Review of "Processes contributing to Arctic cloud dissipation and formation events that bookend clear sky periods" by J. Sedlar, A. Igel, and H. Telg.

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Overview

This paper aims to shed some light on the processes that control the dissipation and formation of low cloud in the Arctic. Such cloud is near-ubiquitous, but infrequent cloud-free conditions are important because of the large contrast in the surface radiation budget between clear and cloudy conditions. Models fail to adequately represent Arctic boundary-layer cloud and (operational forecast models) often fail to reproduce observed cloud free conditions. There is thus a definite need for improved understanding of the processes controlling these clouds.

The approach taken here is to utilise 5 years of measurements from a long-term measurement site at Utqiagvik, on the north coast of Alaska. The measurements include lidar backscatter (a proxy for aerosol concentration profiles), cloud radar, radiosondes, surface meteorology, and surface measurements of total aerosol concentration. Most of the analysis focuses on the ~1 hour period following cloud dissipation or preceding cloud formation that 'bookend' periods that are entirely cloud-free.

The analysis first considers the relationships between cloud dissipation/formation and aerosol profiles, comparing the profiles immediately after/before the transition with those for the clear periods as a whole (broken down by month), going on to consider the surface aerosol concentration either side of cloud transitions, and relationships between aerosol and lower tropospheric stability under both clear and cloudy conditions. This analysis provides no significant evidence for a causal link between aerosol properties and cloud dissipation/formation at the measurement site.

The analysis then considers thermodynamic and dynamic processes. This analysis leads the authors to conclude that "the onset of clear sky periods, and subsequently the end of clear periods, are primarily responsive to transient atmospheric forcing". For the onset of cloud they essentially conclude that under clear skies radiative cooling causes a fall in temperature and associated increase in relative humidity; ultimately saturation point is reached and provided there are sufficient aerosol present low cloud or fog will form. No firm conclusions are drawn about the processes resulting in the dissipation of cloud, other than the association with 'transient atmospheric forcing'.

These conclusions are rather generic and unlikely to help improve modelling of Arctic cloud.

The results remain of interest in providing a picture of typical conditions and some seasonal variations thereof, for periods of clear air bookended by low level clouds. There is considerable scope to improve this picture, however, and I recommend major revision before publication is considered.

We are grateful for the detailed review of our manuscript provided by the reviewer. We have responded with detailed replies to each criticism, comment and suggestion made by the reviewer below (in red).

General/major comments

While the aim of the paper is very worthwhile, I feel it ultimately fails to deliver robust conclusions. In part this is a, perhaps inevitable, result of the limitations of the data set. The aim is to understand what the processes are that lead to cloud dissipation/formation – transient events that are inherently linked to changes in local air mass properties over time. Measurements from a fixed site are, however, unable to distinguish between temporal evolution of the air mass properties resulting from in situ processes and the simple advection of a pre-existing spatial gradient in properties past the measurement site. This is a perennial problem for intensive, and/or long-term measurements. The authors attempt, but I think ultimately fail, to work around this problem by studying the statistics of an ensemble of cases. This provides correlations between measured properties associated with cloud transitions, and the hope is that probable processes can be inferred from these correlations. It is quite possible that observed behaviours might only be explicable by specific processes, and a fairly robust conclusion may be drawn. Sadly I don't think that is the case here.

As the reviewer understands, due to the sparse, detailed observing networks in the polar region, we are limited to specific locations or time periods to study processes criticial to cloud lifecycle changes. To avoid the trap of "case studies", we used 5 years of observations and statistical processing to identify features that are linked to the dissipation and/or formation process of clouds on the North Slope of Alaska (NSA). Using these statistics, we respectfully disagree with the reviewer about our study's lack of delivering conclusions. While cloud dissipation events have been studied in greater detail, the processes leading to the reemergence of lower tropospheric clouds has received considerably less attention. Following the reviewer's suggestions below, we have applied more focus on the separation of the type of forming cloud (base above 400 m, base below 400 m, or fog). This separation and evaluation of vertical aerosol distributions, near surface thermodynamics and winds, and larger-scale transient synoptic distributions has led to an understanding of forming Arctic clouds that has not been reported in the literature. While we cannot state that all the relevant physical processes have been explored, we have documented that on the NSA, the variation in aerosol has little impact on cloud dissipation; instead large-scale atmospheric forcing (exceeding the background seasonal variability in climatological forcing; revised figure and analysis – see detailed comments below and section 4.3.2) has not been reported previously; we feel this is an important result emerging from this study.

