Reply to referee's #2 comments on manuscript:

Interactive comment on "The importance of Aitken mode aerosol particles for cloud sustenance in the summertime high Arctic: A simulation study supported by observational data" by Ines Bulatovic et al.
Anonymous Referee #2
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General comments:

The authors employed two numerical models to perform a series of simulations to study the importance of Aitken mode aerosol particles for cloud sustenance in the summertime high Arctic. The messages in the abstract seem to be clear. After reading through the main text of the paper, I find some interesting results. But I am also confused by arbitrary model and simulation configurations and overwhelmed by poorly interpreted, disorganized, and probably unnecessary results. Throughout the manuscript, the authors used a lot of speculations in their reasoning where solid evidences are expected. The writing also has huge room for improvement. I listed my major and minor concerns as well as suggestions regarding the technical aspects of the manuscript below.

We thank the referee for his/her comments that have helped us improve and clarify the manuscript.

Both models utilized in the study have previously been used for simulating Arctic mixed-phase clouds, which is now explicitly stated in the manuscript (Sect. 2.1). We have on purpose used the default configurations of the two models, as this is what typically would be used for e.g. a model-observation comparison/evaluation.

We have answered the comments by the reviewer in a point-by-point fashion, revised the manuscript accordingly and made a thorough effort to provide a clearer and more organized interpretation of the results.

One of the co-authors on the study is an English native speaker who has carefully read the manuscript and paid attention to linguistic mistakes.

Specific comments (major):

If I understand correctly, the main idea of this paper is that, for some combinations of model configurations (i.e., Aitken mode and accumulation mode aerosol number concentration, aerosol kappa value, and ice number concentration), the modeled cloud can survive through 12 h, meaning that during this period, the clouds can maintain sufficient liquid water against processes that depletes liquid water (like subsidence, entrainment of warm air, losing moisture due to glaciation and precipitation, so on) and generate enough supersaturation to activate the prescribed Aitken mode aerosol. Was the specified aerosol size distribution for each simulation used from the beginning of the simulation? How important are aerosols during the spin-up? In other words, are the differences among simulations using the same model and between MIMICA and RAMS due to the activation behavior when the turbulent motion is very weak? What if all simulations (in each model) begin with a robust cloud and fully-developed the turbulence, spun-up using same configurations, and then switch to different aerosol number concentrations? I don't think a juicy but non-turbulent cloud is a realistic starting point to test aerosols' impacts on sustaining clouds or produce results that are relevant to the real world. Please justify this choice.

Both models use an initial cloud droplet number concentration of 30 cm^{-3}. This is now clarified in the manuscript (Sect. 2.3). In other words, both models start with a fully developed cloud, which thereafter is maintained (or not) based on the prescribed aerosol size distributions and modelled supersaturations. We do believe this type of model setup is realistic as it roughly represents conditions where a cloud forms close to the marginal sea ice zone and thereafter is advected in over the sea ice (where aerosol sources are either absent or small).

A few other questions related to initialization and spin-up: Is the initial cloud size distribution related to the Aitken mode and accumulation mode aerosol? For each model, are the liquid water content profiles in all simulations identical at the beginning? Are all microphysical processes (e.g., all processes related to precipitations) turned on from the beginning?

The profile of cloud water mixing ratio is the same in both models at the beginning of all simulations as it is stated in the manuscript. All microphysical processes are active at the beginning of the simulations, which is now also stated (please see Sect. 2.3).

It is worthwhile to be more specific about the activation of aerosol particles in MIMICA and RAMS as configured for this study. It seems that the activation scheme in MIMICA is identical to the one used to generate Fig. A10. What about in RAMS? Are the ss in Fig. 10 and Fig. 11 same as those used in the activation scheme in the models?

