to why four years of data is used; why not five – or ten?

Please see our response to the previous comment. Our revised introduction does a much better job at framing the research question and developing hypotheses related to aerosol versus general meteorology in determining the fate of low level Arctic clouds. We use the five years (2014-2018) of data because these are the only years where all the instrumentation analyzed were operable simultaneously. This could have been extended by an additional year to 2019, but the results were produced and the original manuscript was being written in mid-to-late 2019.

The paper – even its title– makes a big deal of the clear periods, but if one is inter- ested in cloud dissipation or formation, presumably the happenings before and after the shoulder times are the interesting things; not the clear period per see. Isn't the clear period in between in itself sort beside the point? Also, when clouds are dissi- pated, presumably new clouds will form at some later time, hours or days later; the formation of the new clouds at the end of the clear period may have absolutely nothing to do with the dissipation of the other clouds hours or days earlier. Calling these "book- ends" is misleading in that the reader is lead to think of this as a coupled sequence of events; they may in fact be entirely different. Hence the focus should have been on either cloud dissipation or cloud formation – or both but separately – and then focusing on before and after cloud dissipation/formation.

We are confused with the reviewer's criticism, but we feel this may be mostly related to the title and scope of the introduction. All the results shown in paper, except for potentially Fig. 3, are devoted to understanding the processes connected with the onset of cloud dissipation and the onset of cloud formation. All of the analysis is focused on the vertical structure and variability of aerosol backscatter, near surface thermodynamics and winds, and thermal advection (geopotential thickness tendencies) in time periods prior to cloud dissipation/formation compared to what those time periods just after dissipation/formation.

To address the reviewer's concern, we have changed the title to better reflect that the actual clear periods themselves are not the focus. Further, we have removed any unnecessary discussion of the clear periods in the introduction, besides the important statements related to a need to understand what controls the processes leading to cloud formation and dissipation. NEED TO CHECK THIS!

This constitutes a problem with the lidar, since it is difficult or even impossible to ob- tain aerosol backscatter in the presence of low clouds, attenuating the lidar signal. This is just a fact of life and is discussed on lines 226-227, as in the passing; this information should be given and discussed up front. The results in Figure 3 should therefore be discussed in the context if being clear skies; not in the context of not being cloudy, since that contrast just isn't there. Of course it may still have some value to look at aerosol backscatter directly after dissipation and directly before formation in a statistical sense, as in Figure 4, but this caveat should be discussed up front; that the one set of plots represent after dissipation has happened while the other set is before cloud formation. Without knowing what the structure was before dissipation and after formation of clouds, the information value is limited. And BTW, is this really cloud dissi- pation/formation; isn't it just a hole in the cloud layer advected past the viewer? Maybe this is why its so hard to get statistically robust results?

As stated by the reviewer, the limitations of the HSRL are the reason we had to rely on studying the vertical distribution of aerosol in the time window just after dissipation and just before cloud formation. We supplemented this with Fig. 3 to show the monthly climatological vertical distribution and its variability during entirety of the clear sky periods. To address the reviewer's concern, we have described the limitations of HSRL backscatter profiles together with the description of the instrument in Section 2.

At the end of the discussion section a hypothesis is formulated, almost like in passing; I'm sorry, but I don't get it. It builds on the Tjernström et al (2019) air-mass transfor- mation hypothesis. But a central tenet in that hypothesis is the fact that over melting sea ice, the surface temperature is locked constant at the freezing point; here there is no analogy. So is cloud dissipation leading to surface cooling, then aerosol pooling, followed by fog formation, fog deepening and lifting to clouds?

That would in essence mean that cloud dissipation leads to cloud formation? If this chain of events is really happening, it should be a testable hypothesis; temperature should drop while aerosol concentrations rise with time, followed by fog formation and cloud base rising from zero to some height; in gact, the very same set of data used here could be used to test this hypothesis. Instead the hypothesis is not even clearly repeated in the conclusions, but brushed over with many words in paragraph two and beginning of paragraph three. If you want to pose a hypothesis, do it; else don't!

