Review for acp-2022-36:

'Arctic mixed-phase clouds sometimes dissipate due to insufficient aerosol: evidence from observations and idealized simulations'

by Sterzinger et al.

Arctic clouds remain a great challenge for the climate and weather forecast community, as the processes that determine their life-cycle are poorly understood until today. This study uses a Large-Eddy Simulation to investigate observed cases of rapidly dissipating clouds. The authors explore the hypothesis that a limited aerosol availability may be the primary cause for the observed cloud depletion. This is a very interesting and well-written paper. I have only one major concern regarding the experimental set-up and its realism (see main comment 1). Apart from this, addressing my comments below should not be very time-consuming, thus my recommendation is minor revision.

Main comments:

1) The authors explain the methodology between lines 200-205. However this paragraph concerns the aerosols that drive CCN activation. Are only CCN removed in the sensitivity simulations? Do INs remain unaffected? If only CCN are modified, I wonder to which extent this can be realistic. If e.g. aerosol transport changes drastically due to changing large-scale conditions, shouldn't this affect both CCN and INP availability (especially in decoupled environments were surface aerosol sources are expected to have limited impact)? If all aerosols are removed (n_{aer} and n_{INP}) please state this explicitly in the text. If not, it is worth performing additional simulations with no CCN/INPs at all. While a lack of CCN leads to decreasing cloud liquid, decreasing INP concentrations can reduce the efficiency of WBF process and change the timescales for cloud dissipation.

2) I think that the impact of boundary layer stability is not discussed as much, while it can be very important. For example, while OLI and ASCOS cases are discussed as similar in the text, the ASCOS momentum flux seems about a factor of two weaker than the OLI flux. It is worth investigating and discussing in more detail how the initial thermodynamic state affects cloud evolution. Also if high time-resolution thermodynamic profiles are available (e.g. from radiometers), it should be investigated whether the changes in modelled thermodynamic stability after aerosol depletion conform with observations. If significant deviations are found between model and observations, this might explain to some extent the deviations in LWP evolution (Figure 6).

3) the authors state in the abstract that cloud response to rapid aerosol depletion is case-dependent. However (following my previous comment) is it possible to draw any conclusion regarding the thermodynamic/macrophysical conditions that are more likely to lead to cloud dissipation in the absence of significant aerosol forcing?

Minor Comments:

Line 187: *n* in DeMott formula represents aerosol concentration at STP conditions (scm^{-3})

Section 2.3: I would appreciate more information on the experimental set-up. What profiles are used to force the model? Is it only the potential and RH profiles shown in the figures? Also what about the surface conditions? Is the model run with fixed surface temperature or fixed surface fluxes? What about the assumed surface roughness and albedo? Or is there a surface model? If fixed surface values are used, then state the actual numbers. How long is the spin-up time?

Line 205: For how long is nudging applied (6 hours as indicated in the plots?)? Also why so strong nudging in the PBL is necessary in a model that does not account for varying large-scale forcing? What happens if you don't apply nudging before aerosol removal?

Lines 265-270: Could erroneous cloud top displacements in the model be corrected with a better constrained large-scale subsidence? It wouldn't be strange if the same horizontal divergence is not suitable for all three cases.