Figure A10 (now Fig. A12) shows theoretical critical supersaturation values as a function of particle dry diameter for a range of kappa values, where kappa is a measure of the hygroscopicity of a multi-component or single-component aerosol particle (Petters and Kreidenweis, 2007). The activation schemes in both models use this theory for cloud condensation nuclei activation as stated in the Sect. 2.1. The reason why look-up tables are used in RAMS is only due to computational efficiency.
The supersaturation (ss) statistics shown in Fig. 10 and Fig. 11 (now Fig. 14 and Fig. 15) are directly derived from the model output, i.e. the ss values shown are indeed used to activate aerosols in the two models. The critical dry diameters shown in the figures are calculated for the ss values (i.e. for 75th and 99th ss percentiles) obtained from the models, based on the relationship presented in figure A12 and explained in detail in Petters and Kreidenweis (2007). We have not made any changes in the manuscript as all this information was already available.

It seems that the authors tried to use an observed sounding to set up the baseline simulations, and then perform sensitivity tests on top of that. However, the authors did not provide enough details (for example, dedicated figures) for readers to understand the case. Please consider showing some details. I found the ASCOS sounding available from https://bolin.su.se/data/ascos-radiosoundings. Did the authors use the original 0535 UTC 31 August 2008 sounding from this archive? Or an idealized version of it? The authors used the observed CCN to justify the use of AC20_AK20 as the baseline simulation. However, the cloud layer in the aforementioned sounding seems to be decoupled from the surface. If this is the case, does it still make sense to use surface measurement to determine the base case? The authors mentioned a few times that Arctic stratocumulus may entrain moist air from above the cloud top, but provided no evidence. (Whether other studies showed entrainment of moisture from above the cloud could happen is irrelevant to this study.) Initial sounding (together with profiles from the middle of the simulations) can be used to show whether there is moist layer above the cloud top.

We agree with the reviewer that it is useful to show the radiosonde observations and have therefore added the new Figure 1. We have indeed used the original soundings from 0535 UTC 31 August 2008, as is stated in the manuscript. In Figure 1, we also display the simulated profiles after 6h of simulation. Showing the initial profiles does not make sense as these are the same as the radiosonde observations.

There were no other CCN measurements available from the ASCOS campaign apart from those obtained from a CCN counter that was situated on *Oden*. However, we do agree with the reviewer that the CCN concentrations could be different within (or just below) the cloud due to the decoupled boundary layer. The sentence: "Since the surface boundary layer typically was decoupled from the turbulent layer associated with a cloud (Tjernström et al., 2012), it is however not certain if the CCN concentrations measured at the ship were representative for the cloud layer (cf. also observed vertical profiles of particle concentrations in Igel et al., 2017, Figure 1)." has been added in Section 2.2.

Initial profiles of absolute temperature, potential temperature and specific humidity (Figure 1) show that the simulated case was characterized by both a temperature and humidity inversion.

Choice of model configurations, shared simulation setup, and experiment design are perplexing and arbitrary. Why were the vertical resolutions different, especially as the authors suspected that the different vertical resolutions may be the source of some discrepancies between the simulation results from the two models (e.g., L254). Why were the microphysics in RAMS so much more complicated (even with hail turned on) than in MIMICA? Why was aggregation turned off due to low ice number concentration, but why only turned off for MIMICA? Why was MIMICA used for ice number concentration sensitivity test while certain ice-related budget terms are not available from it (L289)?

As we briefly mentioned above, we wanted to use the models in their standard configurations and examine the similarities/differences in the simulated results. The default setup in RAMS is a fixed vertical grid spacing while in MIMICA the default grid spacing is variable. We agree that the difference in grid spacing could be a source of discrepancies and have therefore run an additional MIMICA baseline simulation with a fixed vertical grid spacing of 10 m as in RAMS. The test did not show any significant differences in the simulated cloud microphysical properties compared to the MIMICA baseline case with a variable grid spacing. This is now explained in the new version of the manuscript (Sect. 2.3).

It is not completely clear to us why the reviewer considers that the microphysics in RAMS is much more complicated compared to MIMICA. We agree that the inclusion of hail could change the microphysical rates for e.g. a deep convective cloud, but for an Arctic stratocumulus cloud the hail production is in general very small. Indeed, in all RAMS simulations 97.9-99.7% of the ice is present as ice crystals so the riming treatment plays a minor role in the simulations. We have also checked the contribution of hail to the surface precipitation. It is 2 orders of magnitude smaller than the contribution from rain. This information is now added in the manuscript (please see Sect. 2.3). The aggregation process is actually switched on in both models, the statement that it was turned off in MIMICA was a mistake and has been corrected.

