Review of “Processes contributing to Arctic cloud dissipation and formation events that bookend clear sky periods” by J. Sedlar et al.

This manuscript presents an analysis of the atmospheric state (including aerosol concentrations) right before and after the onset of cloudy and clear periods at Utqiagvik, Alaska. The main motive of the work is to understand the processes that drive low-level cloud formation and dissipation in an Arctic environment.

I find the overall aim of the study and the analysis of available observations interesting and commendable. However, it seems like the manuscript was put together a bit too hastily; the overview and connection to published literature could be expanded (in particular in terms of Arctic aerosols), the presentation of the instrumentation and methods needs more information and the discussion of the results lacks some clarity and depth. On the data analysis side, I also find some issues with the way that the aerosol data from the CPC are treated. As stated in the manuscript, the data from the CPC will give you the total aerosol number concentration, including aerosols down to 6 nm diameter. This is a problem, at least during summer, when the total aerosol number concentration is dominated by smaller aerosols (nucleation and Aitken mode), which have very little influence on cloud droplet formation. Relating the aerosol concentrations from the CPC with cloud formation is therefore dubious.

We thank the reviewer for their insightful review, with particular attention to the aerosol focus of the manuscript. We have considered all the criticisms, comments and suggestions provided by the reviewer, and we have replied with detailed responses to each comment below, in red.

**General comments:**

- • I would suggest that the authors are a bit more careful when they use the term “the Arctic” or when they refer to certain characteristics of “the Arctic”. The Arctic is not a homogeneous region where clouds, meteorology and surface properties are the same. Many of the features that the authors mention, in particular in the introduction, may not be true for the lower-latitude parts of the Arctic and/or land areas. For example, are clouds ubiquitous over the whole Arctic during the whole year? Does the longwave radiation dominate the radiative energy budget everywhere and during the whole year? Under cloud-free conditions, does effective infrared cooling from the surface cause extremely cold temperatures everywhere? I am thinking for example of Siberia where you in the summertime can have very different conditions compared to over the Arctic Ocean.

The author raises a valid point. Utqiagvik on the North Slope of Alaska (NSA) is only one station with the Arctic region. However, the NSA is still a part of the Arctic, and on top of that, it is home to long data records which make a statistical study like this one possible. To address the reviewer’s concern about representativeness of Utqiagvik as the Arctic, we have changed the title of the manuscript to better reflect the study region. We have kept the motivation in the introduction section regarding Arctic clouds and subsequent clear sky periods as being very important to the surface energy budget of the Arctic; we feel that although the conclusions drawn may not represent the entire high-latitude region, we do feel that they are at a minimum representative of northern Alaska coast.

- • Related to the previous comment, how representative is Barrow as a station for “the Arctic” and the type of cloud formation/dissipation events that you study? I think that the idea that aerosols control cloud formation/dissipation has mainly (only?) been presented for high (>80$^{o}$N) Arctic clouds, i.e. in pristine environments where (accumulation mode) aerosol number concentrations are extremely low. Utqiagvik (or Barrow) has rather high (accumulation mode) aerosol concentrations for an Arctic station (cf. e.g. Freud et al., 2017 or Schmale et al., 2018). It may still be an interesting place to study low-level cloud formation and dissipation, but perhaps not so much from the perspective of an aerosol-limited regime?

After working on this study, we agree with the reviewer that Utqiagvik is likely not a representative location for study the aerosol-limited regime. The presence of land and nearby ocean, often ice free for a considerable portion of the year, revealed that a lack of lower tropospheric aerosol is not a common occurrence on the NSA. However, that does not discredit the attempt made here to quantify from the NSA whether or not signatures in near-surface and lower tropospheric aerosol may indicate a relationship or connection to the mechanisms contributing to dissipation and/or formation of lower troposphere clouds. We have made it clear in the revised discussion and conclusions that changes in aerosol presence are not the cause for cloud dissipation. When it comes to cloud formation, especially fog formation which we have separated and paid more attention to in the revised manuscript, the role of large concentrations of both small and also fewer but larger aerosol in summer are linked with air mass transformation. The end result supports the formation of fog following clear sky periods. To our knowledge, this result and mechanisms promoting summer fog formation, has not been identified on the NSA previously.

