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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 #1

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This manuscript describes an analysis of clear-sky periods following cloud dissipation and prior to cloud formation over Utqiagvik, Alaska, with a focus on low clouds. The au- thors use a rather comprehensive set of ground-based measurements spanning over 5 years to draw insights on the processes contributing to cloud formation and dissipation. Different clear-sky period properties as a function of season are examined, from which the authors suggest differences in the impact of synoptic-scale forcing on cloud forma- tion and dissipation. The authors also find that a scarcity of aerosol particles is likely not the dominating cause for cloud dissipation, and postulate based on their analysis and the literature that cloud formations from late spring to early autumn largely initiates at or near the surface.

The manuscript is generally well written and I found the analysis description intriguing. I appreciate the amount of information that the authors were able to extract from the ground-based measurements and think that this manuscript provides a new analysis of an atmospheric phenomenon, which is often left without being properly examined, namely, clear sky periods. While I agree and/or find sense in most of the authors' interpretation and conclusions, I have a few concerns regarding the methodology and the analysis description, as well as a high number of rather minor comments, which I think the authors should address before this study can be accepted for publication in ACP. I, therefore, recommend major revisions.

We wish to thank the reviewer their detailed revision of our manuscript. As you will find below, we have intently considered each of the reviewer's criticisms, comments and suggestions. We have provided detailed responses to each of the specific comments below (in red).

Major comments: 1. Definition of a strict clear-sky period – I find the methodology rather robust. However, to my understanding, once the 2-h clear-sky threshold is met, intermittent clouds can be detected, as long as the total duration of cloudy periods does not exceed 4%, for example, in the case of a 10 h clear sky period, the last hour may contain the only 24 minutes of (broken) clouds. A similar example is provided in Fig. 1, in which I cannot agree with the authors' description in the text (l. 145-157) that the pe- riod between 10:30-12:45 UTC is strictly a clear sky period; that is, the KAZR, HSRL, and LW measurements all suggest the intermittent presence of a cloud layer (e.g., at ∼11:30 UTC), obviously a tenuous one, and hence, the weak LWN signature, but this is still definitely a cloudy period. Now, I understand that the data analysis here requires a binary definition of either a "clear" or "cloudy" period and that an addition of an in- termediate class period would likely introduce multiple inconsistencies. However, with the current methodology settings and constraints (duration of a clear/cloudy period, al- titude limit for cloud occurrence, etc.): a. The clear sky and cloudy period portioning results in a very high overlap with the "radiatively clear/cloudy" states coined by Stram- ler et al. (2011), which is essentially the only way to argue that in Fig. 1 there is a 9-h long clear-sky period rather than ∼7.0-7.5-h period. The authors should address this point here and other places in the text where it is applicable.

We understand the reviewer's concern, and we agree with their reasoning. It is true that intermittent cloudiness, by our definition of a "clear period", is allowed to emerge sporadically yet still be considered a clear period. We are looking for consistent cloudiness as observed by the vertically pointing remote sensors to identify the start and end points of a derived clear period. While we understand this is in no way strict, it was necessary in order to have any data points for our study. Further, we use the suite of instruments including the KAZR, HSRL and ceilometer to identify any instances, flag these times, and remove them from further analysis, in the time periods of analysis before or after a dissiapation/formation event. However, we still retain the original start and end points of the event because the general criteria for a clear sky period have been satisfied.

We have revised the wording around the duration of the clear sky period shown in Fig. 1, as suggested by the reviewer.

b. Clear sky periods can actually be cloudy, so I think that the authors should omit the use of the "strict" clear sky period definition throughout the text, which can become rather subjective, among other reasons, because of the multiple variable thresholds in this study (e.g., one could argue that only the periods where the thresholds mentioned in l. 236-238 correspond with a "strict" clear sky definition). I recommend the authors to consider terming the clear sky periods in conformance with their effective partitioning of the dataset, for example, I would suggest using the term "prolonged clear sky periods" (corresponding to the duration requirement while remaining objective by not introducing subjective criteria), which precede/follow "persistent cloud occurrence periods".

As the reviewer suggests, we have removed the description of clear sky periods as "strict". We agree that the original wording was confusing.