Aerosol Analysis

The analysis of links with aerosol properties is quite extensive, but ultimately finds no causal links with cloud dissipation/formation. The extensive initial focus on aerosol is (I assume) prompted by results from the central Arctic Ocean where very low aerosol concentrations (< 10 cm^{-3}) have been found to result in clear sky conditions even when the boundary layer is saturated, and several modelling studies have found that it is essential to accurately represent the aerosol in order to effectively represent the cloud and boundary layer structure. (as a side note, I find it odd that while the authors cite 3 modelling studies, all of which utilise the same observed case from the ASCOS project, they don't cite the original observational paper that first documented such CCN limited conditions and on which Sedlar is a co-author).

The CCN limited conditions in the central Arctic, are from a very different environment from the coastal site used here. The surface aerosol measurements in figure 6 and 7 show that concentrations rarely fall much below ~100 cm^{-3}, and are often much higher – far too high for aerosol to be the limiting factor on cloud formation. I think this possibility could have been ruled out much more easily by simply evaluating the surface concentrations (and perhaps relating them to the lidar profiles) for clear sky cases, without the need for the extensive analysis presented here.

The aerosol backscatter profiles show a consistent decrease with altitude through the boundary layer and across the top of the boundary layer and (former) cloud top. This is consistent with a surface source of aerosol. A surface source such as wind-blown dust would include some quite large particles with a significant sedimentation velocity, this would result in the sort of decrease with altitude observed here. No modification of aerosol concentrations by cloud is required.

We believe that it is important to document that aerosol processes controlling the dissipation of clouds over the central sea ice are, as reported here, very different than at the NSA. For this reason, we felt it necessary to highlight that cloud dissipation did not connect with aerosol changes. That because the surface CPC measurements remained high does not mean that changes in aerosol backscatter – proportional to the cross sectional area of aerosol concentrations – would not show indications of sharp gradients in the profile; for example a density gradient of enhanced/diminished aerosol backscatter across the boundary layer or above. This was not observed in the statistics. However, many of the LES and cloud resolving modeling studies referenced in the introduction attempt to emulate changes in background aerosol by varying CCN/IN numbers, conversion efficiency, and sedimentation processes through precipitation. The results from our study suggest that such processes are not of first order importance in determining whether a cloud should dissipate, especially during winter. However, during summer, cloud formation, especially fog formation, is frequently associated with relatively calm synoptic forcing. Enhanced concentrations of aerosol, with some particles still large enough to influence the scattering, (see responses to specific comments below and Figs. 6-7 in the revised paper) and therefore be efficient CCN should conditions permit nucleation to droplets, has not been reported previously. Our study found that the relatively calm synoptic forcing led to thermodynamic adjustment near the surface, in the presence of more particles, supporting fog.

Dynamics/thermodynamics analysis

The analysis in figure 8 reveals an interesting difference in thermodynamic behaviour in the hours prior to cloud formation between summer months (May-August) and the rest of the year. In the summer a decreasing trend in temperature (cooling) prior to cloud formation is accompanied by a decrease in dew point suppression – an increase in relative humidity. No such association is found for the rest of the year, where dewpoint suppression is more or less constant regardless of trends in temperature. The potential link to cloud formation in the summer is clear – increasing relative humidity will eventually result in saturation and condensation. The lack of change in dew point suppression in winter is ascribed to the cooling temperature trend resulting from advection (of increasingly dry air) rather than local cooling. No additional evidence is provided to support this supposition, and it is not clear why there should be a seasonal separation between local cooling and advection of cooler airmasses. Another possibility is that during the winter months the temperature is below freezing and the humidity of air is controlled by the saturation vapour pressure with respect to ice not water. Cooling will enhance this, resulting in growth of ice/frost by vapour deposition and keep the relative humidity with respect to water suppressed.

Following the reviewer's comment, we agree that the dew point depression was not the meaningful tendency. We have revised this analysis to explore the change in relative humidity with respect to temperature changes. We computed relative humidity with respect to ice for the months November through May, and with respect to liquid for June through October; we based these calculations on the mean monthly temperatures. The revised figure and analysis is a better method to explore

changes in both absolute humidity and temperature changes and removes the potential for the results to be controlled by vapor deposition to the surface during the very cold winter and spring seasons.

It is not clear that radiative cooling at the surface will necessarily explain cloud formation – cooling at the surface will tend to lead to increasing stable stratification, suppressing turbulent mixing and keeping the cooling localised to a shallow layer close to the surface. Air aloft might remain unaffected and at constant temperature. Eventually we might expect cooling to result in fog formation, but the formation of an elevated low level cloud depends on more than just surface cooling – mixing sufficient to maintain a more or less well mixed layer that cools as a whole, and an adiabatic profile so that the upper part of the layer saturates first. No attempt is made to distinguish fog and elevated cloud layers in the analysis, although this would seem to be an important distinction from the perspective of the process for cloud/fog formation.

