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Atmospheric Chemistry and Physics Discussions

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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 authors 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 formation 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.

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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.

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 period 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 intermediate class period would likely introduce multiple inconsistencies. However, with the current methodology settings and constraints (duration of a clear/cloudy period, altitude 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 Stramler 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. 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”.

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?

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

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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). 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.

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)

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).

- 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.

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

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

- 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.

- 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 recommend the authors to update this discussion accordingly.

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

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

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

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

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

- 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.

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

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

- l. 192 - "depth of the enhanced" –> "depth of an enhanced"

- l. 204 - suggest removing "It is interesting that"

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

- 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 statement 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 profile below, as also suggested from Fig. 1, so based on the HSRL beta measurements alone, I could argue that the aerosols in the cloud layer are very similar 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 properties at the cloud level, but only because of previous dedicated aerosol studies (some with in-situ measurements) and because surface or near-surface inversions/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).

- l. 217-219 - That is a nice and significant observation. Do the authors think that this

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conclusion holds for other Arctic regions?

- 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?

- 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).

- 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".

- 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 proportional to particle size in addition to concentration), which requires some information not available with this dataset. I would be hesitant to postulating that.

- 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).

- 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.

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

- l. 248 - define RFD

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

- 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 interpolated 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).

- 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?

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

- l. 262-264 - Unlike the formation plots in panels e-h, I do not see any major difference 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).

- l. 266 - define CPC

- 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.

- 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).

- 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.

- 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 depending of course on sufficiently high q_v at the 950 hPa level or so).

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

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

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

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

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

- 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.

- 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?

- 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.

- 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).

- 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.

- 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?

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

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

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

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

- l. 483 - redundant "the"

- 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).

- 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.

- 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 comment #1b). To correspond with the methodology (e.g., 2-h clear sky and cloud occurrence 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."

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

- l. 744 - RFD should be first defined in the caption of Fig. 5 where it is first mentioned.

- l. 759 - insert –> inset

- l. 764-766 - "e-g" –> "e-h"

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