We have considered the reviewer's critique and we agree with the reviewer. We have removed this hypothesis description in the revised manuscript, mainly since we do not have the modelling capacity to test this hypothesis. This was similar to a critique from another reviewer.

Finally, the language is sometimes what I would – in lack of a better description – call "flowery". It is important to have a capturing narrative, but unnecessarily complicated sentence structures sometimes lead to confusion and misunderstanding. So maybe sometimes be a bit less imaginative.

We appreciate the suggestions and we have gone through, with multiple "sets of eyes" to remove colloquial language throughout the manuscript.

Minor comments

Line 28: Drop "even".

Removed as suggested.

Line 29: Please rephrase; the temperature of low clouds do not reach "as cold as -34 °C" in "all seasons".

As suggested, we have rephrased the sentence to the following:

Liquid-bearing clouds have been observed at temperatures as cold as -34 °C (Intrieri et al., 2002), but liquid is most common during the warmer, summer months (Shupe et al., 2011).

Line 14: Unnecessarily complicated. Suggest "While clear sky is less frequent than clouds" or even "While clear skies are rare".

The manuscript has been revised to read: "While clear sky periods are relatively rare, …"

Line 38: Lack of what? "longwave warming" or "Arctic clouds"?

The "lack of" statement has been removed from the sentence to avoid confusion.

Lines 39-40: Only true when the sun is absent or the albedo is high; over bare land and in summer, clear skies usually leads to a surface warming. Even in the Arctic.

This statement is based off of results from Pinto et al. (1997). We have revised the statement to read the following, based on the reviewer's suggestion:

Under cloud free conditions with low solar elevations, effective infrared cooling from the surface results in near-surface temperatures to drop (Pinto et al., 1997).

Lines 41-44: A prime example of when there are too many ideas in the same sentence. Exactly what is it that "is currently understood". I know all this so I understand what you mean, but please rephrase anyway.

Following the reviewer's suggestion, we have changed revised the statement as follows:

The Arctic boundary layer tends to remain relatively shallow following the lack of buoyant mixing because stratocumulus cloud-top generated turbulence is absent during clear skies.

Line 43: "stratocumulus and also"

We assume the reviewer is referring to line 47 and not line 43. However, we argue that the original sentence structure is grammatically correct, whereas updating to "stratocumulus and also" as suggested does not make grammatical sense.

Lines 50-51: I would move up "in the Arctic" in that sentence, or it sounds like the transition everywhere is controlled by Arctic clouds.

We have moved "in the Arctic" from the end of the sentence to the beginning, as suggested by the reviewer.

Line 59-60: So opaque liquid clouds would form out of what? Optically thin ice clouds?

We agree with the reviewer that this statement is relatively vague and difficult to follow. We have revised this statement as follows:

Based on observations from the North Slope of Alaska (NSA) and complementary simulations, Silber et al. (2020) found that clouds forming under low aerosol concentration regimes are incapable of producing the cloud-top turbulence necessary to maintain cloud persistence.

Line 71: In what regard is that?

This statement has been removed in the revised manuscript.

Line 71-72: This is a sentence where the narrative is that clouds dissipate and form at the beginning and end of the clear period, as if the dissipation and the formation where reverse analogs.

We agree with the reviewer's concern, and therefore this sentence has been removed from the revised manuscript.

Lines 74-77: Here is a completely different take; now the formation clear period is at focus, not the dissipation of formation of the clouds.

We respectfully disagree with the reviewer; the text, which has been kept intact from the original submission, has always detailed that the purpose of this paper was to explore common processes or differing processes found around events of formation or dissipation of Arctic lower tropospheric clouds. Below is the actual statement from the original submission, which we have kept in the revised paper:

More specifically, we assess whether the aerosol and the general meteorological variability provide clues to the processes that are important for lower troposphere, below 2 km, cloud dissipation and cloud formation events. By comparing and contrasting the variability of such properties shortly after cloud dissipation (start of clear period) and shortly prior to cloud formation (end of clear period), we aim to learn how changes in aerosol number, aerosol vertical partitioning, and atmospheric thermodynamics contribute to formation and cessation of clear sky periods in the Arctic.