Following the reviewer's suggestion, the revised figures and analysis surrounding them separates low cloud and fog cloud formation events. Please see the detailed responses related to this suggested revision below.

The analysis of geopotential layer thickness trends I find wholly unconvincing. The data points in Figure 10 are mostly very scattered, and in most cases it would be hard to make out a convincing trend by eye. A line can always be fit to the points, but does not imply a robust relationship.

Further, I have serious doubts about whether the calculated tendencies are meaningful, even on a case by case basis. The trends are calculated from 2 consecutive radiosonde profiles prior to the cloud transition. This means, usually, over a 12-hour interval. The example clear sky case shown in figure 1 is barely 9 hours long. The 2 closest sondes preceding the onset of cloud at the end of the clear event actually span the dissipation of the preceding cloud. The later of the two sondes is 1.5 hours after the dissipation, and about 7 hours prior to cloud formation. I would suggest that the geopotential height trend calculated here is more relevant to the dissipation event than to the formation event to which it is actually applied.

Given that we have both clearing and cloud formation both occurring within an interval less than that over which a single geopotential height trend estimate is calculated, that rather suggests that any correlation between the two is suspect at best, and potentially entirely spurious. To make a really meaningful evaluation a much higher time resolution is required for the geopotential height trends. Maybe the output from an operational forecast model would provide a better measure here.

We have considered the reviewer's comments and we fully agree with their concerns. To better capture the thickness tendencies that may have been connected to cloud dissipation or formation, we have analyzed the 1-hr profiles of geopotential height from ERA5 reanalysis. From these profiles, layer thickness tendencies were computed in the 4-hr period leading up to a cloud lifecycle event. The use of reanalysis allowed us to calculate the standard deviation, giving a measure of the climatological, seasonal variability in consecutive 4-hr layer thickness tendencies. We used this variability to quantify the seasons where cloud dissipation/formation events were associated with anomalously large thickness tendencies, suggestive of significant synoptic forcing.

Detailed comments

Line 57: Hines & Bromwich (2017, 10.1175/MWR-D-16-0079.1) also model this case, with similar conclusions to Birch et al.

This relevant reference has been added as suggested.

Line 61: pedantic gammar point 'a myriad of complex processes' should be just 'myriad complex processes' (myriad = countless, so 'there are countless processes' not 'there are a countless of processes'. Or classically myriad = 10,000, with similar implications for the grammar)

Noted and changed. Thank you for identifying this slip.

Line 106: '...measures the number of particles present within a volume of air...' -> 'measures the concentration of particles...'

Changed as suggested.

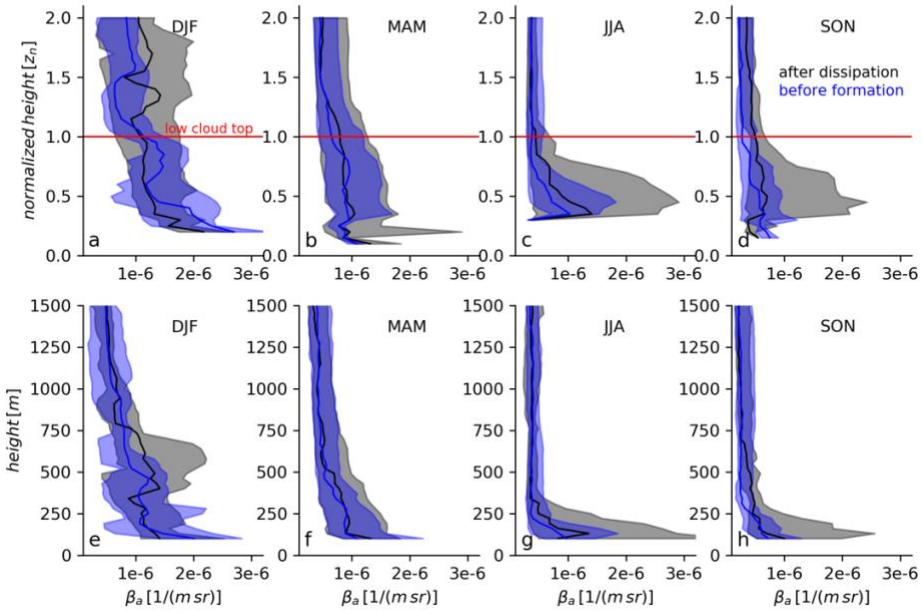
Line 173: the authors note how low cloud and fog can be distinguished here, but never use this to separate out the cases, which I think is relevant for some of the process identification.

We thank the reviewer for stressing this point. We have taken the reviewer's suggestion and included the separation between low cloud and fog cases in order to distinguish whether systematic differences in aerosol and meteorology can be linked to low cloud versus fog formation processes. We have also updated original Figure 2 to include the monthly number of fog formation cases.