- • The authors use CPC measurements to relate aerosol concentrations to cloud formation/dissipation events. Firstly, I think that the methodology related to the CPC measurements needs to be better explained. What air is pumped into the instrument? Is it “whole air”, “cloudy air” or “clear air”? How are ice crystals and cloud (fog) droplets handled by the instrument? Is the air dried? Does the instrument have any detection limit in terms of number? Secondly, the CPC measures particles down to 6 nm (as stated by the authors). The Arctic is typically dominated by small aerosols in summer (cf. e.g. Freud et al., 2017) but these small aerosols are not efficient cloud condensation nuclei. Figure 3 in Freud et al. shows that in summer, the accumulation mode particle concentration typically goes down drastically while the total concentration of aerosols goes up as new particle formation and growth controls the aerosol population. Why did the authors not use Scanning Mobility Particle Sizer (SMPS) aerosol size distribution or CCN measurements from Utqiagvik? I think these should be publically available (cf. e.g. Schmale et al., 2017).
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Air is sampled continuously in all conditions, so the inlet would be best described as a whole air inlet. There may be some losses of the big hydrometeors (ice/droplets) due to inlet design, but those are unlikely to have significant effect on the number concentration. Details regarding the inlet and sampling strategy can be found in Quinn et al. (2002; doi:10.1029/2001JD001248, 2002). In terms of detection limit, communicating directly with the responsible instrument PI, even the highest concentrations observed at Utqiagvik have never reached it; the actual detection limit is not certain but the PI was sure it is above 20,000/cc. This is far greater than any of the concentrations that were measured during our study period.

Our study is designed to be a statistical study from 5 consecutive years cloud dissipation/formation events when all of the measurements/instruments described in the methods were operational. Going back to 2008 to examine CCN and/or SMPS measurements for a handful of cases is not the intention of this paper. Furthermore, unfortunately, the instruments the reviewer suggests to analyze were not in operation during the 2014-2018 study period, following the link to Schmale et al. (2017). We even reached out to the instrument contact PIs from the group (TROPOS) that is supposedly operating an SMPS currently on Barrow, to see if we could kindly have access to their data to include in this study; we received no response back from them.

However, to satisfy the reviewer's concern with potential new particle formation events, we have looked at the brief amount of SMPS data available during September 2007 to mid June 2008; see responses below in specific comments section. The only case we could find during summer (where NPF events are common) occurred in mid June 2008 and the results do suggest signatures of NPF. We feel this further supports our claim that processes are contributing to enhanced particles numbers during clear sky periods on the NSA – which support cloud formation. We have further explored the scattering and Ångström exponent behavior of these distributions (see figure in specific comments below as well as Figure 7 in the revised manuscript) and these results suggest that while CPC numbers are very large, there is still a component to the aerosol distribution that contains larger particles (more efficient as CCN) based on the scattering and Ångström exponent behavior.

Unfortunately, CCN measurements stopped in 2012 at Utqiagvik.

• I find the discussion about the vertical structure of geopotential height and "synoptic activity" and their relation to cloud formation and dissipation events confusing. In Section 3 (lines 387-398), the authors say that "From May through summer, differential advection amongst the atmospheric layers becomes a more frequent occurrence." From this, they conclude that cloud dissipation events are often associated with baroclinic activity in summer. I would also assume then that the *synoptic activity* is more frequent in summer during cloud dissipation events. The same is also true for cloud formation events (lines 400-409); these are more frequently associated with synoptic activity in summer compared to winter. But in the discussion section, it is stated that (in association with cloud formation events) "Variable dynamics resulting in differential atmospheric advection is most prominently observed during the winter and early spring. Furthermore, in the conclusions, the authors state (in relation to cloud dissipation events) "While we report that all months are subjected to synoptic disturbances, the magnitude of the forcing is weaker during late spring and through early autumn than during winter and early spring."

We appreciate the concerns raised by the reviewer, and we have carefully considered these comments when revising the manuscript. First, we have moved on from using the radiosounding-derived geopotential thickness tendencies, instead using ERA5 reanalysis profiles of geopotential height to derive thickness tendencies. Radiosoundings were nominally every 12 hours, and as Reviewer 2 commented, it is very possible that the two consecutive sounding profiles occurring prior to a cloud dissipation for formation event may not be representative of the synoptic setting that actually impacted the thermodynamics. With ERA5, we are able to use 1-hourly profiles, from which we derive thickness tendencies across a 4-hr period prior to a cloud dissipation or formation event. We assert that the 4-hr timescale is more relevant to the synoptic forcing influencing the event, as well as it provides a sufficient number of data points in which to produce a tendency. Furthermore, we are able to calculate the 4-hr consecutive tendencies for each month of a season during the 5-yr period, which provided a mean and standard deviation of the seasonal layer thickness tendencies. We used this seasonally climatology to understand when specific dissipation/formation events exceeded the 5-year climatological standard deviation. These revised results, and the discussion following them, are now discussed more thoroughly in the revised manuscript. In particular, we have removed confusing and contradictory statements, like those described above by the reviewer.

**Specific comments:**

*Abstract:*

- • Line 2: I would suggest reformulating the sentence including "...lack of downwelling...". It sounds like there is no downwelling radiation at all when the cloud is absent.