2. Inconsistent duration thresholds - why were 2 hours used for the analysis in fig. 6 (CPC counts), unlike the rest of the data analysis? How much would the results change with 1 -hour windows? Why were 4-hour windows used in the temperature trend analysis (sec. 4.3.2) instead of consistently working with 1-hour windows? Also, could the 1-hour window allow some separation of fog events (necessarily positive Td depression) from other low cloud events (potentially all depression values possible)? For both the CPC and temperature trend analyses, could there be an influence of intermittent cloudy periods just before (after) cloud dissipation (cloud formation) being classified as part of a clear sky period? How different do the scatter plots look using 1-hour windows instead of the utilized 2/4 hour duration windows?

In the revised analysis and manuscript, we have changed the results to look at either 1-hr or 2-hr time windows around dissipation/formation events for all analyses except for the large-scale layer thickness tendencies (4 hr). We kept the 1 hr period for the HSRL analysis because this analysis was focused on characterizing the vertical distribution of aerosol backscatter very near in time to the actual cloud dissipation or formation event. The intention was to identify if sharp gradients in backscatter (interstitial aerosol layers) were present and could be connected as an important mechansims contributing to cloud dissipation of formation. The HSRL also provided the highest frequency output (30 s) of the datasets analyzed, and we did not want the characteristics of longer duration clear sky period to emerge in the frequency distribution profiles. All other datasets had a 1-min and in order to compute meaningful tendencies, we extended the analysis period around a cloud lifecycle event by another hour to get a similar number of temporal data points as the HSRL. We do not have a legitimate motivation as to why we chose 4-hr windows for temperature trends; however in the revised manuscript, we have used 2-hr windows prior to cloud formation. Tendencies for layer thicknesses use 4-hr windows because we now rely on 1-hr ERA5 reanalysis to compute the tendencies (see specific comments below).

3. Synoptic forcing methodology and analysis: a. I understand that quasi-geostrophic flow occurs further away from the surface, but if largely low clouds are examined here (cloud base up to 400 m AGL in the main data subset; 3 km for the full dataset), why aren't tendencies in a near-surface layer thickness (e.g., 850-950 hPa) examined here (using a similar or different methodology), being more representative and consistent with the presented analysis thus far? It is not obvious to me how much analyzing such a low atmospheric layer could impact the results and discussion throughout this section, for example: - The conclusion that cloud dissipation events are impacted by relatively homogeneous thermal advection across the lower to mid-troposphere (l. 392-393). - "we identify that the height level where cloud formation events occur may be influenced by a weaker synoptic setting from December through May" (l. 413-414). - "Larger-scale differential advection is almost always ongoing prior to cloud dissipation, and as such it is assumed that different air mass origin and thermodynamic properties are likely to go in unison with changing aerosol properties" (l. 427-428).

The choice of these atmospheric layers are motivated by the fact that we are interested in understanding the background state of the atmosphere. Following synoptic forecasting guidelines (https://www.weather.gov/source/zhu/ZHU_Training_Page/Miscellaneous/Heights_Thicknesses/thickness_temperature.htm), the 500-700hPa/700-850 hPa layers are frequently analyzed in terms of the their thickness tendencies to characterize differential advection. Understanding how the thickness of these 2 layers covaries was the primary purpose of this analysis. As for the detailed sentences highlighted: these have all been removed in the revised manuscript because of the updated figure and analysis surrounding it.

b. Point measurement nature of the ground-based data influencing the interpretation - I understand that the results indicate a consistent/inconsistent wind regime in different seasons (e.g., l. 356- 360). Could the consistency of the low-level wind direction examined here be the result of a strong micro-meteorology, e.g., prevalent sea-breeze over Barrow (during sunlit periods; hence, the narrowest distribution during summer when SZA is lowest), which masks synoptic forcing, which could still have a significant influence on a mesoscale? By the same token, high variability during winter is influenced by the synoptic-scale flow (e.g., l. 353-354), but that signature could be enhanced (relative to other seasons) by weak/lack of micro-meteorological sources (e.g., during dark periods). This is an additional degree of freedom in the data that the authors need to consider (e.g., using reanalysis or nearby surface stations) in order to support their conclusions in l. 500- 502, 506-508.