Lines 106-107; what has "a diameter of 10 to 3000 nm"; the volume of the air or the partciles? I know the answer of course, but the sentence is rather unclear.

The statement has been revised to the following:

At the surface, a TSI 3010 condensation particle counter (CPC) measures the number of particles ranging in diameter from 10-3000 nm present within a volume of air.

Line 107: Do all cloud-relevant aerosols absorb alcohol, or do we miss some?

Aerosol composition has a small effect on the detection limit and therefore the counting efficiency of those very small particles (D < 20 nm). In the size range of cloud-relevant aerosols (D > 40 nm), there is no dependence of the counting efficiency on the aerosol composition (see Pg. 9 in the following instrument handbook: https://www.arm.gov/publications/tech_reports/handbooks/doe-sc-arm-tr-227.pdf).

Line 129: Grater than identically zero?

The statement has been revised to the following:

"…point where a cloudy detection status (greater than zero) re-emerged…" to "…time when the ceilometer once again detected cloud overhead and the cloud persisted for at least 2 consecutive hours."

Line 136: How is the agreement on clouds between the ceilometer and the HSRL?

We found the agreement to be surprisingly good. The HSRL is a more sensitive instrument, and therefore there were instances where the HSRL backscatter exceeded a threshold designated as clear sky. Additionally, the CL31 ceilometer has a maximum range of 7600 m, and as such the HSRL would identify cases when higher clouds were present; these instances were excluded from the analysis.

Because of the overlap of the HSRL, the first effective range level where valid data was returned was around 100 m AGL. The ceilometer has a smaller overlap and therefore instances with fog or very low cloud bases were reported by the ceilometer but not by the HSRL. Combining the HSRL with ceilometer and KAZR cloud radar provided a sufficient means to screen the atmosphere for cloud hydrometeor presence.

Line 146: I assume the base is at 100 m and the top is at 400 m; neither is between 100 and 400 m.

The reviewer is correct, and we apologize for the confusion with the original wording. This statement has been revised.

Lines 172-174: Another long sentence with more than one idea confusing the other. Is there any other way a clear period can end than by the emergence of a cloud? And is the ceilometer ever operating in anything but vertical mode?

Following the suggestion of the reviewer, we have revised this entire paragraph, including updating the figure to show the number of cloud formation events that were identified as low clouds and those that were identified as fog.

In regards to the statement about ceilometer operating in vertical mode: The original text did not state ‘vertical mode’ but ‘vertical visibility mode’. This is a change in the ceilometer processing retrieval software. When the laser beam is attenuated at a range gate very close to the instrument, the retrieval switches from attempting to estimate the cloud base height and instead provides a measure of the vertical extinction of the laser – providing a measure of the vertical visibility. This is what was meant by ‘vertical visibility mode’. Regardless, this statement has been removed from the revised manuscript.

Line 188: Not all months have a clear elevated “level of maximum variability”. Figure 4: Why one hour?

The reviewer raises a fair point. The wording has been more carefully discussed to reflect that not all months contain elevated variability in aerosol backscatter.

We choose time windows ranging between 1 and 2 hours around cloud dissipation/formation times in order to closely examine any changes in aerosol (and later in meteorology) that may have had an influence on the cloud lifecycle. The reason the time windows changed in duration was connected to the temporal resolution of the datasets/instrument(s). The HSRL reported data more frequently (30 s) than the other instruments examined (1 min). Therefore we choose longer time windows for the lower temporal resolution data streams.

Lines 226-227: This is really important information to have before looking at Figure 3 & 4.

We agree with the reviewer’s statement. As such, we have included statements in Section 2 under the description of the HSRL, as well as in the opening paragraph of Secion 4.2.1. These statements specify that HSRL backscatter is only analyzed when the period has been determined to be completely cloud free using a combination of measurements from the HSRL, ceilometer and KAZR.