We have removed the statement about the lack of downwelling longwave in the revised abstract.

- • Line 18: I am not sure why you emphasize the link to aerosol concentrations here? Isn't any general change in dynamics/radiative cooling more important?

This statement has been removed from the revised abstract.

*1. Introduction*

- • Line 27: Are there any other studies than Shupe et al. (2011)? Would be interesting to know.

There are many studies prior to Shupe et al. (2011) that document the vertical distribution of clouds, many of these are connected to individual field campaigns or satellite observations that pre-date the Shupe et al. study. Shupe et al. (2011) use pan-Arctic observations (a number of "supersites" containing a variety of active and passive remote sensors) over a number of years to document the vertical partitioning of Arctic clouds. Since our paper relies on observations from one of the observatories analyzed by Shupe et al., we have decided to retain this as the most appropriate reference.

- • Line 27: I suggest changing "These clouds frequently contain concentrations of both..." to "These clouds frequently contain both ...".

Changed as suggested.

- • Line 54. "Simulations of Arctic clouds consistently show that over-abundant ice nuclei or ice crystal concentration can lead to cloud glaciation". I don't think this statement is completely true – it depends on what the authors mean with "over-abundant" and "Arctic clouds". There are several studies that show that mixed-phase clouds in the high Arctic only glaciate at extremely (i.e. unrealistically) high ice crystal number concentrations, e.g. Stevens et al. (2018), Loewe et al. (2018).

We have updated this line to address the reviewer's concern. The revised manuscript now states:

Simulations of Arctic clouds consistently show that enhanced ice nuclei (IN) or ice crystal concentrations can lead to mixed-phase cloud glaciation (Harrington et al. 1999; Jiang et al. 2000; Avramov and Harrington, 2010; Morrison et al., 2011), as ice precipitation acts a net sink of cloud mass (cf. Solomon et al., 2011; Forbes and Ahlgrimm, 2014).

- • Line 56: Related to the previous comment, I think a CCN-limited regime has only been suggested for high Arctic clouds?

We tend to agree with the reviewer in that we also have only seen the CCN limited regime to be present over the central Arctic sea ice. However, with these statements, we are setting the stage for how our analysis will explore both the aerosol vertical distribution characteristics as well as meteorological forcing properties in the attempt to understand the processes important for cloud dissipation and formation on the North Slope of Alaska.

- • Line 61: In this paragraph, it could perhaps also be worthwhile considering the studies by Young et al. (2018) and Dimitrelos et al. (2020) where they point out the importance of large-scale divergence/convergence (and associated free tropospheric moisture supply) in governing the lifetime of Arctic low-level clouds.

Based on the reviewer's comments, along with 2 other reviewers, we have revised the introduction to better identify the scope of this paper. In this regard, we have made careful effort to identify observational and modeling studies that have highlighted the importance of cloud lifecycle evolution due to microphysical changes (CCN, IN) as well as synoptic forcing. We thank the reviewer for alerting us to the Young et al. 2018 paper, which we have included in the revised manuscript.

- • Line 75: When reading the introduction, I was wondering why you focus on atmospheric properties "after cloud dissipation". It would have made more sense to look the atmospheric state before cloud dissipation. In the methods section you then explain why this is not possible, but I think it could be good to include a short explanation already in the introduction.

We try to be consistent throughout the paper. In this respect, we do look at the atmospheric properties both before and after cloud dissipation and formation events. However, for the HSRL analysis, because the lidar signal dominated by cloud hydrometeors, we cannot study the changes in vertical distribution of aerosol backscatter prior to dissipation, or shortly after formation. To address the reviewer's comment, we have changed the introduction to include the the following sentence:

"By comparing and contrasting the variability of such properties around cloud dissipation (start of clear period) and around cloud formation (end of clear period) events,…"

*2. Instruments*

- • Line 91: The description of the HRSL is very brief and should be expanded. For example, what is the detection limit of the lidar? Is there a limit in terms of how close to the surface the signal can be trusted?

After a literature search, we could not find a standard value listed as the detection limit for this particular HSRL. However, we note the backscatter cross sections below $1x10^{-7}$ ($m^{-1}$ $sr^{-1}$) were generally not observed (See original Fig. 5), and therefore could be considered the lower detection limit for a pristine Arctic troposphere. This backscatter detection limit was also identified by Shupe (2007) as the threshold for a completely clear (clean, pristine) atmosphere. A study by Thorsen et al. (2017, **https://doi.org/10.1002/2017GL074521**) has explored various sensitivities of other HSRLs in comparison with CALIOP onboard CALIPSO. That study was focused on how the sensitivity in aerosol backscatter from lidar would be critical for aerosol optical depth estimates. However, in our study, we are more interested in the vertical presence of aerosol layers, in particular if there are sharp contrasts in the vertical distribution of enhanced or dimishied aerosol backscatter cross sections. Our analysis did not show this to be the case. Following the reviewer's suggestion, we have included information regarding the first vertical range analyzed.

