This is the second time I read this, and very I'm happy to say it reads much better this time. After my first read i felt the changes still needed could be easily done by editing and I was leaning towards "minor revision". However, summarizing my finding they now appear somewhat more substantial and there are issues that must be resolved. Hence, I'm going for "major revision" but will let the editor know I would survive if the decision came down to be "minor".

Major comments:
While my first reaction to a summertime case without solar radiation was quite negative, and I don't thing I explicitly suggested that, I must say as long as its clear that this is a hypothetical experiment, is is quite successful.

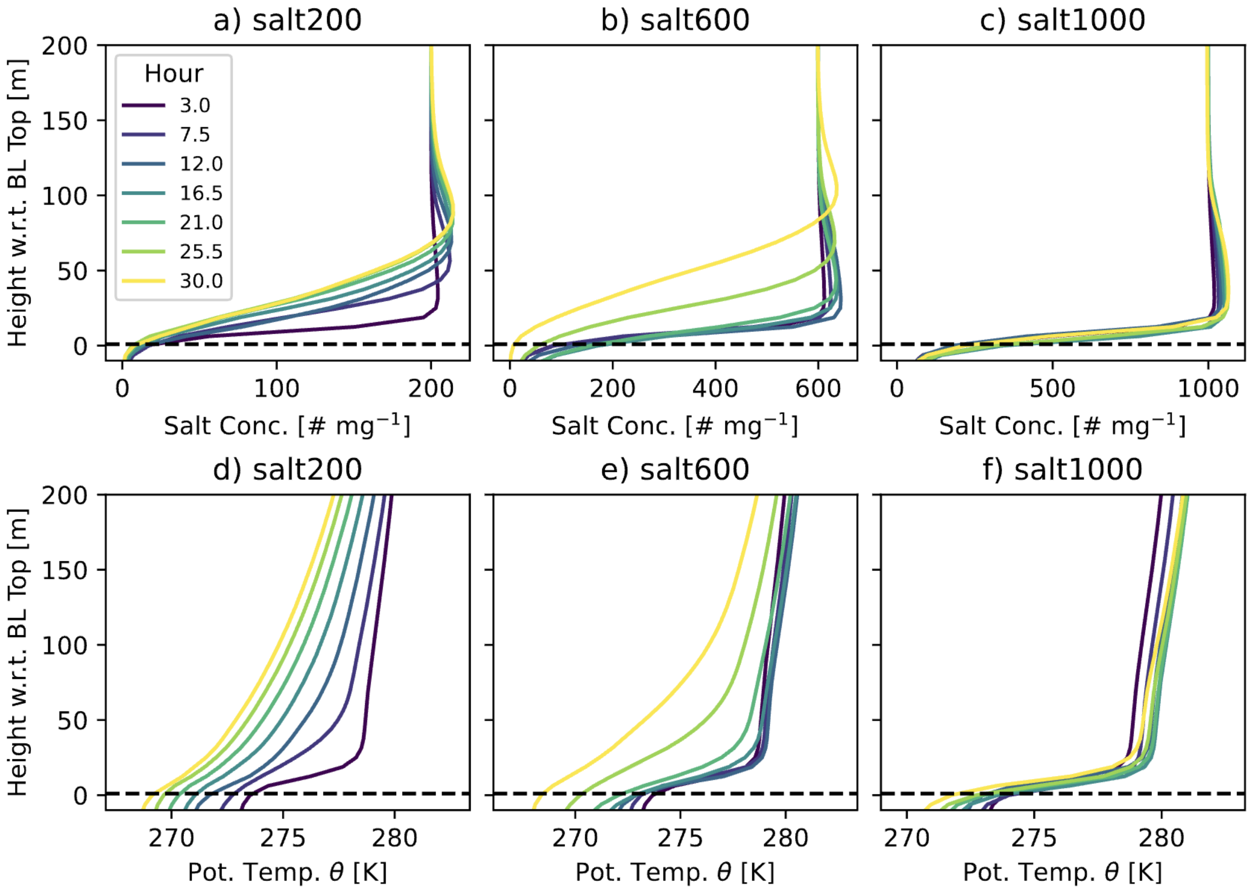
However, the sensitivity case with a stable near-surface PBL is quite unrealistic; I've seen many profiles of clouds like this and I've never seen one like it. I can't see what process that could lead to this shape of profiles, and it does have the unfortunate consequence of making a much juicier cloud than all the other cases. I instead suggest lowering the surface temperature by that amount, rather than increasing the cloud temperature. That would serve to preserve - roughly - the cloud thermodynamics and avoid changing too many things at the same time. In addition it would be more realistic. Fortunately, I don't see the results from the sensitivity tests playing a large role. In fact, I also suggest dropping Section 4.3 entirely. As it is now, it doesn't add much to the conclusions, but it also seems to me to contain a lot more than we get to see. Maybe seeds for another paper, rather than "waste" it here?
We have taken the reviewer's suggestion and removed section 4.3.

