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

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

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

General comments:

- I would suggest that the authors are a bit more careful when they use the term "the Arctic" or when they refer to certain characteristics of "the Arctic". The Arctic is not a homogeneous region where clouds, meteorology and surface properties are the same. Many of the features that the authors mention, in particular in the introduction, may not be true for the lower-latitude parts of the Arctic and/or land areas. For example, are clouds ubiquitous over the whole Arctic during the whole year? Does the longwave radiation dominate the radiative energy budget everywhere and during the whole year? Under cloud-free conditions, does effective infrared cooling from the surface cause extremely cold temperatures everywhere? I am thinking for example of Siberia where you in the summertime can have very different conditions compared to over the Arctic Ocean.
- Related to the previous comment, how representative is Barrow as a station for "the Arctic" and the type of cloud formation/dissipation events that you study? I think that the idea that aerosols control cloud formation/dissipation has mainly (only?) been presented for high (>80ºN) Arctic clouds, i.e. in pristine environments where (accumulation mode) aerosol number concentrations are extremely low. Utqiagvik (or Barrow) has rather high (accumulation mode) aerosol concentrations for an Arctic station (cf. e.g. Freud et al., 2017 or Schmale et al., 2018). It may still be an interesting place to study low-level cloud formation and dissipation, but perhaps not so much from the perspective of an aerosol-limited regime?
- The authors use CPC measurements to relate aerosol concentrations to cloud formation/dissipation events. Firstly, I think that the methodology related to the CPC measurements needs to be better explained. What air is pumped into the instrument? Is it "whole air", "cloudy air" or "clear air"? How are ice crystals and cloud (fog) droplets handled by the instrument? Is the air dried? Does the instrument have any detection limit in terms of number? Secondly, the CPC measures particles down to 6 nm (as stated by the authors). The Arctic is typically dominated by small aerosols in

summer (cf. e.g. Freud et al., 2017) but these small aerosols are not efficient cloud condensation nuclei. Figure 3 in Freud et al. shows that in summer, the accumulation mode particle concentration typically goes down drastically while the total concentration of aerosols goes up as new particle formation and growth controls the aerosol population. Why did the authors not use Scanning Mobility Particle Sizer (SMPS) aerosol size distribution or CCN measurements from Utqiagvik? I think these should be publically available (cf. e.g. Schmale et al., 2017).

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

Specific comments:

Abstract:

- Line 2: I would suggest reformulating the sentence including "…lack of downwelling…". It sounds like there is no downwelling radiation at all when the cloud is absent.
- Line 18: I am not sure why you emphasize the link to aerosol concentrations here? Isn't any general change in dynamics/radiative cooling more important?

1. Introduction

- Line 27: Are there any other studies than Shupe et al. (2011)? Would be interesting to know.
- Line 27: I suggest changing "These clouds frequently contain concentrations of both…" to "These clouds frequently contain both …".
- Line 54. "Simulations of Arctic clouds consistently show that over-abundant ice nuclei or ice crystal concentration can lead to cloud glaciation". I don't think this statement is completely true – it depends on what the authors mean with "over-abundant" and "Arctic clouds". There are several studies that show that mixed-phase clouds in the high Arctic only glaciate at extremely (i.e. unrealistically) high ice crystal number concentrations, e.g. Stevens et al. (2018), Loewe et al. (2018).
- Line 56: Related to the previous comment, I think a CCN-limited regime has only been suggested for high Arctic clouds?
- Line 61: In this paragraph, it could perhaps also be worthwhile considering the studies by Young et al. (2018) and Dimitrelos et al. (2020) where they point out the importance of large-scale divergence/convergence (and associated free tropospheric moisture supply) in governing the lifetime of Arctic low-level clouds.
- Line 75: When reading the introduction, I was wondering why you focus on atmospheric properties "after cloud dissipation". It would have made more sense to look the atmospheric state before cloud dissipation. In the methods section you then

explain why this is not possible, but I think it could be good to include a short explanation already in the introduction.

2. Instruments

- Line 91: The description of the HRSL is very brief and should be expanded. For example, what is the detection limit of the lidar? Is there a limit in terms of how close to the surface the signal can be trusted?
- Line 1010: How small concentrations of small cloud droplets can the cloud radar observe?