Line 189: the authors note a peak in the variability in backscatter between a few 100 metres and ~1km. This is presumably a result of variability in BL top, and the associated gradient in aerosol & backscatter across it. This is not mentioned here, and throughout the discussion of figure 3 the profiles are discussed in isolation from any consideration of BL depth. I found this frustrating – there are several places where a feature of these profiles is discussed and some inference made, where my first reaction was that this was a result of variation in BL depth and this point was apparently being missed (see notes below). Same with figure 4. Only much later, at figure 5 is this point acknowledged, and profiles normalised to cloud top height. Given the importance of cloud/BL top in relation to aerosol profiles I think too much is made of the results from figures 3 and 4, when it could be stated up front that to properly interpret the profiles they need to be plotted against altitude normalised to BL top – maybe both true and normalised heights are needed to fully interpret them, but the issue needs acknowledging up front.

Original Figure 4 and analysis surrounding it has been updated, taking into consideration the reviewer's concern for variability in aerosol backscatter profiles associated with cloud top/boundary layer variability. The updated figure (see below) looks at backscatter for both the normalized heights (normalized to the median cloud top height observed within 60 minutes of low cloud formation) and the full profile up to 1.5 km for comparison. We find two interesting features in this updated analysis: 1. Aerosol backscatter is largest across the boundary layer/cloud layer, and decreases rapidly above the cloud top (seen also in the full profiles for e-h). This confirms our hypothesis that the primary aerosol concentrations emerge from near the surface and tend to be mixed within the rather shallow cloudy boundary layer. Thus, we find support for low cloud formation based on the profile of available aerosol 2. The backscatter across the soon to be cloudy boundary layer is similar, or slightly smaller, prior to cloud formation (blue) than shortly after the

cloud dissipated (black) for all seasons but winter (a). From this, we conclude it unlikely that advected plumes of increased aerosol concentration (increased to levels above those present shortly after the cloud dissipated) were responsible for supporting low cloud formation; or vice versa, that low aerosol concentration drove cloud dissipation. However, the median and interquartile spread in backscatter during winter is slightly larger where the low cloudy boundary layer would soon form. We have included such a discussion in the revised manuscript.



Line 201: 'most obvious is a reduction in backscatter in November just before cloud formation (Fig. 4d)' – this doesn't apply at all altitudes, only 200-600m. This might result from, say, subsidence causing BL depth to decrease – change is then not in situ, but movement of layers. It is also not clear that this reduction is relevant to the subsequent cloud formation since we are given no information as to what altitude that cloud/fog formed at.

It is perhaps also worth noting that there are only 6 cases for analysis in November, so a single strong case may dominate the statistics.

The original figure has been revised to not examine 4 representative months, but to examine the seasonal profiles and their associated variability; monthly cases have been combined into seasons to improve the representability of the statistics The updated figure (above) and analysis around it now addresses the reviewer's concern.

Line 204: 'It is interesting that the level where backscatter transitions to its quasi-constant value is at or above where low cloud formation (base < 400 m or surface fog) occurred'
a) this is exactly what we would expect for any scalar quantity with a surface source (eg water vapour in marine environment)...so reassuring rather than interesting? b) to properly assess this you need to plot against a normalised altitude – you know where cloud top was/will-be so don't need to approximate to 'at or above where low cloud occurred'.

The statement regarding "It is interesting…" has been removed from the revised manuscript. The entire text surrounding the original Figure 4 has been updated, which now includes normalized altitude, as well as the full altitude profiles up 1500 m.

Lines 206-209. "Consistency in aerosol backscatter structure from start to end of these clear periods seems to mimic the behaviour of a residual layer of relatively well-mixed aerosol trapped across the lowest few hundred meters of the atmosphere. This mixed layer may have been an artifact of the previous sub-cloud mixed layer prior to dissipation." a) it is not clear what altitude the authors refer to here – assuming they refer to the 'quasi constant' value from 2 lines up, then they refer to the layer above the BL/cloud, i.e. in the free troposphere. Here aerosol profiles depend mostly on advection and conditions upwind, perhaps far upwind. The reference to a previous subcloud layer then seems rather spurious. And again, you know where the cloud layer was (and will be) so you can pin point this, you don't need to speculate. Normalised altitudes would help again.
If the reference is really to within the BL, then this needs making clear.

The original text was confusing and we understand the reviewer's concern. The figure has been updated by normalizing to the formation cloud top height level (panels a-d) and also shown as a function of altitude (panels e-h). The text related to the previous cloud driven mixed layer has been removed in the revised manuscript.

Line 208: "since the transition to a quasi-constant value is occurring at or above cloud base" – physically we expect the transition to quasi-constant free-troposphere values at cloud top, the rather vague, and physically misleading, phrasing 'at or above cloud base' would be unnecessary if the profiles were assessed against a normalised altitude.