I also have two more substantial comments. The first relates to the experimental set up in relation to reality and what is explored. In reality, entrainment tends to elevate the top of the cloud layer by mixing in free troposphere air into the boundary layer, and so for a constant cloud-top height, subsidence is required to balance this. Hence, if entrainment is varied while subsidence is kept constant the cloud top height will vary. And they do; this is also what the simulations show and - in my opinion - while realistic it adds two complications.

First, at the end of the manuscript there is a discussion about how the subsidence taking over from entrainment and "collapses" the boundary layer such that the "plume" of aerosols in the free troposphere separates from the boundary-layer top. Now, for the life of me I can't understand why the free troposphere aerosols are not advected along with the PBL top so that the "plume" follows it. These results to me seem unrealistic and I would like that this is properly checked before the manuscript is accepted. I understand that this could lead to a major revision, but I hope not.

The reviewer is correct that this mechanism was not sufficiently explained in the text. The collapse of the boundary layer also has the effect of weakening and thickening the inversion. This effectively increases the distance between cloud top (near the bottom of the inversion) and the tropospheric aerosol (near the top of the inversion). We have updated Fig. 7 to also include the evolution of the temperature inversion for the given cases, and have added discussion of the inversion evolution in the text .

Attached below is the updated Fig 7. The new second row shows that the lower LWP clouds (salt200 and salt600) have a weakening and thickening inversion. The top of this inversion occurs at the same level as the boundary of the free tropospheric aerosol layer. This kickstarts a negative feedback, in which a weaker cloud results in a weakening and thickening inversion, which separates cloud top from the aerosol layer, which in turn continues to weaken the cloud.



Second, if this is realistic it contaminates - if you will - the results. There is the effect of entraining aerosols, which to a first order is related to the aerosol concentration in the free troposphere and, to a second order, has the feedback that a more "active" and persistent cloud, entrains more efficiently thereby contributing to its own persistence. But with the PBL collapse and the separation, which also generates a feedback by additionally suppressing cloud formation, follows yet another aspect that, if realistic, seems to be somewhat artificial. Hence, one could debate if it had been better to tune the different experiments such that the PBL top remained unchanged by changing the aerosols aloft, the same in all runs. I'm not saying this is the right way to go, but it should at least be discussed and this would be the minor revision.

We are not clear exactly what the reviewer is trying to say here. Indeed, a more “active” and persistent cloud will entrain aerosol more efficiently and contributes to its own persistence. In the absence of adequate aerosol to sustain the cloud in the boundary layer, we fail to see what is artificial about a separation of cloud-top from the tropospheric aerosol causing a negative feedback, as described in the response to the prior comment.

Detailed comments:
Figure 1: Maybe plot the height axis scaled to the main inversion base would make things clearer?
It's a little messy regardless of what we do, but we have adopted the reviewer's suggestion. This allowed us to remove the circles that marked the BL top.

Line 64: No black lines in figure, but I guess that would just make if messy? Maybe suggestion above would help?
The black lines are included, we have updated the wording to "The thin black line overlaid on the thicker, colored lines indicates the most likely location of a cloud layer" to be more clear.

Lines 66-67: Suggest "All aerosol profiles except that of 24 July 2020 (Fig. 1b) have higher ... inversion."
Changed as suggested.

Line 70: Drop "can"; you do, don't you?
There is no "can" on line 70 or even near line 70. We assessed all uses of the word "can" in the manuscript and removed two of them.

Lines 107-108: What do you mean by "ice is negligible"? Experience is that much of the precip from even from summer Sc is frozen, rather than drizzle, so in what way is it negligible.
It is negligible insofar as its concentration is low, even for a mixed-phase cloud, and we don't believe that its presence is impacting the qualitative results of our study.

Line 113: Again, change to "Surface heat fluxes were..."; surface moment flux is not zero!!
Fixed as suggested.

Lines 121-122: Inversions "5K per 100 m or stronger" seems to me to be on the strong side, looking at published studies. Its not unusual, but probably not "frequent" either.
The results from MOSAiC as reported in Jozef et al. (2023), which were the focus of this statement, show that this inversion strength is the most common during the summer months.