- • Line 1010: How small concentrations of small cloud droplets can the cloud radar observe?

Generally, this will be dependent upon whether ice crystals are present or not. We have included the following line to the revised manuscript:

While the KAZR is capable of observing concentrations of small droplets, its measurement is sensitive volume squared and therefore the signal may be attenuated in by the presence of ice crystals which are typically larger than droplets (e.g., de Boer et al., 2009).

*3. Methods*

- • General: it would be nice to have a map of the location of the station and also a brief description of the typical conditions (closeness to sea, potential pollution sources etc.)

We argue that the Arctic science community is generally well-versed in the geographic location of Utqiagvik and the North Slope of Alaska. Therefore we have decided against including a map. We did however include the following paragraph to the start of Section 2 describing the general cloud conditions, general air mass footprint and connection with pollution from nearby oil fields and wildfires:

The observatory at Utqiagvik is an ideal location to understanding the contribution of meteorological and aerosol processes to Arctic cloud dissipation and formation. Generally, cloud fractions are high, typically between 60 and 95%, and lower tropospheric clouds were common, especially during sunlit months (Shupe et al., 2011; Sedlar, 2014). Having a relatively large cloud occurrence makes the NSA a viable location to further study the process that lead to the formation or cessation of a clear sky period. Utqiagvik is at a coastal site, located within 2 km of the coast line along the NSA. Seasonal climatologies of the back-trajectory footprint of air masses reaching the observatory were predominantly from the high Arctic Ocean, and to a lesser extent from the continent to the south (Freud et al., 2017). Pollution from the oil fields around Prudhoe Bay did not regularly lead to changes in background aerosol or cloud microphysical properties at Utqiagvik (Maahn et al., 2017). However, wildfires may sporadically influence the background aerosol concentrations and chemical composition across the NSA during active fire seasons (Creamean et al., 2018).

- • Line 130: I'm just curious, why 96%?

There is no real scientific reasoning for the choice of 96%. At some point, it was necessary to "allow" occasional observed cloud signatures within our definition of a clear sky period, otherwise our study would have been limited to very few cases and would not be sufficient as a statisical study.

- • Lines 138-140: I suggest replacing the word "when" with "if".

Changed as suggested.

- • Line 146: Why show times as UTC and not local times? Would make it easier to interpret the radiative fluxes.

This is a matter of preference, and we have decided to keep the hours in UTC time.

• Line 154: It is not completely evident to me that the mixed layer (elevated aerosol backscatter) is shallower during the clear period. How do you see this? Maybe it would help to draw a line at the start of the clear and cloudy periods?

We interpret the mixed layer depth variability as the height where the HSRL backscatter beings to drop off dramatically with height. Prior to cloud dissipation (around 04:00UTC) this backscatter transition occurs above the 300 m level. This level shows a gradual decrease in height up until around the mid-point of the clear period (~ 08:00UTC). This is what we refer to as the shallower mixed layer, which is further shown in the inset of equivalent potential temperature profiles in panel c; there we find the mixed layer depth has decreased rather considerably, indicating a mixed layer depth of < 200 m. This layer depth is below the previous cloud base height (~300 m). We do not have a radiosounding during the cloud dissipation phase, but if we assume the cloud was coupled with the surface just prior to dissipation, this would suggest a decrease in mixed layer depth of more than 100 m had occurred.

- • Line 155: "Evolution in near-surface meteorology showed modest changes...". I interpret "modest" as "not pronounced", but maybe this is not what the authors mean. I would say that the change in wind direction is fairly pronounced at the time of cloud formation? And also the change in dew point temperature?

We have revised the statements to reflect the changes observed in wind direction and near-surface thermodynamics, as suggested.

- • Line 157: It is quite interesting that the particle concentrations increase so dramatically during the clear period. In summer, new particle formation and/or condensational growth of nucleation mode particles often takes place when there is sunlight and (initially) low background concentrations of aerosols (e.g. Freud et al., 2017). Could this be what is happening? Was this a typical pattern or only a one-time feature? Important here is of course also what air the CPC samples, if it is "whole" air or only cloud-free air.

As the reviewer knows from further reading, this was not a one time example, but a relatively consistent process especially during the summer. We have continued with this analysis, following the reviewer's suggestions and questioning below, in subsequent sections of this manuscript. However, we do not feel that it is appropriate to hypothesize on new particle formation at this point in the manuscript, as we are using Fig. 1 to simply show the typical setting of a dissipation event, clear sky period, and formation event.

