This paper describes a hypothetical numerical Large Eddy Simulation, to test the hypothesis that Arctic clouds are sensitive to entrainment of free troposphere aerosols from remote sources. There is nothing fundamentally wrong with the paper; I just can't help wonder why this study was perceived, why now, and what new we can learn from it? There are a lot of detailed results that seems a bit in lack of a plan how to use, and I think the study needs to develop before it can be published; hence "major revision".

Thank you for the critical comments. They point to a need to better motivate the study and frame the conclusions. In response, we have substantially modified these parts of the paper.

Additionally, in response to comments from both reviewers, we decided to make major changes to our simulations. The surface temperature has been increased to 273.15K, a value that is frequently measured in the transition seasons over ice in the Arctic. Solar radiation is now turned off, as suggested by this reviewer, in order to simplify the setup and remove the need to specify a date associated with the simulations. Ice processes remain on, but because these simulated clouds are only slightly supercooled, the ice is unimportant and is now completely neglected in the presentation of the results. A small suite of sensitivity tests is presented in which the initial thermodynamics conditions and the subsidence rate are modified. Among these are simulations that are very similar to our original simulations that used a surface temperature of 263 K. We do not intend for these sensitivity simulations to be a complete investigation of the sensitivity of our results to these choices, but they do serve to illustrate possible alternative outcomes.

Main concerns

The study essentially falls a part into three stories: 1) one that deals with the entrainment of aerosols from the free troposphere into the boundary layer; 2) a second that deals with the effects of those aerosols on the liquid water clouds in this boundary layer, and; 3) third, the results, as a consequence of 1 & 2, on the boundary-layer structure.

For all parts, one problem is the case and the LES. There are no attempts to illustrate or ascertain that this LES in this setup is capable to simulate this cloud and that the processes that makes it tick are adequatly modeled. The case itself looks a lot like a midlatitude marine stratocumulus but is placed at a specific date and specific location in the Arctic. But there is no information on how the case was designed and why these choices were made. The results are dependent on the rate of entrainment, which is dependent on the model formulations, the setup (e.g. subsidence) and the resolution etc. If the entrainment is sensitive to for example resolution, then all the results here will also be.

We have added more justification for the set up in the manuscript. Our setup is based on our experience with the real simulated cases in Sterzinger et al 2022 in which the shallowest boundary layer was about 600m, all were well-mixed or well-mixed with a shallow stable layer at the surface, and liquid water paths were ~25-100 g/m^2. Many of our choices are also supported by Jozef et al. 2023. Please see the revised manuscript for more details. The date and place are unimportant except insofar as it sets the solar radiation. We agree that there are similarities to midlatitude marine stratocumulus setups, but we note that in both the original simulations and the new simulations, we included a moisture inversion above cloud top which is a feature that is typically only found in the Arctic. That inversion was not obvious in the original submission since we only showed the initial relative humidity profile.

In response to this comment, we have added a small suite of sensitivity tests which include modifications to the subsidence rate and thermodynamic conditions. These include tests in which the temperature inversion strength is halved, the surface temperature is decreased by 10K, and a stable layer is introduced below the cloud layer.

And what is new here? The second author already a few years ago showed, using another LES but on a well-documented real observed case, that free troposphere aerosols are indeed entrained into the PBL where they have an effect on the clouds. In that paper, as also in this, one main conclusion was that aerosol observations near the surface does not necessarily provide any guidance as to what goes on in the cloud layer, especially if this is dynamically decoupled. So what does this paper add that wasn't shown already in the previous paper? And why should aerosols not be entrained? At the interface of a turbulent boundary layer to a laminar free troposphere there is always entrainment. How much may depend on how vigorous the turbulence is and how well resolved the stably stratified interface is resolved.

Yes, Igel et al (2015), the second author's paper, already showed that the aerosols can be entrained. However, it is incorrect to state that this paper also looked at the impact on the cloud properties. It did not and in that respect, this paper adds to the previous study.

We agree – it is not surprising that the aerosols are entrained. As the reviewer says, why wouldn't they be entrained? The question though is not will they be entrained, but rather, will they be entrained quickly enough (when present at realistic concentrations) to impact the lifetime of the cloud? Is their presence necessary or sufficient for the maintenance of the cloud? These questions are now more clearly made in the revision. Our sensitivity tests allow us to see how the results change with different thermodynamic and turbulent conditions.

So, what is new here? Sure, there are more runs here and more details but how are those used to shed more light on the delicate balances at the cloud top? It looks to me that the synoptic scale divergence prescribed has a large effect on the result; maybe there should have been less runs with different aerosol settings and more runs with different subsidence? And why was not inversion strength varied or the cloud bulk features(?); none of the simulated clouds are very dense. So, the results show that entrainment of free troposphere aerosols happens (provided there is any right at the interface), but any boundary-layer meteorologist could have told you that! What else did we learn?

Again, the point was not to learn that the entrainment happens, please see the above responses.