The reviewer raises a number of valid arguments, and the simple answer is unfortunately we do not have the data sets to address them. We agree with the reviewer that reanalysis or a spatial analysis may provide some insights to these questions. However, this alone would be easily enough material to comprise a separate study. We feel that the results and conclusions drawn in this paper about the mechanisms contributing to cloud lifecycle changes at Utqiagvik provide a baseline and a framework for further synoptic analyses. Our main conclusion is that finding the atmosphere to be in a complete steady state is rare, and therefore idealized modeling studies only exploring the changes to cloud from aerosol microphysics is not relevant on the North Slope of Alaska.

4. Given the relatively small effective dataset, statistical significance tests could have a large impact on the discussion and interpretation of the results. The authors should perform such tests and refer to them throughout the discussions for which they are relevant (e.g., l. 262-264, l. 360-362, l. 391-392)

The reviewer raises a very valid concern, and we appreciate the suggestion. To examine the distributions of number concentrations from the CPC prior and post cloud dissipation/formation events, we have performed a Wilcoxon rand-sum significance test. This test is used to determine whether the null hypothesis that the median values amongst two populations can be rejected.

For the original dew point depression and temperature trends analysis, a Pearson correlation coefficient and associated p-value for the relationship is presented. These include corrrelations for all cases, low/fog and fog only cases.

For the geopotential height tendencies, 4 hr trends from consecutive 4 hr periods within a month of in geopotential height between the two atmospheric layers during the 5 year period have been computed from ERA5 reanalysis. From these monthly trends, monthly mean and standard deviations in the 4-hr trends are used to identify when 4-hr trends prior to (post) cloud dissipation (formation) events were within, or exceeded, +/- one standard deviation of the seasonal mean. Including the stasistics of seasonal variability highlights our original finding/conclusion that winter and spring cloud dissipation/formation events occur more frequently for larger forcing (geopential height tendencies) than during summer and autumn. During summer, nearly all the low cloud/fog formation events occur in coincidence with small geopotential tendencies.

Minor comments: - There is an occasional change of tense throughout the manuscript (e.g., l. 16-20, 196-215, 311-319, 387-398). I recommend the authors to be more consistent from this aspect, as I think that it improves the manuscript's readability.

- "Barrow" should be replaced with "Utqiagvik" throughout the text (except for in the abstract and introduction).

We have replaced Barrow with Utqiagvik, as suggested.

- l. 24 – Because the essence of this first sentence is elaborated below, I suggest removing the first reference or adding a few more references (e.g., Curry et al., 1996, https://journals.ametsoc.org/jcli/article/9/8/1731/36313), as Herman and Goody (1974) only discussed summertime clouds, in which SW radiation plays a role.

Changed as suggested.

- l. 27 - add "water" before "particles"

We have changed the text to "…water and ice particles…"

- l. 96 - suggest modifying "the signal becomes" to "the signal typically becomes"

Changed as suggested.

- l. 101 - I do not think that this is necessarily true over the Arctic. There are numerous examples of cases in which the droplet size and/or concentrations are too small to be detected by ground-based Ka-Band radars such as KAZR (e.g., first hour in Shupe, 2011, fig. 1, where the radar echoes are below cloud base). I agree that voxel-wise KAZR is indeed capable of detecting (in its high-sensitivity mode) most hydrometeor echoes, but many tenuous liquid-bearing clouds (which are common in the Arctic) can remain undetected by radar. I suggest rewording this sentence to address this general misconception.

Following the reviewer's concern, we have updated the sentence to the following:

The millimeter wavelength (35 GHz) provides high sensitivity and signal to noise ratio allowing the radar to observe cloud droplets, although some may be missed (de Boer et al., 2009).

- l. 113-115 - Note that Long and Turner (2008) only analyzed the downwelling LW and only during clear-sky periods, in which the downwelling fluxes are relatively lower, and found that the 4 Wmˆ-2 value holds for only 2/3 of the NSA cases. The LW flux uncertainties are likely larger and contain a flux percentage component, as also suggested by the ARM handbook for these pyrgeometers (see Table 6 in https://www.arm.gov/publications/tech_reports/handbooks/sirs_handbook.pdf). I rec- ommend the authors to update this discussion accordingly.