We agree with this statement and understand the ambiguity that was introduced in the original phrasing. The new figure with profiles normalized to cloud top height now shows the transition in aerosol backscatter does occur above cloud top, as the reviewer indicates.

The following statement "the data suggest that suface aerosol properties such as number concentration are likely often unrepresentative of aerosol properties at cloud level" I agree with, but not because the 'transition to a quasi-constant value is occurring at or above cloud base' but because there is a general decrease in backscatter with altitude in the lowest levels.

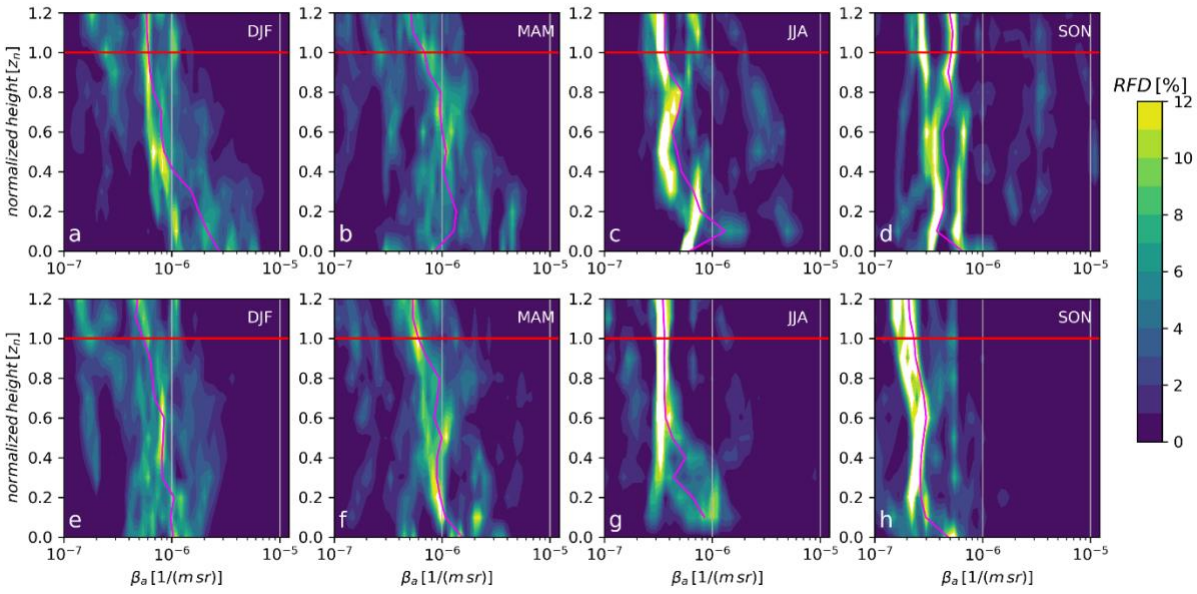
We agree with the reviewer on this statement. The original paragraph has been removed in the revised manuscript, however we do include the discussion regarding the decreasing aerosol backscatter with height across the lower troposphere in the revised Discussion section.

Line 224 & figure 5: only Feb-May are shown in figure 5 'because these months had the most frequent clear sky periods'. This is irritating, since it omits November, the one month in figure 4 which showed a behaviour distinct from the other months shown, and which might be explained by the normalised altitude used here. In general, given the very sparse data set, the limiting of data shown to specific months seems counter productive – better to use all of it all the time – combine months to reduce issues with poor stats in single months. Define season boundaries rather than using whole months to better group consistent seasonal behaviour. If you insist on using only a subset, then at least be consistent and use the same subset throughout.

The reviewer raises a valid point. For the backscatter profile figures, we have now combined monthly data into seasons. All seasons have now been included in the figures and the analysis text.

While the full 2D RFD in figure 5 is useful – it really highlights the variability and that this is clustered (on individual cases?) rather than uniform, it isn't easy to directly compare these plots with figures 3 and 4. The addition of median profiles would help.

The updated figure now clusters the monthly data into seasons (to improve stats for limited number of cases in a single month) and the median profiles normalized to cloud top height have also been included, as suggested (magenta lines – see updated figure below).



Line 233: the words 'and above (fig. 5a-d)' don't fit grammatically with any of the rest of this sentence.

This text has been removed from the revised manuscript.

Line 237: '...cutoff between aerosol and clear sky (Shupe, 2007)' – here 'clear sky' appears to be being used to mean something different than every other occurrence...a complete (?) lack of aerosol? I would rephrase or risk this being interpreted as just 'cloud free'.

The text has been updated to state "…a threshold value determined as pristine (Shupe, 2007)".

