Reviewer #1

The authors have generally speaking addressed my posted major concerns. There are two minor comments without an author response (corresponding to - l. 287, - l. 372 in the original submission).

Our apologies to the reviewer for missing responses to these issues raised during the first review. As the reviewer is aware, there were four major revisions to respond and incorporate with the submission, and by mistake these two points got lost in the process. In no way did we intentionally neglect them. In fact, both of these suggestions were dealt with, either during the revised submission, or with this second revised submission.

Reviewer 2

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

Submitted manuscript version 2. Ian M. Brooks

Overview

This revised manuscript is a significant improvement on the original. The major issues raised in my original review have been addressed. The result remain somewhat inconclusive, but nevertheless the extensive documentation of cloud, aerosol, thermodynamic, and large scale dynamic conditions are a useful contribution to the field.

I recommend that the manuscript is suitable for publication after minor revision. Detailed comments to be addressed are noted below.

Once more, we wish to thank Reviewer #2 for their careful consideration of our manuscript. The reviewer is still concerned with inconclusive results. This study has opened the door, and left it open, for the community to either lend support to our hypotheses and result that support them, or to nullify them and propose alternate hypotheses; the essence of scientific process.

We have considered the reviewer's minor suggestions and incorporated those where appropriate. Responses to the reviewer's comments are listed in red.

Detailed comments

Line 11 – "A suite of remote sensing and in situ instrumentation from the high-latitude observatory are analysed;..." -> "Measurements from a suite of...are analysed;..." – the measurements are analysed not the instruments.

Updated as suggested.

Line 14-15 – "the clear period bookends" – 'bookends' here is a rather casual, and not entirely clear, term. Maybe rephrase to something like '...aerosol....is relatively invariant during the periods bookending clear sky conditions'

Changed as suggested.

Line 20 – "aerosol particles concentrations changed by a factor" – a factor of what? Need a value (and sign) of the change here

Added that concentrations changed by a factor of two around summer formation events.

Line 40 – "effective infrared cooling from the surface results in near-surface temperatures to drop" – grammar, 'to drop' doesn't fit with the rest of this statement -> "effective infrared cooling from the surface results in near-surface temperatures decreasing"

Changed as suggested.

Line 108 – "tropospheric clouds were common" – tense doesn't match first part of sentence -> "tropospheric clouds are common"

Changed as suggested.

Line 137 - "although some concentrations may" – a very vague statement, need more detail. 'some' concentrations...high, low, variable but under some particular conditions?

This statement has been revised to reflect "low concentrations of small droplet sizes."

Line 141 – "its measurement is sensitive volume squared" – grammar – "its measurement is sensitive to particle volume squared"

Changed as suggested.

Methods

Line 172 – "...condition was not met, the clear period was discarded..." – suggest changing wording to "...condition was not met, the period was discarded...", if the period is discarded because of intermittent cloud then it's not really a 'clear' period for the purposes of this study.

Thank you for catching this ambiguity. Changed as suggested.

Line 228-238, discussion of figure 3 – the processes mentioned as possible causes of the drop in aerosol backscatter between BL and overlying air are all reasonable. An additional factor may be the typical decrease in humidity across BL top. For hygroscopic aerosol, particle size can change significantly with relative humidity (ballpark values are a doubling between 'dry' and 80% HR, and another doubling between 80% and ~100% RH for particles such as sea salt), this might lead to a drop in backscatter across BL top even for an aerosol population that was uniform in concentration and dry radius across the inversion. This is, of course, highly dependent on aerosol chemistry, and change in RH across BL top, and not quantifiable here, but worth keeping in mind.

The reviewer raises a very valid process that may influence the change in backscatter between near surface boundary layer sources and free troposphere. We agree with the reviewer, and have added the following as a potential process of importance:

…"and variability in the relative humidity profile."

Figure 4 – some of the panels show colours (at high backscatter) outside the range indicated on the colour bar.

This is true and is a caveat of holding the color bar limits the same for each seasonal subpanel. By increasing the upper limit, the details in the distributions would be less obvious for DJF and MAM. On the other hand, JJA and SON are dominated by fewer cases and as such the profiles of the PDFs reveal those peaks in backscatter outside the color bar range.

Line 347 – "...these clouds often modulate the stratification due to cloud top radiative cooling and induced turbulence..."

i) This phrasing is ambiguous – not clear if the meaning is that the stratification itself, or the modulation of the stratification, is due to cloud-top radiative cooling,
ii) The stratification referred to (or implied by the preceding statement) is the 'static stability near the surface' – I'm not sure that cloud-top radiative cooling and associated turbulent mixing impacts strongly (or in some cases at all) on the near surface stratification. That is much more strongly influenced by the simple presence of cloud and whether the surface itself is cooling radiatively (clear skies) or not (cloudy skies). Cloud driven turbulence will certainly impact BL thermodynamic structure as a whole, and might extend to the near-surface layer, but is only one of several factors affecting surface stability.