The uncertainty estimate has been updated as listed in the SIRS handbook provided by the reviewer, thank you.

- Fig 1. – The title for panel a says "MMCR reflectivity" instead of "KAZR reflectivity".

Changed as suggested.

- l. 147 - following major comment #1, suggest changing to "nearly 7 hours" or elaborate accordingly.

Updated as suggested by the reviewer.

- l. 191-194 – This sentence is slightly confusing. I suggest rewording and or breaking it in two.

This section has been updated during the revision to better reflect the seasonal variability and how its magnitude varies with season.

- l. 165-166 - I suspect that these are 24 individual periods per month? Or is it a MAM seasonal mean? In which year?

The reviewer is correct. Figure 2 identifies the total number of clear periods during a particular month over the 5 year period. The text has been rephrased to clarify this point.

- l. 187-189 - This is hard to interpret in the logarithmic scale used in Fig. 3.

Please see response to the next comment below.

- Fig. 3 - Because the aerosol beta signal is largely concentrated on a single order of magnitude, I think that the logarithmic scale on the x-axis makes the figure more difficult to interpret, especially with regards to the shaded sigma, which may become misleading depending on the mean value. I suggest setting the x-axis scale to linear and/or plotting profiles of the SD absolute value and/or fraction relative to the mean value.

Considering the reviewer's suggestion here, as well as the in the comment prior, we have changed the x-axis to linear scale.

- l. 189-190 - I cannot agree that this is the case in some months, e.g., DEC, FEB, MAR.

In the revised manuscript, the statements that caused the reviewer to disagree have been removed.

- l. 190-191 - Difficult to say that for panel f

This statement is no longer included in the manuscript.

- l. 192 - "depth of the enhanced" –> "depth of an enhanced" - l. 204 - suggest removing "It is interesting that"

These changes have been made as suggested.

- l. 212 - "therefore impossible" –> "therefore it is impossible"

This statement is no longer included in the manuscript.

- l. 213-215 - To my understanding of the text description, if the transition phase (sloped part of the profile) occurs at or above cloud base levels, then this state- ment doesn't hold, because there is a certain depth (either just at cloud base or above) where the aerosol profile appears to be more similar to the aerosol pro- file below, as also suggested from Fig. 1, so based on the HSRL beta mea- surements alone, I could argue that the aerosols in the cloud layer are very simi- lar to those at the surface and that the surface is actually representative. I agree that the surface aerosol properties are likely often unrepresentative of aerosol prop- erties at the cloud level, but only because of previous dedicated aerosol stud- ies (some with in-situ measurements) and because surface or near-surface inver- sions/stable layers are so common over the Arctic (e.g., Tjernström and Graversen, 2009, https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.380).

We agree with the reviewer, in particular the wording in the original manuscript was confusing. This has been addressed by updating the figure which includes both normalized height (by the low cloud formation top height) and the full profiles up to 1.5 km. The lines the reviewer is referring to are not included in the revised manuscript. Instead, the new figure with profiles normalized to the top height of following low cloud layer formation (a-d) now show the aerosol backscatter 1) decreased up to the cloud top normalization height; and 2) from spring through summer the new figure shows that aerosol did not increase compared to the magnitude in the same height layers after cloud dissipation. From this analysis, we conclude that interstitial aerosol being advected across the lower troposphere were unlikely to be processes supporting low level cloud formation. The text has been revised to reflect these changes.

- l. 217-219 - That is a nice and significant observation. Do the authors think that this conclusion holds for other Arctic regions?

We agree in that the similarity in HSRL backscatter suggests that aerosols remained present and they did not drop to a concentration critical for sustaining cloud. This has been emphasized in the revised manuscript surrounding the updated figures. We believe that limiting aerosol concentration as a mechanisms for dissipating or inhibiting cloud formation are not present on the NSA, and likely only relevant over the central Arctic sea ice where a lack of particles sources exist.

- l. 221-222 - what is the time range before/following dissipation/formation that the authors use to determine whether the data corresponds with the 2 km threshold?

We use the 60 min window prior/post dissipation/formation to come to the mean cloud top height. This is now included in the revised manuscript.