*4. Results*

- • Line 165: Just out of curiosity, was there any difference in length of the clear periods between the seasons?

Generally, no. Each month tended to have clear periods that ranged from about 3 hours to as many as 26 hours; the longest clear periods were infrequent and therefore skewed the distributions.

- • Line 170: I assume that the clouds with bases below 400m also could include other clouds than fog and low clouds? For example nimbostratus, cumulus and cumulonimbus.

While this is possible, the typical low cloud type across the Arctic is the low level stratocumulus, often mixed-phase. The identified fog events are unlikely to be anything other than fog (which is by definition a cloud with a base level at the surface and reduced visibility). In the revised manuscript, more consideration has been made to separate the low cloud and the fog cases to identify whether different atmospheric processes or mechanisms could be linked to the different cloud formations.

- • Line 188: What is the "1-sigma envelope"?

The 1-sigma envelope referred to the 1 standard deviation around the mean profile at each height. This statement has been removed in the revised manuscript.

- • Lines 190-194: I have several questions/comments regarding this paragraph.

○ When is the boundary layer backscatter (which should be dependent on the aerosol surface area, so mainly the accumulation mode) the highest/lowest? How does this agree with other in-situ measurements of CCN and/or aerosol size distribution measurements (e.g. Freud et al., 2017; Schmale et al., 2018; Schmeisser et al., 2018)

These measurements of HSRL backscatter are valid for a limited number of clear sky periods only, not the entire monthly distribution as is the case for the studies listed by the reviewer. We find that in terms of CCN concentrations, the seasonal cycle shown in Fig 3 (along with Fig. 6 later in the manuscript) are very similar to those measured from Utqiagvik (Lubin et al., 2020). We have included this important connection with our analysis of Fig. 6 later.

○ Is it really true that the "transition layer" is the shallowest in summer? October and September looks pretty shallow too?

We have revised the statement to reflect the reviewer's point; the revised sentence is:

For example, the summer and early autumn (g-k) mean backscatter decrease happens over a shallower layer above the surface and is more abrupt than during winter and spring (a-f).

○ I don't understand the sentence that begins with "Many processes may contribute to ...". Shouldn't this layer just be a result of the vertical depth of the boundary layer/mixed layer?

This is correct, and we did include one of the BL processes that the reviewer is referring to, namely the lower atmosphere stratification. However, we have included 'boundary layer mixing' in the revised manuscript to satisfy the reviewer's concern.

• Line 213: The limitation of the HSRL should be mentioned in Section 2.

The revised Fig. 5 shows the median and interquartile spread of the seasonal aerosol backscatter for low cloud and fog forming events only. Below the cloud layer, it is apparent the backscatter is considerably larger than above cloud top. While we may be approaching the detection limit of the HSRL above the cloud top, and further above into the free troposphere (see panels g-h), it is clear the aerosol backscatter in the layer where cloud would eventually form is above this backscatter. Therefore, we do not see any evidence that the HSRL would miss small concentrations of aerosol particles. Even if it were that small aerosol concentrations were below the detection limit of the instrument, the fact that aerosol backscatter remained larger after the cloud dissipated compared to just before it formed suggests that a sparsity of aerosol (with which to be activated as CCN) was not the reason for the cloud dissipation; it certainly did not inhibit the formation of cloud.

• Line 214: Can you really draw this conclusion from looking at averages? I would think that in order to make this statement, you would have to look at the individual profiles and make sure that the transition layer is always below cloud or within the cloud that the clear-sky period bookends?

This statement has been removed from the revised manuscript. We agree with the reviewer that the climatological profiles may prohibit potentially small scale features which may be ongoing on a case by case basis.

• Line 221: The selection based on a maximum cloud top height below 2km makes sense and should be done from the beginning.

The specification that this paper focuses on cases with clouds below 2 km has been added to the last paragraph in the introduction.

• Line 236: The cutoff backscatter values should be mentioned in Section 2. But I am also wondering what the authors mean with "clear sky"? I assume there should still be aerosols present, it is just that the instrument cannot detect these low concentrations?

In the revised manuscript, this section has been updated. We have specified that the threshold of $1\text{x}10^{-7}$ ($m^{-1}$ $sr^{-1}$) was developed by Shupe (2007) as the distinction of pristine Arctic air. While there may be aerosols present, having such a small contribution to the backscatter suggests their cross sectional area must be small meaning the number concentrations should also be small.

• Line 241: What do the authors mean with the sentence "Being that the aerosol backscatter... was at minimum..."? Where and how do you see this?

This statement has been removed from the revised manuscript.

• Line 241: Related to the comment above, how low backscatter values would you need in order to have accumulation mode aerosol concentrations below ~10cm-3?