After re-reading the original passage, we agree with the reviewer's concern. To address these valid arguments, we have revised the sentence as:

"Arctic stratocumulus clouds exert a critical influence on the stratification of the lower Arctic atmosphere via their significant greenhouse effect (longwave forcing at the surface) and cloud-generated turbulent mixing.."

Line 354 – '950 hPa level is generally around 500 m AGL in the Arctic, which frequently encompasses all, or a fraction of, the Arctic atmospheric boundary layer and the sub-cloud mixed layer' – rather loose and partly redundant phrasing. The lowest 500m must always encompass at least part of the BL. It will often encompass at least part of the sub-cloud mixed layer – though since your focus here is on cases where cloud base is <= 400m, it must also always encompass the sub-cloud layer for all cases considered here.

This statement has been revised as follows:

"…frequently encompasses all, or a large fraction, of the low cloud driven mixed layer…"

Line 485 – 'Little changes in the vertical structure...' -> 'Little change in the vertical structure...'

Changed as suggested.

Line 525 – '...prior in...' -> '...prior to...'

Changed as suggested.

Reviewer 3

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

This is a clearly improved version of a study where 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).

The framing and organization of the study is much improved and has less of a "helpless searching" character; they go in with a hypotheses and invalidates most of them; that's clearly useful results. I'm basically fine with the revision, I only have a few minor points below that the authors can use if they wish to clarify/improve the text more.

Once more, we wish to thank Reviewer #3 for their careful consideration of our manuscript. We have considered the reviewer's minor suggestions and incorporated those where appropriate. Responses to the reviewer's comments are listed in red.

Line 10: Drop "relatively"; it is "limited", full stop.

Removed as suggested.

Lines 31-33: Valid only if albedo is also high enough.

This is true, so we have added "reflective sea ice" to document this important point.

Line 60: No atmosphere anywhere is very stationary; the Arctic atmosphere is no exception.

Absolute true. We have reworded this line to highlight that the atmosphere is not stationary and synoptic forcing, etc., is ongoing during all seasons across the Arctic.

Line 76: Unclear use of "transition of cloud lifecycle". First, a "cycle" implies "transition", so what is a transition of the cycle? Second, does this (= low CCN count) apply also on cloud formation?

To address the first point, the text has been modified as follows:

"…are an efficient mechanism in initiating cloud dissipation…"

For the second point, one could hypothesize that low number concentrations based on the Mauritsen et al. (2011) study which documented a situation where the low particle concentrations likely contributed to cloud dissipation; and since the concentrations remained low, cloud formation was likely suppressed, even in a supersaturated environment. However, this text in the paper is describing results of modeling studies of cloud dissipation processes so we do not wish to speculate on formation inhibition; that is the scientific hyptotheses/basis to test within our study.

Line 142: "… attenuated in by the presence …"?

Updated as follows:

"…the signal may be attenuated by ice crystals…"

Line 140: Unclear formulation in "Near-surface measurements … where observed …" I submit you either "used" near-surface measurements or "observed" near-surface meteorological variables.

The phrase has been updated to "Near-surface measurements…. were made from…"

Lines 162-174: I can think of cases, especially close to a coastline, where there would be substantial but partial cloudiness for extended periods of time while the nadir-pointing instruments would either indicate completely clear or completely cloudy conditions.

Unfortunately, this is the trade-off between spatial coverage and detailed vertical sampling. While it may be the case that variable cloudiness occurs outside the field of view of zenith-viewing instruments, the NSA is well documented as a cloudy environment, which agrees well with observations of cloud occurrence/persistence over the Arctic sea ice.

Line 170: Why 96%? Why not 95% – or 90%?

Since we require at least a 2-hr period of clear sky, 96% corresponds to approximately 115 min of clear sky; meaning we allow for 5-min of intermittent cloudiness or scattered cloudiness within the lowest allowable period (2 hrs). This could have been decreased to only 90%; however, this would inevitable allow longer periods of intermittent cloudiness to be considered within an identified clear sky period.

Lines 198-199: Later you find little evidence that aerosols change much at either "bookend", but for this example there seems to be a large almost "hard-top" change; a little confusing as one goes into the story with this image imprinted on the retina. Maybe this was an odd case; maybe you should drop the CPC results in this figure.

We find the results in Fig 1e to be consistent with the changes reported in CPC concentrations around dissipation/formation shown in Fig. 6. While not consistent across all seasons, the aerosol concentration changes are quite large during events in spring and autumn, and especially large during summer. This is one of the major findings of the paper, and as such we do not think the example in Fig. 1 is misleading or is it an odd case.