3. Methods

- General: it would be nice to have a map of the location of the station and also a brief description of the typical conditions (closeness to sea, potential pollution sources etc.)
- Line 130: I'm just curious, why 96%?
- Lines 138-140: I suggest replacing the word "when" with "if".
- Line 146: Why show times as UTC and not local times? Would make it easier to interpret the radiative fluxes.
- Line 154: It is not completely evident to me that the mixed layer (elevated aerosol backscatter) is shallower during the clear period. How do you see this? Maybe it would help to draw a line at the start of the clear and cloudy periods?
- Line 155: "Evolution in near-surface meteorology showed modest changes…". I interpret "modest" as "not pronounced", but maybe this is not what the authors mean. I would say that the change in wind direction is fairly pronounced at the time of cloud formation? And also the change in dew point temperature?
- Line 157: It is quite interesting that the particle concentrations increase so dramatically during the clear period. In summer, new particle formation and/or condensational growth of nucleation mode particles often takes place when there is sunlight and (initially) low background concentrations of aerosols (e.g. Freud et al., 2017). Could this be what is happening? Was this a typical pattern or only a one-time feature? Important here is of course also what air the CPC samples, if it is "whole" air or only cloud-free air.

4. Results

- Line 165: Just out of curiosity, was there any difference in length of the clear periods between the seasons?
- Line 170: I assume that the clouds with bases below 400m also could include other clouds than fog and low clouds? For example nimbostratus, cumulus and cumulonimbus.
- Line 188: What is the "1-sigma envelope"?
- Lines 190-194: I have several questions/comments regarding this paragraph.
    - When is the boundary layer backscatter (which should be dependent on the aerosol surface area, so mainly the accumulation mode) the highest/lowest? How does this agree with other in-situ measurements of CCN and/or aerosol size distribution measurements (e.g. Freud et al., 2017; Schmale et al., 2018; Schmeisser et al., 2018)
    - Is it really true that the "transition layer" is the shallowest in summer? October and September looks pretty shallow too?
    - I don't understand the sentence that begins with "Many processes may contribute to …". Shouldn't this layer just be a result of the vertical depth of the boundary layer/mixed layer?
- Line 213: The limitation of the HSRL should be mentioned in Section 2.

- Line 214: Can you really draw this conclusion from looking at averages? I would think that in order to make this statement, you would have to look at the individual profiles and make sure that the transition layer is always below cloud or within the cloud that the clear-sky period bookends?
- Line 221: The selection based on a maximum cloud top height below 2km makes sense and should be done from the beginning.
- Line 236: The cutoff backscatter values should be mentioned in Section 2. But I am also wondering what the authors mean with “clear sky”? I assume there should still be aerosols present, it is just that the instrument cannot detect these low concentrations?
- Line 241: What do the authors mean with the sentence “Being that the aerosol backscatter… was at minimum…”? Where and how do you see this?
- Line 241: Related to the comment above, how low backscatter values would you need in order to have accumulation mode aerosol concentrations below ~10cm-3?
- Line 248: Please define “RFD”.
- Lines 257-260. I do not think this argument holds. The backscatter will be dependent on surface area. If the aerosol population is dominated by small particles in summer, then the surface area will not be at its maximum, see also Freud et al. (2017).
- Lines 269-271: This results is interesting as the increased number of particles in spring/summer could be due to new particle formation and growth during clear periods, please see previous comment (Chapter 3, line 157).
- Lines 271-274: Does the CPC measure “whole air” or only “clear air”? If it is “whole air”, then why would the concentrations decrase?
- Line 290: I do not think this argument is true. The downwelling LW should also be dependent on the temperature, in particular if the LWP is larger than ~20gm^{-2} (emissivity close to 1).
- Line 293: How is the analysis affected by any presence of a stable surface layer (boundary layer decoupling)?
- Line 297: I think it should be mentioned in Section 3 that you use the soundings to calculate LTS.
- Line 300: Related to figure 7, why is the cooling generally smaller with more stable stratification (for clear sky)?
- Line 318: Which mechanisms are you referring to?
- Line 342: So this means that in summer you mainly have fog formation due to radiative cooling?
- Line 356: Are these results then inconsistent with the geopotential tendencies where you concluded that synoptic activity was more frequent in summer and spring during cloud dissipation events (lines 395-398)?
- Line 365: For the analysis of geopotential tendencies, I think it could also be interesting to look at these from the perspective of large-scale subsidence and convergence as in Young et al. (2018) and Dimitrelos et al. (2020). It would also be interesting to look at vertical profiles of moisture to see if the layer right above the cloud is a source or sink of moisture.
- Line 372: I would suggest inserting a “vertical” before “structure”.
- Line 380: How much was the number of cases reduced?
- Line 401: You mean in late spring/summer…?

5. Discussion

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

6. Conclusions

- Line 499: I thought the forcing from synoptic disturbances was stronger in late spring through summer (lines 395-398)?
- Line 511: I guess there is also a possibility that the cloud formation and dissipation events does not happen "in-situ" but rather that transport of clouds (and clear air) contribute to the observations made at Utqiagvik?

References

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