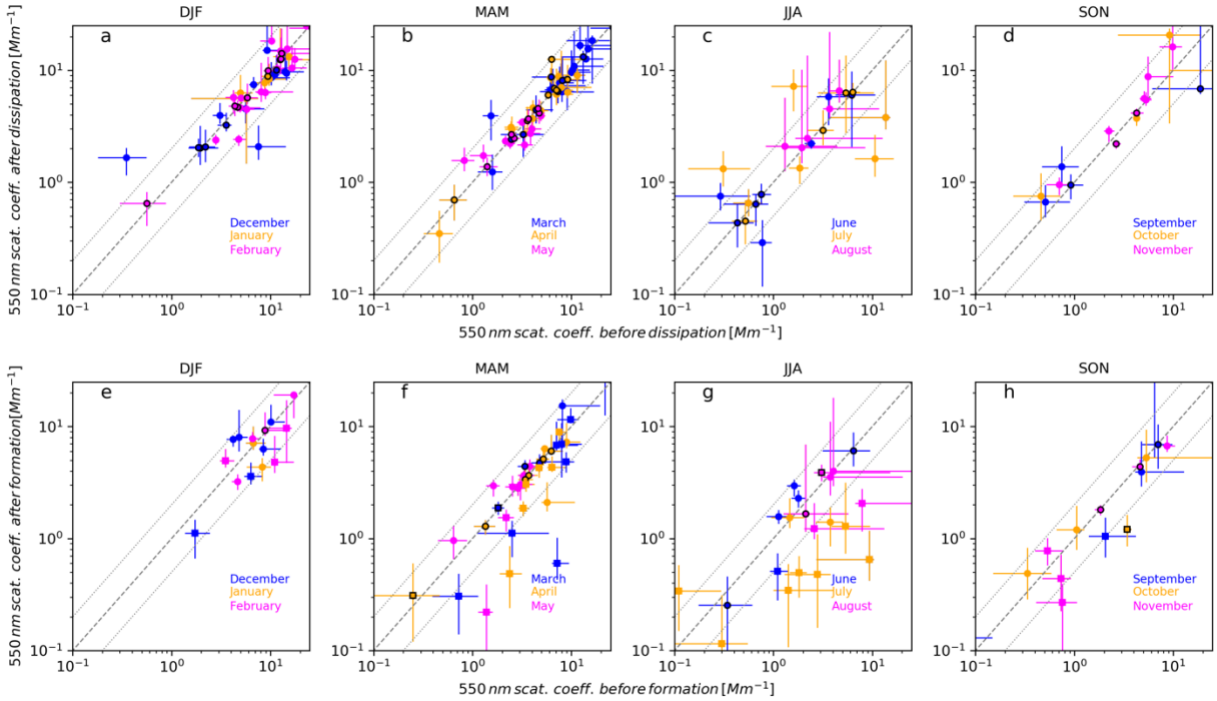
Line 241: "Being that aerosol backscatter near and above cloud top (zn=1) was at a minimum suggests that low aerosol concentrations near cloud top could have played a role in its dissipation" – only aerosol below cloud top are directly relevant to its properties, those above can't affect its microphysics. They can only play a role if entrained into cloud, but since the measurements are obtained after dissipation, aerosol above the former cloud top clearly were not entrained. This contradicts the statement on line 239 and is again contradicted (or at least...amended) on line 245.

This text has been removed from the revised manuscript because of the ambiguity introduced, as identified by the reviewer.

