Reviewer #1

The authors have addressed all comments and have done a great job in documenting the limitations in their model set-up. Since these, however, are not mentioned in the discussion session, I think it would be good to include the term 'idealized' or at least 'semi-idealized' for the description of their LES simulations in line 398.

> We have clarified that these simulations are idealized in the conclusions as suggested by the reviewer.

I also think that the statement in lines 405-406 is slightly strong, so I suggest replacing 'more likely' with 'likely' or 'possibly'. You see small errors in processes like rain autoconversion and WBF can rapidly deplete cloud liquid. For example with this highly idealized aerosol forcing, you achieve an enhanced condensational growth of the existing droplets. Thus the diameter threshold for droplet-to-rain autconversion can likely be reached much faster than in the case of a more gradual aerosol reduction.

> We have clarified in this section that we believe that our simulations (ASCOS and SMT) that agree more closely with observations are more likely to have dissipated from a lack of aerosol than OLI, which did not. Of course, just because the LWP response is similar between model and observations does not mean that the causality behind dissipation must be the same. We have clarified in this section that model-obs agreement in LWP decrease suggests to us that those cases should be investigated further.

Reviewer #2

Overview

Overall, this revised manuscript shows a clear improvement from the initial submission. The methodology of the study is now explained sufficiently and the results are now presented in a better way. That said, there are significant "disagreements" between what is described in the revised manuscript (section 2.3) and what is the setup of the model study (doi.org/10.5281/zenodo.6514322). Further- more, chosen methods lead to certain limitations of this study, but this is currently not reflected in the conclusions.

Therefore, in the current version of the manuscript, some parts of methods are misleading. Authors are kindly requested to correct the description of their methodology and briefly clarify the limitations of their study in the conclusions.

1 Overall recommendation

Publish after minor revisions

2 Minor Comments

2.1 CCN concentrations

The description of the CCN concentrations in the manuscript differs from the prescribed subsidence in the model setup files (repository https://doi.org/10.5281/zenodo.6514322).

Table 1:
The initial concentration of CCN in the SMT case is listed ad 200 cm^{-3}.

Meanwhile, model setup (SMT-RAMSIN) lists the same CCN concentration as in the OLI case.

CCN MAX = 80., ! CCN (#/mg) normally 170

This value also appears in the horizontally averaged concentrations in the model output.

It is of course understandable that authors used similar CCN concentrations in all three cases. However, this has to be clearly stated and also briefly justified.

> We appreciate the reviewer pointing this out, as this was due to an outdated model namelist/data that was uploaded to Zenodo and used to generate some figures. We have corrected the uploaded files (now available at https://doi.org/10.5281/zenodo.6600103), as well as re-generated the affected figures with data from the simulation reported in Table 1. Differences between the simulations were very small and do not affect our conclusions.

2.2 Subsidence

The description of the subsidence in the manuscript differs from the prescribed subsidence in the model setup files (repository https://doi.org/10.5281/zenodo.6514322). Lines 231–233: "..Large-scale subsidence is applied throughout the simulation by imposing a horizontal divergence of $2\times10^{-6}s^{-1}$ at every model level, with a boundary condition of $w_{sub} = 0$ at the surface."

Meanwhile, model setup (ASCOC-RAMSIN, OLI-RAMSIN, and SMT-RAMSIN) do not prescribe this subsidence, but rather large-scale convergence:

CDIVMAX = -6.1224e-6, ! Divergence amplitude (s -1) (negative = convergence)

This large scale convergence also appears in the horizontally averaged vertical velocity in the model output.

This is still and interesting example of an idealized model setup. Nevertheless, the description in the methodology has to be corrected.

The prescribed subsidence is set by the namelist parameter 'DIVLS', not 'CDIVMAX'. The DIVLS values in all namelists correspond to large scale subsidence as correctly stated in the manuscript. CDIVMAX is used for convergence forcing for idealized convection simulations and as such is not used at all in our simulations.

2.3 Description of Albedo

The Experiment Setup (2.3) does not describe the surface albedo. While some of the cases take place during the polar day, the surface albedo is an important property in the radiative scheme.

The model setup prescribed albedo value 0.5 (ASCOC-RAMSIN), respectively 0.6 (SMT-RAMSIN). These albedo values should be described in 2.3 and justified.

We have clarified the albedo values used in section 2.3. These values are in-line with albedo measurements taken over the Arctic, which typically range from 0.5-0.7 (e.g. Lindsay and Rothrock, 1994)

2.4 Comparison Figure not Consistent

The figure 5 shows an important comparison of all three cases. Unfortunately the time point of CCN removal differ across it figures. The time scales differs as well.

This figure would be much more useful if the time points and time scales were synchronised across all three panels (in a similar way as it is in figure 6).

This figure would furthermore benefit from being larger in size.

The figure has been updated to be aligned vertically, with aerosol dissipation occurring at the same location on each panel and the time scale on either side has been standardized.

2.5 Turbulent Decay to Non-LES Regime

In some of the simulations, the dissipation of the clouds leads to decay of turbulence.

This could be clearly seen in the horizontally averaged variable TKEP in the model outputs. By the end of one of the simulations, it drops to values 0.0005–0.0009 m^2/s^2, which is almost the same value as in the non-turbulent free atmosphere above the boundary layer (0.0005). This indicates that the subgrid scheme no longer approximated the turbulent cascade and the model no longer resolves large eddies.

While this is totally acceptable result, it would be fair to state in the Discussion and Conclusions that the LES by the end of the simulation does not operate in LES regime, but rather as a fine-scale convection-permitting model.

In the absence external forcings (which are not applied to our simulations), when the clouds are fully-dissipated there is no mechanisms to generate turbulent motions, so it is natural that TKE settles to the background value.

2.6 Coupled Boundary Layer

Line 408:
“...coupled boundary layer (OLI)...“
This statement is in a direct disagreement with the description on line 204: ”...surface fluxes are disabled...”

Perhaps it would make sense to correct line 408 to “previously coupled” or “recently decoupled”.

We have changed the language in this line to “previously coupled” as suggested by the reviewer.

2.7 Strong Conclusion Statement

Line 422–424:
“We believe that, given the evidence from these three simulations, the microphysical balance state of the cloud may be more important to determining the response to aerosol removal than boundary layer properties”

The conclusions should clarify for which group of boundary layers this applies (decoupled, & polar day, &, large scale convergence, & weak windshear, etc.).

We have clarified that this conclusion is bounded by the conditions the reviewer has pointed out and that further testing with additional cases, boundary layer conditions, and model forcings is needed to more clearly understand the relative impacts at play.

2.8–12 Further Minor Comments

- line 238
  The word “decase” does not seem to fit the context here.

  Typo fixed to read “decrease”

- Figure 7, panels a and b
  The timeseries “precip” differs between the panels.
  Either there is a numerical mistake, or the captions is incorrect in the sense that “precip” refers to two different variables.

  This variable refers the to the removal of liquid/ice (for panel (a) and (b), respectively) by accumulation of precipitation at the surface. The caption of the figure has been updated to make this more clear

- Figure 8, panels a and b Same as for Figure 7a,b.

  See response for Figure 7

- The figures 7, 8, and 9 show very important results, but the panels are very small. Please expand them.

  The size of the figures as well as the panels have been increased

- Code and data availability.
  The links should be updated to the newest version of repositories.

  Links have been added