Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-815-RC3, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Processes contributing to Arctic cloud dissipation and formation events that bookend clear sky periods" *by* Joseph Sedlar et al.

Anonymous Referee #3

Received and published: 17 September 2020

In this study the authors attempt to explore reasons for dissipation and formation of low clouds in the Arctic, using a multitude of data from the ARM site in Utqiagvik (Barrow). They first isolate clear-sky periods using a ceilometer and refine these with additional data. They then proceed to analyze data from lidar aerosol backscatter and from in-situ surface measurements of aerosols, radiation and basic meteorology as well as indicators of atmospheric tendencies from soundings. They do this using composites of data for four years.

Their effort is ungrateful in the sense that it turns out to be very difficult to tease out any solid relationships. This is, while of course frustrating, in itself not a reason to reject

a paper; a negative result is also a result, and it all rests with how this is handled. However, the paper could be better organized and more clearly written. I recommend that the paper is accepted after major revision focusing more on the structure and language of the paper, more than on the results themselves.

Major comments: This is an original way to analyze data, and the approach is interesting. I commend the use of more than cases studies; while this is likely a reason for the lack of clear results, it represents a way to obtain more general results. Anyone can dig out a single case and speculate about reasons for a given outcome, but this is close to useless in a more general sense unless it can be shown that results are more general.

While this is a strong case for this paper it is also a bit of a weakness in the present manuscript. The background to the problem and the motivation for the method is presented in a very hand-waiving fashion; the current introduction reads more like a list of previous studies and suggestions than an organized argument. Many examples of suggested aerosol influence is listed, but isn't it quite clear why. While aerosols are certainly important, different clouds form mainly because of dynamics than by aerosol constraints. Different types of clouds form in different situations and differently at different locations because of different predominant dynamics; low clouds in the Arctic Ocean, frontal clouds in extratropical cyclones and deep convection in the tropics. All of this is modified but not determined by aerosols.

Hence, I wish that the authors more deeply criticize and discuss the problem of representatively, as a motivation to stay away from case studies, and then present more clearly the hypotheses they are attempting to test including potential effects of atmospheric dynamics. As it stands, I get the impression they throw whatever data they can lay their hands on, on this problem in the hope that something might show up. I also miss the motivation to why four years of data is used; why not five – or ten?

The paper – even its title– makes a big deal of the clear periods, but if one is interested in cloud dissipation or formation, presumably the happenings before and after

the shoulder times are the interesting things; not the clear period per see. Isn't the clear period in between in itself sort beside the point? Also, when clouds are dissipated, presumably new clouds will form at some later time, hours or days later; the formation of the new clouds at the end of the clear period may have absolutely nothing to do with the dissipation of the other clouds hours or days earlier. Calling these "bookends" is misleading in that the reader is lead to think of this as a coupled sequence of events; they may in fact be entirely different. Hence the focus should have been on either cloud dissipation or cloud formation – or both but separately – and then focusing on before and after cloud dissipation/formation.

This constitutes a problem with the lidar, since it is difficult or even impossible to obtain aerosol backscatter in the presence of low clouds, attenuating the lidar signal. This is just a fact of life and is discussed on lines 226-227, as in the passing; this information should be given and discussed up front. The results in Figure 3 should therefore be discussed in the context if being clear skies; not in the context of not being cloudy, since that contrast just isn't there. Of course it may still have some value to look at aerosol backscatter directly after dissipation and directly before formation in a statistical sense, as in Figure 4, but this caveat should be discussed up front; that the one set of plots represent after dissipation has happened while the other set is before cloud formation. Without knowing what the structure was before dissipation and after formation of clouds, the information value is limited. And BTW, is this really cloud dissipation/formation; isn't it just a hole in the cloud layer advected past the viewer? Maybe this is why its so hard to get statistically robust results?

At the end of the discussion section a hypothesis is formulated, almost like in passing; I'm sorry, but I don't get it. It builds on the Tjernström et al (2019) air-mass transformation hypothesis. But a central tenet in that hypothesis is the fact that over melting sea ice, the surface temperature is locked constant at the freezing point; here there is no analogy. So is cloud dissipation leading to surface cooling, then aerosol pooling, followed by fog formation, fog deepening and lifting to clouds? That would in essence

mean that cloud dissipation leads to cloud formation? If this chain of events is really happening, it should be a testable hypothesis; temperature should drop while aerosol concentrations rise with time, followed by fog formation and cloud base rising from zero to some height; in gact, the very same set of data used here could be used to test this hypothesis. Instead the hypothesis is not even clearly repeated in the conclusions, but brushed over with many words in paragraph two and beginning of paragraph three. If you want to pose a hypothesis, do it; else don't!