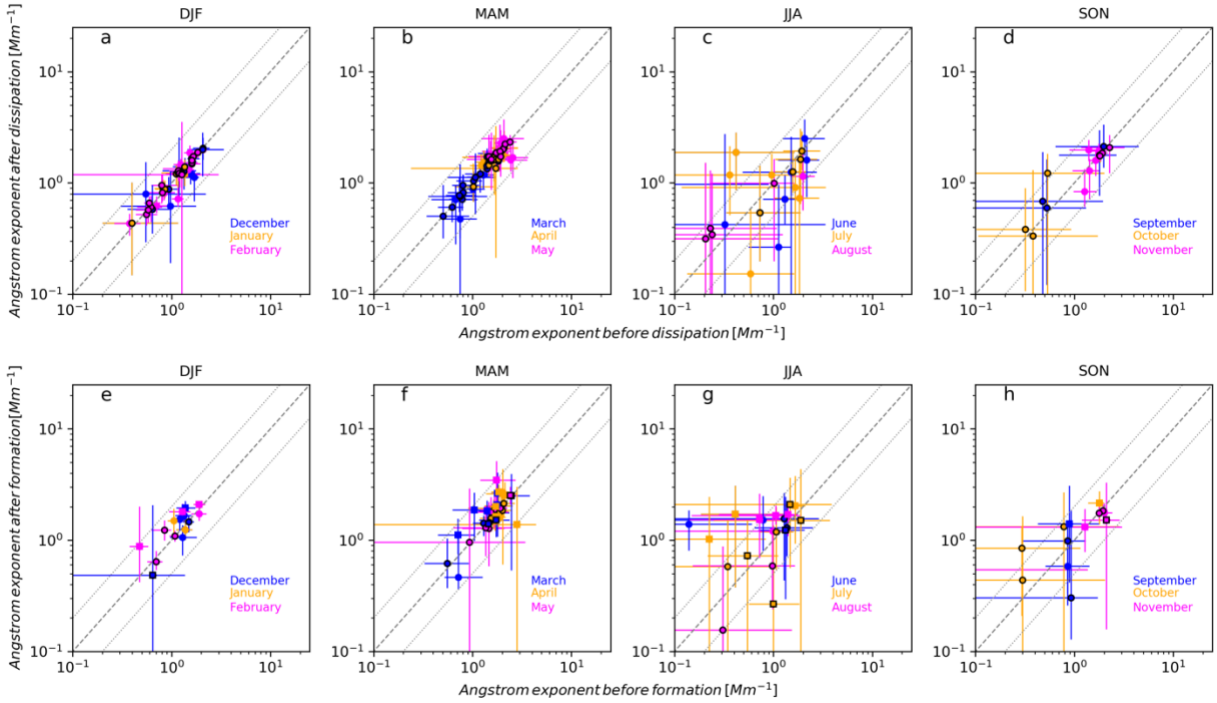
Line 266-274: The discussion of aerosol concentration at the surface needs more nuance.
In the case of low cloud - formation should not impact aerosol concentration at the surface - CCN lifted above LCL will nucleate a droplet, but if the drop is moved down again it will evaporate leaving the aerosol particle - number of particles is conserved.
Loss of particles requires:
i) coalescence of droplets - evap would then tend to consolidate all the original aerosol into a single large particle.
ii) scavenging of aerosol by droplets - evap as in (i)
iii) precip - loss of CCN & scavenged aerosol to surface.
All these are possible, but not discussed.

The reviewer raises a number of possible reasons that aerosol concentrations at the surface may decrease. However, as we are unable to test any of these possible processes, adding these possible mechanisms in the discussion to aid in determining the fate of CPC concentrations prior/post cloud lifecycle introduces additional speculation. With this figure and the analysis around it, we are able to show that CPC concentrations have a seasonal dependence around the dissipation/formation times. In particular, we find that aerosol is often as large, or even larger, after dissipation than before dissipation – which shows that aerosol are still present in terms of number. Secondly, we find that surface aerosol concentrations are considerably larger, often with median values twice as large, prior to low cloud formation, compared to after cloud formation during summer and into autumn. We find it likely that the larger concentrations of aerosol near the surface are likely contributing to efficiency of nucleation of a cloud drop.

Further, we have explored the optical properties (see figures below) of these near-surface aerosol prior to, and after, dissipation and formation events. We studied the 500 nm scattering coefficient measured from the nephelometer, as well as the Ångström exponent. Outside of summer, there distributions of scattering coefficient and Ångström exponent did not change systematically around cloud dissipation for formation. In summer, however, especially in July, an increase in scattering coefficient coinciding with a decrease in Ångström exponent was observed. The behavior suggests that the increased concentrations of particles observed during summer are not solely a response of very small particles formed from new particle formation events. Instead these particles appear to have sufficient size (and therefore mass) to provide a source of droplet nucleation.



Median and interquartile distribution of 550 nm scattering coefficient (Mm^{-1}) for 2 hours before and after cloud dissipation (a-d) and 2 hours before and after low cloud formation (circles) and fog formation (squares) (e-h). Events where the distributions ***were not*** significantly different at the 95% confidence level from a Wilcoxon rank sum significance test have a black marker edge color.



Same as above, but for the Ångström exponent.

In fog the CPC might undercount total particles, even when conserved, if droplets don't make it through the inlet into counter (quite probable).
Again, it would be useful here to distinguish between low (but elevated) cloud and fog.

The revised figure and analysis surrounding it now includes the separation between low cloud (base > 400 m) and fog formation episodes in panels e-h. We find that in winter, there is little connection between concentration changes around formation and whether a low cloud for fog layer forms. During summer, it is more apparent that the fog formation episodes are associated with a larger decrease in particles from pre- to post-formation. The decrease likely reflects the fact that some aerosol particles are activated to form cloud droplets in the fog (in connection with the change in relative humidity associated with temperature decreases, as shown in the meteorology analyses).

