Review of "**The importance of Aitken mode aerosol particles for cloud sustenance in the summertime high Arctic: A simulation study supported by observational data**" by Bulatovic et al.

General comment

The authors have substantially improved the manuscript and have included the reviewers' suggestions. The discussion has been expanded and now better embeds the study in the existing literature. Similarly, the introduction has been restructured and additional relevant studies are cited. All figures have been improved according to the suggestions. I still have a few minor comments which should be addressed prior to final publication, which are listed below.

We thank the reviewer again for his/her careful reading and further comments.

Specific comments

Line 46: "cloud liquid growth" should rather be "cloud liquid water increase"

The sentence is now:" The turbulence further increases cloud liquid water as strong overturning means strong updrafts that allow efficient condensation of water vapor onto cloud droplets."

Line 47, line 50: "water vapor" instead of "vapor"

"vapor" has been changed with "water vapor".

Line 274: How are IWP and LWP defined (i.e. including or excluding precipitation)?

In both models the LWP and IWP include precipitation by definition. The observed LWP and IWP do also include precipitation. This has now been added in the caption of the Figure 2.

Line 293: consider changing "is better" with "performs better"

It is now "performs better".

Line 300: RAMS also produces vertical bands of increased rain throughout the cloud, e.g. at hours 8 and 9. Can you explain those? Again, is the autoconversion similar for both models? Or is the autoconversion rate especially high in RAMS? You show the condensational growth of raindrops, but (if available) it could be interesting to have a look at the conversion rate from cloud droplets to rain drops.

In RAMS, the available diagnostic rate is the sum of the autoconversion and the collection of cloud droplets by rain drops. This sum is higher in RAMS compared to MIMICA for the baseline simulation (please see attached figure). However, we cannot know whether the autoconversion rate or the collection rate has a stronger impact on the rain budget in RAMS and which rate contributes to the vertical bands observed in the qr profiles simulated by this model.
In the new version of the manuscript we have now added a sentence: "For the baseline simulations, the sum of the autoconversion rate and the collection rate of cloud droplets by rain drops is two orders of magnitude higher in RAMS than in MIMICA, which contributes to the higher rain mass mixing ratios in the RAMS sub-cloud layer.".

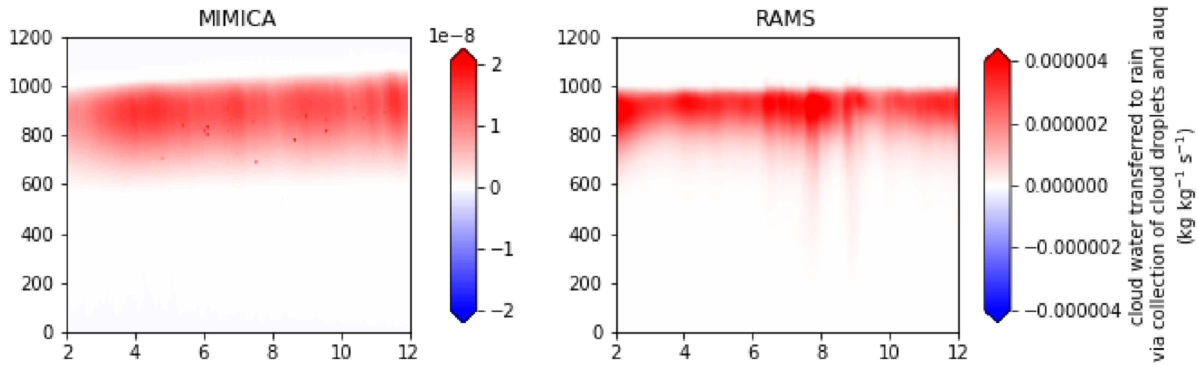

Figure 3: Thanks very much for including the cloud top and base height. However, how did you calculate cloud top and base height? Please add this information.

In both models, the cloud layer is considered to be present between altitudes where the cloud water mixing ratio (qc) is larger than $1 \cdot 10^{-6}$ kg/kg. This has now been added in the text: "In both models, the cloud base (cloud top) height is the altitude above (below) which the cloud droplet mixing ratio exceeds the value $1 \cdot 10^{-6}$ kg/kg.".

Line 367: "the cloud base altitude changes…"

This has been changed.

Line 411: "when no accumulation mode particles **are** present"

"are" has been added.

Line 439: "vary" instead of "very"

This has been corrected.

Line 452: "in the two model**s**"

"s" is added.

Line 456 ff: I appreciate the additional discussion of the radiative budget in the simulations. Did you also analyze the impact of SW radiation at the surface (as done in Figure 10 for LW radiation)? I would assume that especially (and most likely only) in summer there might also be an impact. Or did you only investigate SW radiation at the model top, as written in line 461? Also, did you look at the **net** surface LW and SW radiation? Overall it would be interesting to see, how the **net** surface energy balance changes for the inclusion of Aitken mode particles and how these results could be extrapolated to the summertime high Arctic radiative budget.

We thank the reviewer for this point. The reason why we have investigated the Aitken mode influence on the top of the model (TOM) SW radiation only is that we have considered that the cloud impact (i.e. different concentrations of Aitken mode particles) would be the most pronounced on the reflected component of the SW radiation. We have now also investigated the Aitken mode influence on (net) SW radiation at the surface and it is *not* significant.

The sentence has been changed to: “Both models simulate no significant influence of Aitken mode particles on the SW radiation, consistent with the low insolation (not shown).”, i.e. we have removed the part ”at the top of the model domains”.
We have also looked at the net LW surface radiation fluxes and they show the same result as the corresponding downward fluxes. The net flux is the difference between downward and upward flux, and the later one only depends on the surface emissivity and the surface temperature - and these two parameters are prescribed in both models so they do not vary between the simulations.

Line 681: Apart from the choice of prescribed/prognostic ICNC/INPs, secondary ice formation may also play a relevant role in determining the cloud evolution, especially in summertime Arctic MPCs (Sotiropoulou et al., 2020; doi: 10.5194/acp-20-1301-2020) and at the temperatures shown in Figure 1. However, this is not mentioned at all. Are secondary ice processes omitted (in the first version of the manuscript I read they are)? Does this also introduce additional uncertainties or effects (i.e. a potentially higher ice fraction and a decreased influence of Aitken mode particles)? This could also be mentioned in e.g. Section 5.2.

This is a good point. Secondary ice processes are not considered in the present study and could obviously have an impact on the total ice fraction simulated by the model. In the section 5.2., the last sentence is now: ”The results most likely depend on whether the ice crystal concentrations are prognostic or prescribed and if secondary ice processes are considered in the calculations of the ice crystal number concentration (Sotiropoulou et al., 2020).”.