See Jozef et al. (2023) figure 3: The shallow (greens) and the thick, near-neutral (reds) mixed layers occur at similar frequencies. In both, the > 5K / 100m (dark green, dark red) are the most common.

Lines 152-153: The stable configuration is somewhat unrealistic, for three reasons: 1) Keeping the same surface temperature makes the whole PBL quite warm. It would seem more natural to instead lower the surface temperature and keeping the cloud layer as in control; 2) The structure with a deep quite stable layer is something that I've never seen. Instead the common structure would be two well mixed layers, one associated with the cloud and one with near-surface shear produced turbulence, separated by a shallow inversion; 3) Together, 1) & 2) makes the liquid water content unrealistically high.

Yes, we agree, inclusion of a very shallow mixed-layer at the surface would have made this profile more realistic albeit more complicated. Regardless, we have removed section 4.3 and so there is no need to revise this simulation.

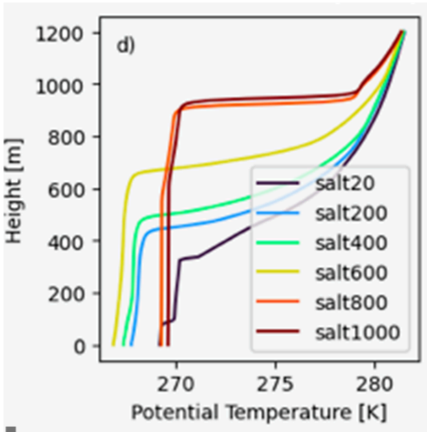
Lines 163: I suggest "the six aerosol sensitivity simulations", since there are more simulations coming.
Changed as suggested.

Lines a63-164: Suggest "appear to attain quasi-steady cloud tops", since this doesn't happen until after some 15h.
Changed as suggested.

Line 171: Do you mean "too small to be observed, or "too small to be measured"? These are not the same you know…
We have removed this descriptor and have kept the sentence to "which is essentially negligible".

Lines 177-179: This seems to contradict earlier statements, and previously published studies by the second author, that near-surface aerosol observations are not representative for the cloud layer
Agreed, in our simulations, despite the lack of surface fluxes, the entire layer from the surface to the cloud top remains well-mixed and as such the droplet and surface aerosol concentrations are similar. Also, as the reviewer has seen, our new simulations no longer show such large droplet concentrations near cloud top. It's not clear to us why that is the case. Regardless, the unrepresentativeness of the surface aerosol concentration is greatest when the cloud layer is decoupled from the surface, as is common in this region, and in such a case, we expect that the second author's previous conclusion would still hold.

Para beginning /w line 203: This needs to be stated carefully. Many of these simulations are essentially optically thin clouds, clouds that are not black bodies, at least large parts of the time. This is especially true for the low aerosol cases. But here these clouds are kept appearing in a well mixed PBL, in part by the initial conditions. In reality, without solar radiation the surface temperature would drop, and the clouds would become decoupled. But the set up here artificially prohibits this development by i) keeping the surface temperature fixed and ii) by the zero heat flux at the surface. One could argue that while "gray clouds" certainly are realistic in the Arctic, such clouds in a (relatively) deep well mixed PBL isn't.
Instead decoupling would occur, possibly with fog forming in the lower layer.
We disagree with the reviewer that the low aerosol case clouds are maintaining a well-mixed PBL. It is evident at hour 15 from the potential temperature profiles in Figure 6d that these cases are developing stable profiles below the cloud layer. This is even more evident at the end of the simulation which we include here. As such, we would argue that these simulations are evolving in the way the reviewer suggests they ought to evolve.



Para beginning /w line 235 & fig6: Variance of vertical velocity most certainly does not go to zero at the surface in reality; is this perhaps only the resolved-scale variance? If so that needs to be clear; scaled with friction velocity, this variance tends towards a fixed rather well-known value.
The reviewer is correct that this is resolved-scale variance. This has been clarified in the text.

Line 237: Drop "average" or rephrase; the average vertical wind is always close to zero, minus the subsidence. It is the strength of updrafts and downdrafts that become stronger or weaker, which is manifested in the variance, but the average vertical velocity remains (essentially) unaffected.
The word “average” was confusing in this case and has been removed.

Line 252: Choice of words; this has nothing to do with "success" or "failure". Maybe "efficiency"?
“Successful” changed to “Efficient”.