This would require the use of a forward model of Mie scattering, which is beyond the scope of this paper.

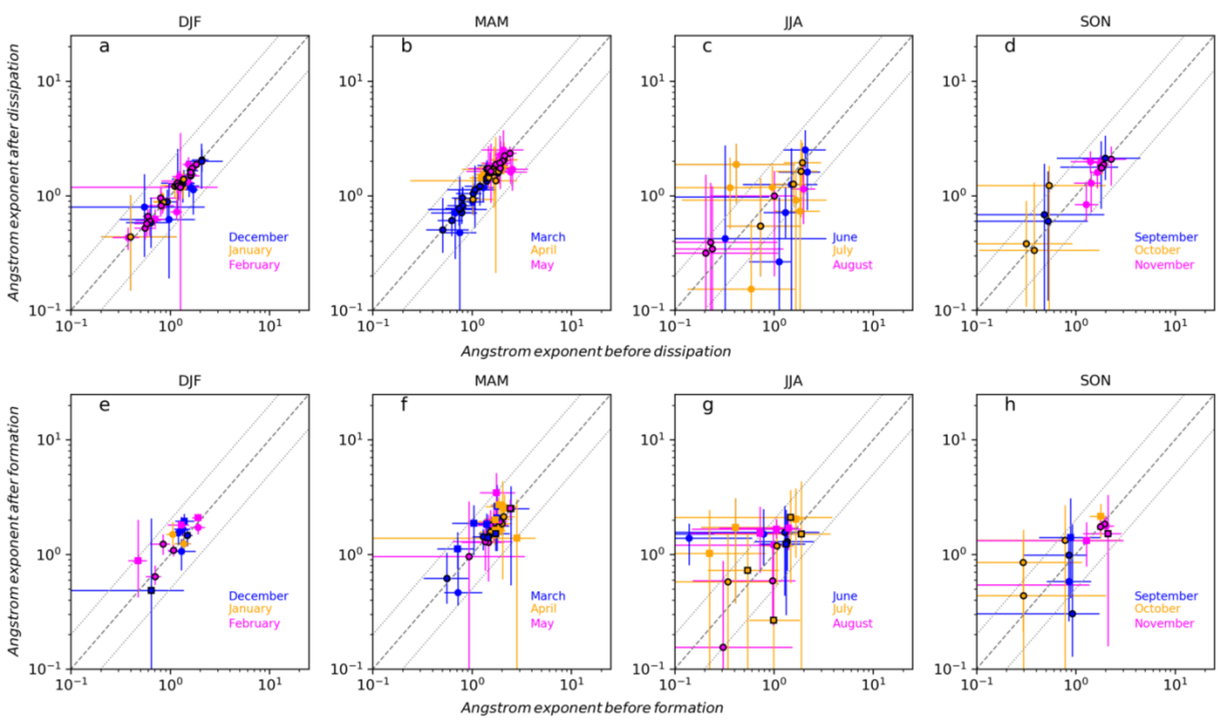
- Line 248: Please define "RFD".

RFD has been defined as relative frequency distribution, as suggested by the reviewer.

- Lines 257-260. I do not think this argument holds. The backscatter will be dependent on surface area. If the aerosol population is dominated by small particles in summer, then the surface area will not be at its maximum, see also Freud et al. (2017).

We agree with the reviewer, in that the original statement was misleading. We have expanded the analysis in this section by the following: 1) we identified a bug in our plotting of Fig. 6, where instead of the interquartile range being plotted, error bars were mistakenly plotting the median value +/- the 25th and 75th percentiles. 2) we have included the median and interquartile range of the 550 nm scattering coefficient around cloud dissipation and formation times. This figure illustrates that the scattering coefficient during summer tends to be as large, or larger, prior to formation than after formation (panel g). That the scattering coefficient has not consistently decreased suggests a sufficient presence of accumulation model particles that much more readily scatter light compared to a distribution dominated by smaller Aitken or ultrafine particles.

We also looked at the Ångström exponent in the same manner; see figure below. We find that the exponent was similar or even smaller prior to formation compared to after formation (g). As the Ångström exponent is inversely proportional to size, it is clear that it's not only very small particles but also larger particles are contributing to the size distributions and the increase in particles observed by the CPC.