- l. 226-227 - the lidar signal could be fully attenuated by cloud, but is not necessarily fully attenuated by cloud. Perhaps the authors can simply say that using a subset of cases without full lidar attenuation before dissipation/after formation would result in very few samples to analyze (number of examined samples is already rather low), also because of the data filtering (see major comment #1a).

The reviewer is absolutely correct. However, we have removed this discussion from the revised manuscript.

- l. 227-230 - Indeed, precipitating hydrometeors typically dominated the aerosol signal, but occasionally there are cases in which the ice number concentration is so low, that it is barely detectable in lidar measurements. I recommend the authors to add "typically" or “largely” to the text. Also, please change "drizzle droplets" to "drizzle drops".

Again, the reviewer is absolutely correct. We have removed this discussion from the revised manuscript.

- l. 241-242 - I am not sure this can be said without forward calculations of aerosol properties (given that the backscatter is proportional to the surface area, which is pro- portional to particle size in addition to concentration), which requires some information not available with this dataset. I would be hesitant to postulating that.

We agree with the reviewer’s concern. In connection with Reviewer 2’s comment stressing the importance of entrainment and the inability to assume entrainment is ongoing due to the lack of a cloud layer presence, we have removed this statement from the revised manuscript.

- l. 242-244 - such findings were first reported up to a few decades ago and should be cited here as well, e.g., Curry et al. (1996, https://journals.ametsoc.org/jcli/article/9/8/1731/36313) and references therein, Jiang et al., (2001, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2000JD900303), Fridlind et al. (2012, https://journals.ametsoc.org/jas/article/69/1/365/27245).

We appreciate the additional references connected to importance of cloud top entrainment as a scalar source to the cloud layer. In lieu of the removal of role of free atmosphere aerosol source in the revised manuscript, the inclusion of these additional references is no longer relevant to the revised manuscript.

- l. 244-246 - I cannot tell whether this sentence agrees with the data depiction in Fig. 5 because the cloud base height was not considered in the height normalization.

This statement is no longer included in the revised manuscript.

- l. 247 - by flatter do the authors mean less variable? - l. 248 - define RFD

Correct, we referred to a reduction in variability; this has been updated as suggested. The definition of RFD has been included earlier in the manuscript, as suggested.

- l. 248-249 – Not sure I understand the authors' intention here. Perhaps "typically agrees in magnitude with the profiles prior to cloud dissipation"?

This statement has been removed from the revised manuscript.

- Fig 5 and the associated discussion - Fig. 5 is hard to interpret because: a. There is no normalized height for cloud base. b. The small dataset combined with the inter- polated shading can be misleading. While I agree with the general conclusions of this discussion, currently it is rather difficult to evaluate and follow the different arguments. I recommend using larger RFD bin widths and adding a normalized height for cloud base (e.g., at 0.5).

We have addressed this by combined monthly data into seasons. This has increased the representativeness of the frequency distributions by including more data in each subpanel. We have also included the seasonal median profiles to help distinguish how a reduced number of cases (for example in JJA) influences the distributions. We decided to not normalize by the cloud base for two reasons: 1) We have extended the analysis original Fig. 4 to look at the profiles of seasonal median/25th-75th profiles of all low cloud and fog cases, which in effect focuses the analysis truly on the lowest 300-500 m of the atmosphere; and 2) because cloud base heights, as discussed with the number of fog and low cloud cases in section 4.1 limited the number of valid cloud bases to normalize the height grid. Furthermore, the depth of the layer between observed cloud base and cloud top is relatively shallow in these low Arctic clouds. As such the normalization would become dominated by specific cases (i.e., the RFD would be biases by individual cases).

- l. 256-260 –The authors analyze here specific periods, which makes me wonder whether these seasonal patterns agree with the published literature of aerosol/CCN/CN seasonal variability over the NSA (e.g., Quinn et al., 2002, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2001JD001248; Lubin et al., 2020, https://journals.ametsoc.org/bams/article/101/7/E1069/345559), or are these seasonal signals muted in the bulk statistics reported in the literature?

Thank you to the reviewer for pointing us towards these relevant studies. We have included their citations as they agree with the general seasonality in number concentrations that we observed.