Lines 204-205: Not an English expert, but I associate "vast" with space as in a large area; here it is used with "persistence" which is temporal. Is there such a thing as a "vast persistence"?

Vast has been removed from the sentence.

Line 211: Pretty obvious that it would, don't you think?

We believe it is important to relate the periods of clear skies with overall monthly cloudiness. It is not entirely trivial to think that abbreviated clear sky periods would not be more frequent during months with high cloud occurrence. Or vice versa, months with less cloudiness may lead infrequent but longer individual periods of clear skies.

Figure 3: It looks to me that adjacent months are quite similar within seasons. Given the few cases (September & October only six) I wonder why you choose to show this monthly and not by season. Also wonder how representative some of these profiles are.

These profile statistics could have been combined into seasonal plots, but we believe it is important to see where the transition height between lower troposphere mixed layer aerosol backscatter and free troposphere occur each month.

Lines 293-294: Correct me if I'm wrong, but up tpo this point the discussion is general, including both "bookends", so the "cloud lifecycle changes" include both dissipation and formation? Then does the concentration drop across both. Also see my earlier comment about changes (or transitions) in life cycles; the cycle implies a change, so what is a change in the cycle?

SThe reviewer is not incorrect; the paper looks at changes occurring both around dissipation and formation. Figure 6 shows the concentrations both before and after dissipation (a-d) and formation (e-h). As described above, the changes depend on the month, with summer showing the largest change in aerosol concentrations, especially for the low cloud and fog events.

Line 298: Here it seems to shift from general to specific; dissipation. Maybe mark this by a new paragraph.

New paragraph has been added, as suggested.

Line 329: Confusing: "… are not limited to Aitken but include larger particle sizes that can activate." Do you mean smaller?

Correct. We have added the word "smaller" before Aitken for emphasis.

Line 338: Using "… imply that … may not …" indicates unnecessary uncertainty. Stick to only "imply"; that is sufficient.

Changed as suggested.

Line 354: I would use "a large fraction of". With the surface being one anchor point and the other being ~500 m, while "a fraction of" could be interpreted as even only 1% of the whole, the sentence becomes confusing.

Changed as suggested.

Line 367: "… clear-sky LWN and LTS modes …" sounds like these two variables had two separate modes, rather than that the combined pdf of both has different modes. Suggest using "LWN/LTS modes".

Changed as suggested.

Line 415 and onward: The wind direction by itself is a blunt instrument; one can have a small change in a generally southerly wind, say from 165 to 195 degrees, as a cold front passes moving east to west, but still with a substantial change in air mass. The latter may be much more significant than for example changes from 180 to 360 degrees on either side of a passing high-pressure ridge within the same air mass.

The reviewer is correct. However, we believe the results shown for the differences, or lack thereof, in wind around dissipation/formation events is consistent with the humidity and synoptic tendency results, and therefore we kept the text in its current form.

Reviewer 4

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I would like the thank the authors for their effort in replying to and considering all comments they have received. This is an interesting study and the manuscript now reads very well. I recommend publication and have just a few minor comments:

We thank the reviewer for once again considering our manuscript for publication. We are happy to hear the reviewer has responding positively our revisions, which were a result of thorough suggestions/comments by this, and 3 other, external reviewers. Our responses are provided below in red.

• Regarding the detection limit of the CPC, in my previous comment (general comment #3 by reviewer 4), I was wondering about the lower detection limit of the CPC and not the higher.

We apologize for the confusion in our response to this comment during the first review. According to the specifications listed in the instrument manual, the lower detection limit is $1x10^{-4}$ cm^{-3}. The concentrations observed in our study (e.g. Fig. 6) are orders of magnitude larger than this detection limit.

• Line 537: I would suggest adding "and subsequent wet scavenging/deposition" after "… result of aerosol activation". If there was no wet deposition, then the aerosol number concentration should not change a lot (if both cloudy and clear air is sampled by the CPC). The decrease in aerosol concentration suggests that the occurrence of drizzle, which is interesting in itself.

We do not wish to speculate on drizzle; however coalescence/scavenging is likely and ongoing process. We added "and/or coalescence/scavenging"

• Line 547-548: Please modify the sentence "… onset of clear sky periods, and subsequently the end of clear periods…".

We have modified the text to clarify this statement.

• Line 555: It is not completely clear what the author means with "at the same time" here. I would suggest merging this paragraph with the previous as it would also make it clearer that the increase in aerosol concentrations occurs during the clear periods.

We have clarified this statement by removing "At the same time" and replacing with "In summer".