Line 239: What type of aerosol particle would not come from “below”; what aerosols do not have an origin at the surface except for those emitted by aircraft?

This statement has been removed from the revised manuscript.

Line 278: “agrees” with what?

We are unsure statement the reviewer is referring to, as original Line 278 did not include the “agrees” terminology.

Figure 6: Why now 2 hours; earlier it was one?

As described above, we have accommodated the analysis periods around dissipation or formation events based on the temporal frequency of the measurements. The extension to 2 hours was to consider the lower frequency observations, but also to better capture whether or not variability in the distributions of CPC concentrations was connected to broader changes in the air mass properties.

Line 279: You are not exploring “phenomena”; you are exploring variables and trying to infer “phenomena”.

Phenomena has been changed to processes.

Line 325: ”strongly transparent”? Better say “almost opaque”.

Almost opaque is the complete opposite of how the Arctic atmosphere interacts with infrared radiation during clear sky conditions; the atmosphere is transparent, hence its reference as the window region. However, based on this confusion and the suggestion from another reviewer, we have changed “strongly” to “largely” to better reflect our meaning.

Lines 342-344: Not sure I get this; if the dew-point deficit has a positive trend (is increasing) and the temperature has a negative trend (is decreasing), does that neces- sarily mean RH is increasing? Could the dew point not decrease so much more than temperature that RH stays constant or even decrease?

Following the reviewer’s criticism, as well as comments from reviewer 2, we have computed the tendencies in relative humidity with respect to ice (for November-May) and with respect to water (for June-October) and compared these to tendencies in the temperature. Calculations of relative humidity with respect to ice or liquid were based on the monthly mean near-surface temperatures observed at the NSA.

Line 383: “in flux”? Maybe chose a different wording?

This description has been removed in the revised manuscript.

Line 423: About the source of aerosols again; isn’t this trivial? Moreover, I think aerosols are defined as “airborne . . . particles” so there’s one “airborne” to many here. Line 424: “general stable stratification” is probably incorrect, or

The reviewer is correct, and these statements have been removed from the revised discussion section. Further, we have removed the word “general” and left stable stratification.

Line 364-365: This is a bold sentence, supported by only one reference. I’m not nec- essarily disagreeing, but still.

We have toned down the statement here, as it was mainly meant to be a transitional sentence motivating the need to analyze the synoptic situation.

Line 383: “in flux”; is this a good choice of words?

This description has been removed in the revised manuscript.

Line 423: Here are the aerosol sources again; I’m no expert but unless you emit them from an aircraft, don’t they have to come from the surface?

The reviewer is correct, and these statements have been removed from the revised discussion section. Further, we have removed the word “general” and left stable stratification.

Line 424: The statement on “general stable conditions” is probably inaccurate or at the very least debatable. Studies have shown that the most common near surface stratification over the whole year is near-neutral, but that stably stratified conditions prevail in clear conditions especially in the winter when they are also deep and strong. Additionally, is there no ground based convection over Alaska or at Barrow; I get over the ocean but this is on land?

While near-neutral stratification close to the surface may be most common, this is primarily due the high fraction of low level stratiform cloudiness although their mixed layer may not always extend down to the surface and therefore indicate a decoupled stratification. When the clouds clear, however, the lack of downwelling longwave critically impacts the surface energy budget, as we have shown with the stability metric in Fig. 7. There may be instances of near-neutral stratification, depending on the time of day during the clear sky period, but predominantly, the lowest ~500 m are stably stratified.

Lines 485-489: Here's that hypothesis; I would have much liked to have the hypothesis at the front and the paper about testing it, or at the end as a bridge to the next study. Here it isn't even a conclusion; reading a bit hasty one could have missed it.

Being that we do not have the modelling simulation to support the formulation of our original hypothesis, we have decided to remove this discussion in the revised manuscript.

Line 511: Maybe avoid the word "transparent" in this context, as it is so intimately linked to other things in this manuscript.

We understand the reviewer's concern, and we have changed the wording to "apparent".