- l. 257-260 - This sentence reads awkwardly - suggest rewording.

During the revision of this section, this original sentence has been removed.

- l. 262-264 - Unlike the formation plots in panels e-h, I do not see any major differ- ence between these panels (a-b, c-d). I think that the authors need some statistical significance tests here to convince a reader (see major comment #4).

We agree with the reviewer that differences were sometimes difficult to assert in the original plot. While in the process of adding significance testing, we determined the original figure contained a bug. Instead of the bars showing the 25-75th percentile range, the 25th and 75th percentiles were being added/subtracted to the median values, essentially being treated as error bars. We have fixed this figure to properly show the interquartile ranges, and this helps to better distinguish differences amongst the distributions.

Further, we have included the 2:1 and 0.5:1 lines (in addition to the 1:1 line) to better identify visually how the median distributions have changed depending up time period prior to, or post, cloud lifecycle change. A Wilcoxon rank-sum significance test was performed to test whether the distributions that the medians are computed from were significantly different. Since the majority of the distributions were able to reject the null hypothesis that the distributions were equal at the 5% level, only cases where statistical significance was less than 5% were highlighted with a black marker edge color.

- l. 266 - define CPC

We have now defined CPC (as Condensation Particle Counter) in Section 2, Instruments.

- l. 276 - I do not think that the analysis failed to identify drastic signal changes, it simply did not identify significant changes, which I think is a nice observation on its own. I suggest rephrasing this sentence and remove "Therefore" in the following sentence.

We have followed the reviewer's suggestion and rephrased the opening paragraph of Section 4.3 as follows:

The previous analyses did not identify major changes in the vertical distribution or surface concentration of aerosols surrounding cloud dissipation and formation events. Increased surface particle concentrations prior to low cloud formation during summer were the most significant finding. The results imply that cloud-free periods may not be driven by significant changes in aerosol presence alone, consistent with conclusions drawn from an Arctic dissipation case examined in detail (Kalesse et al., 2016).

- l. 287 - "sub-cloud mixing driven by cloud-top turbulence" - suggest changing to "cloud-top radiative cooling" or "cloud-induced turbulence".

- l. 289-291 - the effective cloud temperature is rather important as well, i.e., the LWN is primarily proportional to the effective temperature differences between the cloud (cloud emissivity profile considered) and the surface, e.g., compare the LWN histogram in Stramler et al., (2011, https://journals.ametsoc.org/jcli/article/24/6/1747/32737) to that in Silber et al., 2019 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JD029471).

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, …

- l. 291-292 - Generally speaking, utilization of the equivalent potential temperature variable to examine static stability is only valid in moist processes (see Ch. 4 in Emanuel, 1994), e.g., in the case of a fog cloud extending from the surface up to 950 hPa. Otherwise, the virtual potential temperature or liquid water potential temperature should be used. Alternatively, the authors may use the equivalent potential temperature only in liquid-containing heights. I suspect that addressing this comment as suggested above would not significantly change the results in this section, and therefore, consider this a minor comment.

We agree that equivalent potential temperature is valid in a moist environment. The Arctic is frequently very high in relative humidity, even though specific humidity magnitudes can be relatively low (e.g. Tjernström et al. 2012, ACP, https://doi.org/10.5194/acp-12-6863-2012). Further since the absolute humidity if relatively low, it typically has little influence on the equivalent potential temperature calculation. In this low absolute humidity environment, even if the equivalent potential temperature at 950 hPa changes by 1-2 degrees, this will not impact the LTS stability metric that we have computed. As the review mentions, the results would not significantly change, and therefore we have not changed the analysis to potential temperature.

- l. 302-310 - That is a nice discussion. The authors should also explain the apparent correlation between LWN and LTS (e.g., stronger stratification may indicate higher downwelling LW due to relatively higher temperature at 950 hPa or so, strongly de- pending of course on sufficiently high q_v at the 950 hPa level or so).