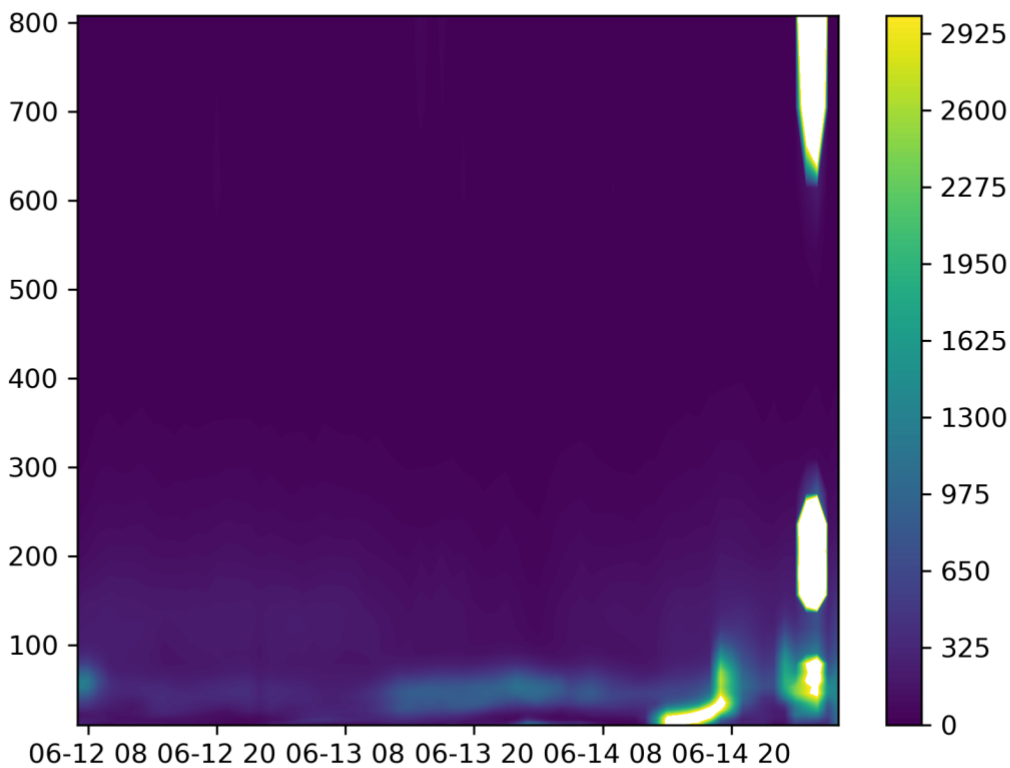


Same as in Figure 6 of manuscript, but for the Ångström exponent.

- Lines 269-271: This results is interesting as the increased number of particles in spring/summer could be due to new particle formation and growth during clear periods, please see previous comment (Chapter 3, line 157).

We agree with the reviewer that this result is interesting, and we have based much of our discussion around the formation of fog during summer around a number of processes ongoing near the surface; with the increase in particles, potentially from new particle formation events, playing a role in the formation process. We have revised the discussion section to highlight this.

As discussed above, SMPS data was not available for the time period of this study. However, we did look back at the September 2007-June 2008 SMPS dataset and did a similar analysis of cloud free periods then. Below is a figure that shows one particular clear sky period during June 2008, showing the time evolution of the SMPS particle size distribution. There is evidence of signatures that are consistent with a new particle formation event (potential banana curve) occurring towards the end of the clear sky period.




- Lines 271-274: Does the CPC measure “whole air” or only “clear air”? If it is “whole air”, then why would the concentrations decrase?

The CPC measures what the reviewer calls "whole air". The inlet samples air continuously during all conditions. Following the changes observed in the near surface thermodynamics and the lack of wind direction/speed changes for fog events, we have no reason not to believe that the decrease in particles is not from an uptake through activation into a fog droplet.

- Line 290: I do not think this argument is true. The downwelling LW should also be dependent on the temperature, in particular if the LWP is larger than ~$20 gm^{-2}$ (emissivity close to 1).

We have revised to text to highlight the importance of emission temperature once the cloud imitates a blackbody, as follows:

"…in the data since LWN is primarily proportional to cloud infrared emissivity (which asymptotes at liquid water paths between 30-50 g $m^{-2}$ (e.g., Shupe and Intrieri, 2004)) and the effective temperature difference between the cloud (or clear sky) and surface,…"

- Line 293: How is the analysis affected by any presence of a stable surface layer (boundary layer decoupling)?

The methodology of calculating equivalent potential temperature differences between the surface and 950 hPa will include any increases in potential temperature found within this layer. If there is a stable layer at 20 m AGL, or 200 m AGL, a potential temperature difference between 950hPa and the surface will be reflected in this calculation.

- Line 297: I think it should be mentioned in Section 3 that you use the soundings to calculate LTS.

Such a statement was already included in the original manuscript – see line 119-120. We have kept this in the revised version.

- Line 300: Related to figure 7, why is the cooling generally smaller with more stable stratification (for clear sky)?