Once inside the boundary layer these aerosols have more or less the expected effects. Ice is there but IN is so small that ice plays no important role to the dynamics here. So why have it there? Solar radiation is also present, but so little it (probably) do not have an effect. So why have it there at all? More free troposphere aerosol results in more CCN in the boundary layer – obviously – and this results in denser clouds (larger LWP) with smaller and more numerous droplets. And this in turn leads to more cloud top buoyancy and more mixing; Nothing spectacular here; mostly the expected effects. But lots of details that may – or may not – reveal something. Problem is those are never explored in any detail; the analysis is unimaginative and the result is rather boring. Probably right, at least in principle, but still rather boring. With this effort in numerical simulation, there just has to be something more you can do.

We agree, the ice plays a minimal role and we also agree that the solar radiation plays a minimal role. We have taken the reviewer's suggestion to turn off solar radiation but choose to keep ice processes.

Minor comments:

Line 8: That the cloud top does not move is controlled by subsidence. In reality this mean that the cloud does move upward by mixing, but that i is immediately advected back down again by synoptic-scale advection. The bulk result may be close to net zero, but mixing and advection are not the same thing.

We have removed "does not lower rapidly in time such that it" from the abstract.

Line 15: Clouds are indeed important for Arctic climate but I have seen no study that shows them to have a large effect on “Arctic amplification”. If these authors have, I’d like to see a reference here.

We have removed these statements as part of our rewrite of the introduction.

Lines 20-21: A reference should be inserted here, as this statement comes from one single study (SHEBA) and most other summer studies I have seen does not show this warming regime, unless the ice has melted completely.

We have removed these statements as part of our rewrite of the introduction.

Line 45: “argued” is better than “found”; there is no way in which Matt could have proven this, but it is a good argument.

Agreed and changed.

Line 64: What “scale” is that? LES is a numerical technique and is appropriate for flows with large enough eddies but there is no specific scale; it all depends on the problem.

This statement has been removed.

Lines 73-74: So what happens with coalescence? Each droplet forms on one CCN each, but after having collided, forming a larger drop, there is no way the new aerosol formed by evaporation of that drop can be assigned back to the original CCNs.

RAMS tracks the aerosol mass contained within hydrometeors such that aerosol mass is contained within rain. When the rain evaporates, the particle returned to the atmosphere is should be larger than the particles that were originally activated to form the droplets that then collided to form the raindrop.

Line 69: I’d like to know more about the aerosols scheme. How is formation of new aerosols handled and how are aerosols that become CCN or are entrained replaced? How is size distribution consndered?

We have added more information regarding the aerosol scheme. In short, we do not have new particle formation – it is clearly a limitation of the study. The size distribution is assumed to be lognormal. The mean size changes in time, but the distribution width is a constant. More information has been added to the manuscript.

Line 81-83: This is a different motivation to that given earlier, and does not rely on assumptions on entrainment.

We’re unsure what the reviewer’s concern is here.

Line 84: Is this radiation code sensitive to effective droplet radius?

Yes. This is now explicitly stated.

Line 86-87: Setting the heat fluxes to zero is a reasonable assumtions; they are never very large anyway. But what about the momentum flux? Is this a free-slip simulation? How is this “reasonable” for ice surfaces?

The roughness length for momentum is set to 0.0005m based on Schroeder et al 2003 (https://doi.org/10.1029/2002JC001385). This same study finds that the roughness lengths for sensible and latent heat fluxes are 3-4 orders of magnitude lower, which justifies our choice to neglect these fluxes (as the reviewer points out).

Line 94: It looks like the cloud top is decreasing, at least for the less vigorous boundary layers. So, how was this value selected and how is the LES upper boundary and the magnitude of the entrainment affected? Is it a special target to have a constant cloud top constant, and should that then not require different divergence? And how would that affect entrainment?

The divergence rate was selected somewhat arbitrarily, our main concern being not to choose a value so low that the cloud top hit the model top. We now included sensitivity tests to the value that we chose.

Line 102: In the whole intro with references and all, the units for aerosol concentreations is cm^{-3}. Here all of a sudden, in the LES, it is mg^{-1}. Why?

RAMS takes number concentration input in mg^{-1}, but it is typical to discuss concentration in cm^{-3}, so we use that unit elsewhere.

Line 105-106: Since IN and ice processes are hardly considered, why even bother with IN? This is hardly a prototypical mixed-phase cloud as it is.

We have removed the discussion of ice processes.

Line 156: Delete space between “salt” and “20”.

Thanks, this has been done.

Line 165: Do you mean “dropets” and not “aerosols” here?

Yes, thank you.

Line 176: What “cloud radius”?

We mean “cloud droplet radius”. This is corrected.

Line 179: What “cloud number concentration”?

Similarly, we mean “cloud droplet number concentration”. This is corrected.

Lines 198-199: Why choose such an awkward date? While it is true that aerosol concentrations are low in autumn, this is a hypothetical LES and it would not be more or less hypothetical with the sun switched off completely.

No date is used in the revision.

Line 200: The flux divergence cannot have the unit “W m^{-2}”.
True, the unit is typically W m^{-2} m^{-1}. Here we have only taken the difference in flux between cloud top and cloud base, but not divided by the cloud thickness, so the unit is accurately reported. We applied the term “flux divergence” despite not dividing by the thickness as has been done by others, e.g. Zheng et al 2021 (https://doi.org/10.1029/2021GL094676). We have changed the term to “flux difference.” Note that this quantity is equivalent to the integrated cooling rate.