We are confused by the reviewer's request to explain the correlation between LWN and LTS. There are not many instances in the RFDs where the LTS is strongly positive and the LWN is relatively small, as suggested by the reviewer. In Sedlar et al. (2020), this mode in the RFD is discussed as being a consequence of warm, moist advection that becomes "trapped" in a vertical sense near the surface (e.g. Tjernström et al., 2015, GRL). Ultimately, we decided that this discussion does not add any scientific explanation to the major results of the figure, which are meant to show the separation between the "radiatively opaque" and "radiatively clear" modes in the LWN-LTS parameter space.

- l. 309 - suggest "warmer temperature" –> "higher temperature"

Changed as suggested.

- Fig. 7 & 10 - Please add the season to each panel, as already shown in other figures. It helps to follow the text.

To keep consistency throughout, seasonal titles have been included in the title of each panel, as suggested by the reviewer.

- Fig. 8 - suggest using constant y-axis limits - evaluation of the figure can be confusing at the moment (especially with regards to panel b).

Figure 8 has been updated in the revised manuscript, including using constant x- and y-axis limits, as suggested.

- l. 325 - suggest "strongly" –> "largely"

Changed as suggested.

- l. 331 - "Dew point depressions are then computed" - suggest rewording this part of the sentence.

This section has been revised, and this statement is no longer included.

- l. 358-362 - "insert/s" –> "inset/s"

- l. 367-369 - Isn't the moisture content a second-order term relative to mean temperature? If so, then I suggest focusing on the temperature or providing some observational evidence (e.g., via references), about temperature and moisture advection occurring commensurately.

In principle, the reviewer is correct. However, the modest specific humidity magnitudes typically found over the polar regions mean that only small changes to the absolute moisture can have a relatively large impact in the moist static energy through an atmospheric layer (e.g., Naakka et al., 2019, Int. J. Climatol. doi:10.1002/joc.5988)

- l. 372 - does "quasi-geostrophy" refers to "quasi-geostrophic flow"? Also, please provide a chapter or page number for Holton, 1992.

- l. 382-385 - I do not find this argument convincing (e.g., in panels b, f), and the current version of Fig. 10 where x- and y-axis limits are inconsistent between one-another and between panels depicts a misleading picture. Also, given that one should expect larger tendencies at higher altitudes (lower pressure levels), then what scale of tendencies in each layer would be considered small?

We thank the reviewer for suggesting to hold the x- and y-axis limits to the same bounds for each subpanel. Further, in light of Reviewer #2's concern with the geopotential height tendencies being computed from 12-hour soundings (the time scale is too long), we have used hourly ERA5 reanalysis profiles of geopotential height. From these data, we compute 4-hr tendencies (going back 4 hours from a dissipation or formation event) in order to increase the temporal resolution and focus on atmospheric dynamics in a time window closer to the actual dissipation/formation event. Because we are now using reanalysis data, in order to place the observed layer tendencies into context, we have computed consecutive 4-hr tendencies for all months within a season for the full five year period. This provides a measure of the variability of layer tendencies (a mean and +/- one standard deviation) with which we now compare the tendencies around a dissipation/formation event against.

- l. 387-389 - This sentence is rather confusing. I suggest rewording. Also, I think that labeling the figure quadrants might really help a reader follow this section and quickly understand that quadrant x represents thermodynamic structure change y, and so on.

The original wording was likely overcomplicated, as the reviewer indicates; in the revised manuscript, we have removed this statement. This section of the manuscript has been completely revised, including using ERA5 reanalysis to compute layer geopotential thickness tendencies computed over a 4-hr window prior to cloud dissipation or formation events. Application of reanalysis thickness tendencies provided more robust results than the nominal 12-hr radiosoundings prior to cloud dissipation and formation events. The better temporal resolution permits the focus of the synoptic activity, or lack thereof, on the actual cloud lifecycle event. From this analysis, we find a robust consistency across all seasons that represented a barotropic-type forcing amongst the 500-700 and 700-850 hPa layers; when plotting the two layer tendencies as a scatter plot, this barotropic signature emerges with a positively sloped relationship.

- l. 391-392 - with such a low number of samples, I would not consider r=0.44 or r=0.5 for that matter to represent moderate correlation (explaining not more than a quarter of the thickness co-variability). In any case, statistical significance for each month should be examined and provided here as well (see major comment #4).