It is likely that these instances are associated with significant temperature and/or moisture advection at low levels, contributing to a stronger temperature inversion in the lower troposphere. Even though clouds are absent, increased temperatures, especially in the presence of enhance moisture, will cause a relative increase in the downwelling longwave, which will act to offset the LWN deficit. The very strong LTS values are consistent with strong, low-level temperature inversions.

- Line 318: Which mechanisms are you referring to?

The next section explores the relationship of cloud dissipation and formation events to near-surface thermodynamic, winds and synoptic changes, which we link to the changes in aerosol characteristics.

- Line 342: So this means that in summer you mainly have fog formation due to radiative cooling?

This is one of the primary findings that we are asserting in this paper. The subsequent analyses and discussion section further emphasize this point.

• Line 356: Are these results then inconsistent with the geopotential tendencies where you concluded that synoptic activity was more frequent in summer and spring during cloud dissipation events (lines 395-398)?

We don't believe the results to be inconsistent. The original manuscript used 12 hr radiosounding profiles to calculate the layer thickness tendencies. Depending upon the start/end time of a clear period, this meant that the thickness tendencies could be computed a full 12to 23 hours prior to the actual dissipation or formation time. Using reanalysis has allowed us to reduce the potential for increased time lag between the thickness tendencies and the time periods of interest. The updated figures and results are consistent, namely that abrupt synoptic frontal forcing, while occurring occasionally, is not the primary feature observed from spring through autumn. Instead during these seasons, low cloud and especially fog formation events are connected with the least amount of variability in near surface wind direction (and wind speed differences) and also have the smallest layer thickness tendencies.

• Line 365: For the analysis of geopotential tendencies, I think it could also be interesting to look at these from the perspective of large-scale subsidence and convergence as in Young et al. (2018) and Dimitrelos et al. (2020). It would also be interesting to look at vertical profiles of moisture to see if the layer right above the cloud is a source or sink of moisture.

While these studies are interesting and show a connection to large scale structure, they are not explicitly focused on synoptic scale forcing. Young et al. shows that simulated cloud lifecycle is associated with divergence, although this can emerge through stagnant air mass modification as well as synoptic forcing. The Dimitrelos et al. study also limits the change in cloud to changes in divergence and its impact on cloud top processes.

While we agree with the reviewer that processes occurring near cloud top are important, we simply do not have the temporal availability of profiling in order to match the statistical climatology of our study. This would be more geared toward a case study analysis. We intend to use a cloud resolving model to further explore a handful of these dissipation cases in a future paper.

• Line 372: I would suggest inserting a "vertical" before "structure".

Revised as suggested.

• Line 380: How much was the number of cases reduced?

Because we now rely on 1-hr reanalysis data, the analysis is completed using all available dissipation and formation cases.

• Line 401: You mean in late spring/summer…?

In the revised manuscript, the description of he figures and the analysis of the results show have been completed redone. This statement no longer exists in the revised manuscript.

*5. Discussion*

• Line 430: I am not convinced that differences in horizontal advection is the main reason for the differences in vertical distribution of aerosols, see e.g. Freud et al. (2017).

In terms of seasonal variability, it is likely that changes in vertical distributions of aerosols results from either advection or cloud processing and deposition. Freud et al. demonstrated the footprint of aerosol typically extends from over the central Arctic. Mauritsen et al. (2011) found that over the central Arctic, aerosol concentration can potentially fall well below 10 cm-3. These “pristine” air masses are generally not stagnant and must be transported across the Arctic. Our results have identified that synoptic forcing at the NSA was largest in winter and spring, weakest during summer. Combined with the larger variability in clear sky aerosol backscatter across the lowest ~1 km during winter and spring, we are left to conclude that advection is indeed an important mechanism in aerosol vertical distribution.

However, in the revised manuscript, we have removed the statement that the reviewer has questioned.

*6. Conclusions*

- Line 499: I thought the forcing from synoptic disturbances was stronger in late spring through summer (lines 395-398)?

We understand the confusion that was raised by these contradictory statements in the original manuscript. Following Reviewer 2’s concern with using infrequent (12 hr) radiosoundings to understand thickness tendences, we have revised the analysis to use 1 hr reanalysis profiles from the state of the art ERA5 reanalysis. Now using a 4 hr window prior to dissipation or formation events to compute tendences constrains the analysis to focus on the synoptic evolution directly connected with a cloud lifecycle event. The new results continue to reveal that winter is more synoptically active than summer, while spring, and to a lesser extent autumn, represent transitional seasons in synoptic activity. These results have been more carefully described in the Section 4.3.2.

- Line 511: I guess there is also a possibility that the cloud formation and dissipation events does not happen “in-situ” but rather that transport of clouds (and clear air) contribute to the observations made at Utqiagvik?

Absolutely. We only have observations at one point so this paper only attempts to analyze the ongoing processes surrounding the clear sky periods.

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