Finally, the language is sometimes what I would – in lack of a better description – call "flowery". It is important to have a capturing narrative, but unnecessarily complicated sentence structures sometimes lead to confusion and misunderstanding. So maybe sometimes be a bit less imaginative.

Minor comments

Line 28: Drop "even".

Line 29: Please rephrase; the temperature of low clouds do not reach "as cold as -34 °C" in "all seasons".

Line 14: Unnecessarily complicated. Suggest "While clear sky is less frequent than clouds" or even "While clear skies are rare".

Line 38: Lack of what? "longwave warming" or "Arctic clouds"?

Lines 39-40: Only true when the sun is absent or the albedo is high; over bare land and in summer, clear skies usually leads to a surface warming. Even in the Arctic.

Lines 41-44: A prime example of when there are too many ideas in the same sentence. Exactly what is it that "is currently understood". I know all this so I understand what you mean, but please rephrase anyway.

Line 43: "stratocumulus and also"

Lines 50-51: I would move up "in the Arctic" in that sentence, or it sounds like the

transition everywhere is controlled by Arctic clouds.

Line 59-60: So opaque liquid clouds would form out of what? Optically thin ice clouds?

Line 71: In what regard is that?

Line 71-72: This is a sentence where the narrative is that clouds dissipate and form at the beginning and end of the clear period, as if the dissipation and the formation where reverse analogs.

Lines 74-77: Here is a completely different take; now the formation clear period is at focus, not the dissipation of formation of the clouds.

Lines 106-107; what has “a diameter of 10 to 3000 nm”; the volume of the air or the partciles? I know the answer of course, but the sentence is rather unclear.

Line 107: Do all cloud-relevant aerosols absorb alcohol, or do we miss some?

Line 129: Grater than identically zero?

Line 136: How is the agreement on clouds between the ceilometer and the HSRL?

Line 146: I assume the base is at 100 m and the top is at 400 m; neither is between 100 and 400 m.

Lines 172-174: Another long sentence with more than one idea confusing the other. Is there any other way a clear period can end than by the emergence of a cloud? And is the ceilometer ever operating in anything but vertical mode?

Line 188: Not all months have a clear elevated “level of maximum variability”. Figure 4: Why one hour?

Lines 226-227: This is really important information to have before looking at Figure 3 & 4.

Line 239: What type of aerosol particle would not come from “below”; what aerosols do not have an origin at the surface except for those emitted by aircraft?

Line 278: “agrees” with what?

Figure 6: Why now 2 hours; earlier it was one?

Line 279: You are not exploring “phenomena”; you are exploring variables and trying to infer “phenomena”.

Line 325: ”strongly transparent”? Better say “almost opaque”.

Lines 342-344: Not sure I get this; if the dew-point deficit has a positive trend (is increasing) and the temperature has a negative trend (is decreasing), does that necessarily mean RH is increasing? Could the dew point not decrease so much more than temperature that RH stays constant or even decrease?

Line 383: “in flux”? Maybe chose a different wording?

Line 423: About the source of aerosols again; isn’t this trivial? Moreover, I think aerosols are defined as “airborne . . . particles” so there’s one “airborne” to many here.

Line 424: “general stable stratification” is probably incorrect, or

Line 364-365: This is a bold sentence, supported by only one reference. I’m not necessarily disagreeing, but still.

Line 383: “in flux”; is this a good choice of words?

Line 423: Here are the aerosol sources again; I’m no expert but unless you emit them from an aircraft, don’t they have to come from the surface?

Line 424: The statement on “general stable conditions” is probably inaccurate or at the very least debatable. Studies have shown that the most common near surface stratification over the whole year is near-neutral, but that stably stratified conditions prevail in clear conditions especially in the winter when they are also deep and strong. Additionally, is there no ground based convection over Alaska or at Barrow; I get over the ocean but this is on land?

Lines 485-489: Here's that hypothesis; I would have much liked to have the hypothesis at the front and the paper about testing it, or at the end as a bridge to the next study. Here it isn't even a conclusion; reading a bit hasty one could have missed it.

Line 511: Maybe avoid the word "transparent" in this context, as it is so intimately linked to other things in this manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-815, 2020.