We agree with the reviewer. However, the lack of correlation coefficients in the linear regressions are actually a consistent signature of one of the main conclusions of this analysis. Correlations were generally smallest during summer, especially prior to fog formation events. We assert that a lack of synoptic activity is an important mechanism that allowed the near surface to adjust thermodynamically to the large net longwave radiation deficit, promoting saturation and fog formation. The small correlations are representative of the layer tendencies clustering around zero (the origin in Fig. 10), with no real relationship in the sign and magnitude of the layer tendencies. Further, to account for variability, we have computed the seasonal mean and one standard deviation (1-sigma) of the thickness tendencies for each layer (values exceeding the blue dashed lines in Fig. 10). This variability allows us to determine that winter and spring dissipation/formation events more often were associated with thickness tendencies exceeding the seasonal 4-hr climatology, indicating significantly large synoptic forcing was more common in winter/spring than summer.

- l. 395-398 and throughout the discussion concerning Fig. 10 - the authors should consistently use (in the text and figure) height or thickness; to my understanding, the authors only refer to thickness as a reference height is not provided.

We have revised the manuscript, including the figure caption, to consistently refer to geopotential thickness tendencies rather than height tendencies.

- l. 401-403 - Does this nice observation also agrees with the fact that frontal clouds are often observed during winter, as suggested by the wind direction analysis?

This statement has been removed in the revised manuscript, but we use the consistent results from the thickness tendencies together with wind direction changes during winter to argue that winter dissipation and formation events are influenced by larger synoptic activity than summer.

- l. 413-414 – I recommend replacing "weaker" with "shallower".

Shallower is not the context we were trying to describe, as this could easily be interpreted as a measure of a disturbance's vertical scale. Regardless, this statement is no longer included in the revised manuscript.

- l. 428 - suggest changing "is almost always ongoing" to "often occurs"

This statement has been removed from the revised manuscript.

- l. 431-432 - what do the authors mean by "aerosol vertical partitioning"?

Aerosol vertical partitioning refers to the general gradient structure of aerosol across the lower troposphere, being largest near the surface and generally decreasing with height. The vertical partitioning also describes any interstitial layers with enhanced or diminished aerosol backscatter signatures.

Following the reviewer's questioning of the terminology, we have replaced "aerosol vertical partitioning" with "aerosol vertical structure" throughout the manuscript.

- l. 452-453 - suggest removing "of the lower atmosphere".

This statement has been removed from the revised manuscript.

- l. 483 - redundant "the"

We have removed the discussion surrounding the air mass transformation process from the revised manuscript. Instead we focus on our main results that show a seasonal difference in synoptic forcing, near-surface cooling and transition towards saturation, and an enhancement in the particle concentration near the surface during summer – all results which suggest the summer, and to some extent spring, fog formation mechanism differ from that of winter.

- l. 483-484 - given the implemented methodology, the authors should explicitly state in this sentence that they refer to low-clouds (obviously, not necessarily fog).

See the response directly above.

- l. 514-515 - again, this is often the case, not always. As suggested above, the authors can say that when omitting the fully attenuated cases, the data subset becomes too small to be meaningful.

We do agree with the reviewer, but for the sake of clarity and the overall length of the manuscript, we have removed this discussion in its entirety in the revised manuscript.

- l. 519-521 - I would be hesitant to claim that the dominant Arctic cloud type following clear sky periods are low clouds, even though it could be tentatively suggested by the liquid/mixed cloud RFDs presented in Shupe (2011). My main concern here is about the subjectiveness of a clear-sky definition based on the methodology (see major com- ment #1b). To correspond with the methodology (e.g., 2-h clear sky and cloud occur- rence period thresholds) I could agree with "formation of persistent low-level clouds or fog, which have been shown in this study to be the dominant Arctic cloud type following prolonged clear-sky periods."

We respect the reviewer's concern regarding the general representativeness of our results. We have changed the text as the reviewer has suggested.

- l. 739 - Fig. 6 caption - should be (e-h)

Revised as suggested.

- l. 744 - RFD should be first defined in the caption of Fig. 5 where it is first mentioned. - l. 759 - insert –> inset

Both points have been revised as suggested.

- l. 764-766 - "e-g" –> "e-h"

Revised as suggested.