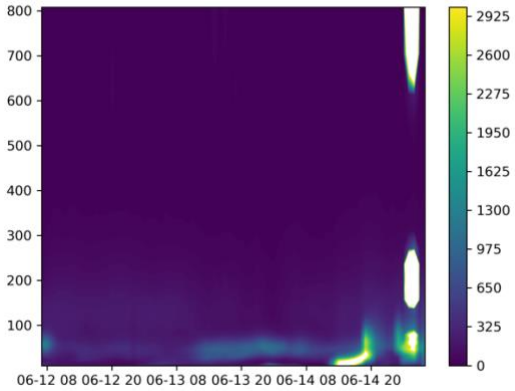
Line 290: "LWN is primarily proportional to cloud liquid..." – only for liquid water paths below the black body limit of ~50 g m^{-2}, above that there is little impact on LW radiation.

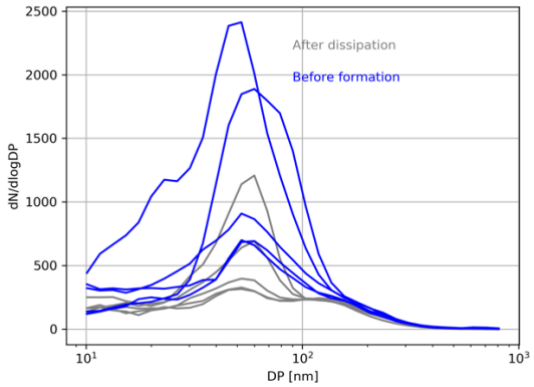
We completely agree with the point raised by the reviewer. To address this, we have changed the text to the following:

…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 and surface, …

Line 318: "These seasonal and sky condition differences in particle concentrations suggest different mechanisms are responsible for aerosol numbers near the surface" – this is interesting. Is this simply a result of having an exposed local surface during summer, which may be a strong source or aerosol, and a snow covered or frozen surface for the rest of the year?

It is intriguing to see different "modes" in aerosol number concentrations emerge for the seasons. The changing surface landscape probably plays a role in the absolute numbers, with the more exposed (and potentially drier) surface during summer contributing to near surface particle concentrations. However, we showed in Fig. 9 that the predominant wind directions during summer were often east-northeast. This would suggest that advection from the land to the south is not primary source but instead the open ocean contribution may be a contributing factor. Reviewer #4 has addressed the potential of new particle formation, a process found to be more frequent and contribute to the larger near-surface concentrations during summer (Freud et al., 2017). A dataset of SMPS size-resolved number concentrations is available for September 2007 through mid-June 2008 at Barrow. We explored the clear sky periods during the few months available in this time frame and were able to identify an example where it loosely appears that a new particle formation event was captured in the hours toward the cessation of a clear sky period, leading to cloud formation (see contour plot below, as well as hourly size-resolved number concentrations for the 5 hours after dissipation compare to 5 hours before formation for this particular June 2008 clear sky event. From these figures and supporting literature (e.g. Freud et al., 2017), we cannot dismiss new particle formation as contributing to the enhanced number concentrations during summer compared to winter.





Line 391: "least squares linear regression of the tendencies between the layers reveal a moderate agreement to the monthly cases" – 'with the monthly cases' or 'for' the monthly cases depending on your intended meaning.

We understand the confusion raised by the reviewer. This statement has been removed from the revised manuscript.

Line 418: "The statistical analyses presented fail to identify a definitive signal in aerosol vertical profiles indicating changes in aerosol partitioning are the primary cause for cloud dissipation." – poor phrasing, this is easily misread as meaning "changes in aerosol partitioning are the primary cause for cloud dissipation" rather than "fail to identify a definitive signal in aerosol vertical profiles that would support changes in aerosol partitioning being the primary cause for cloud dissipation"

We agree with the confusion of the statement; this line has been removed in the revised manuscript.

Line 480: "Here, a similar transformation process has been identified on the northern edge of NSA" – I'm not sure one has been identified, only inferred as a potential mechanism.

The discussion around the air mass transformation has been removed from the revised manuscript, primarily because we are unable to track an air mass in a Lagrangian framework to completely explore its transformation process.

Line 488: 'morphology' is not a verb!

This has been removed from the revised manuscript.

Line 505: 'increased pooling of aerosol particles near the surface' – I'm not sure that an 'increase' in pooling is demonstrated. And none is needed, concentrations rarely fall low enough for aerosol to limit cloud formation, so no pooling of aerosol is required to 'provide the ingredients' for cloud formation.

We appreciate the reviewer's concern. We agree that the ingredients for cloud formation are already present and no additional "pooling" is needed. We do feel that the change in particle

number concentrations between before formation and after formation is an important indicator, especially during fog formation, that a fraction of the particles near the surface that were present have likely now been activated into cloud droplets.