We agree with the reviewer that the ice tests could have been performed with both models and have now made the corresponding simulations. We present results from both MIMICA and RAMS in the new version of the manuscript and we find that the results are consistent.

Much of the results regarding rain and ice are only superficially described and discussed, with no obvious connection to the main goal of the paper, and sometimes contain errors. A few examples are provided here.

L277: "The pockets of condensation and evaporation present in the main cloud layer are well-correlated with updrafts and downdrafts and they tend to cancel each other in the mean. This is why the average condensation rate in the main cloud is of the same order of magnitude as the one in the sub-cloud layer." Totally lost.

We have clarified the sentence. RAMS produces a sub-cloud condensation layer (as Fig. 4b shows) and the sentence explains why this layer is present and so pronounced. The relatively high mean value is a result of infrequent but strong condensation due to sub-cloud convection simulated in RAMS.

L415: "The presence of both positive and negative differences with time is a result of differences in cloud dynamics with different distributions of updrafts and downdrafts with time that govern the rain production in the cloud (cf. Fig. A9)." But there is nothing about "distributions with time" in Fig. A9.

We agree with the reviewer that the Fig. A9 (Now Fig. A11) was not a good choice to refer to. We now instead refer to Fig. A8, which shows that there are different distributions of updrafts and downdrafts with time (Fig. A8b,c). The figure also shows that the temporal evolution of updrafts and downdrafts is well correlated with the evolution of the collection rate of rain drops by graupel (Fig. A8a) and therefore also with the rain budget. Results are only presented for the baseline simulation, but the results are qualitatively the same for all simulations.

L421: "change in the Aitken mode particle number concentration results in that maximum updrafts are reached at somewhat different times", what is the significance of this?

The sentence has been rephrased in the new version of the manuscript.

L422: “Differences are in general greater in MIMICA than in RAMS since there is a slightly more total ice in MIMICA”. Does “since” mean “because” here? Does greater total ice water content (or path?) have to lead to greater differences? Other than the “entrainment of moisture aloft”, here are a few additional examples of unacceptable speculation.

We have now changed the reasoning for the observed difference in the Aitken mode influence on total ice mixing ratio between the two models. Previous studies have shown that a pronounced CCN influence on ice in CCN-perturbed clouds can be related to stronger LW cloud top cooling (e.g., Possner et al., 2017, Solomon et al., 2018, Eirund et al., 2019), which is present in MIMICA and not in RAMS. Please see the Subsection 3.3.1 for more details.

L288-289, “Examining the total ice deposition/sublimation rates would most likely lead to similar rates between the two models”. (BTW, “The ice crystal deposition and sublimation rates are higher in RAMS than in MIMICA since the two models partition the total ice deposition differently among ice hydrometeor categories” is also just speculation, isn’t it?)

The sentence in the brackets is not a speculation. We have indeed analysed the (available) deposition rates for all ice species in both models and concluded that they differ between the same ice hydrometeor categories in the two models. The sentence has been reformulated to:” The ice crystal deposition and sublimation rates are higher in RAMS than in MIMICA since the two models partition the total ice deposition differently among ice hydrometeor categories **(not shown)**.”.
“Examining the total ice deposition/sublimation rates would most likely lead to similar rates between the two models” is on the other hand something that we cannot be sure about and we therefore removed this sentence from the new version of the manuscript.

L411-412: “an increase in the Aitken mode particle concentration may lead to stronger turbulence and more cloud liquid water production”. Turbulence intensity and cloud liquid water budget can be diagnosed. It does not make sense to speculate (“may lead to”).

This is a good suggestion and we have now included a figure of the time-mean, resolved TKE within the cloud layer (Fig. A6). In both models, the TKE is higher in cases with a higher number of Aitken mode particles (comparing the pairs of simulations with the same number of accumulation mode particles). The relation between the cloud-averaged TKE and cloud water mixing ratio is obtained by comparing Fig. 5 and Fig. A6. The text has been changed accordingly.

Minor comments:

Contents in Sections 2.1 and 2.3 are not clearly separately. For example, why are number of model grid points and resolution introduced in Section 2.1 but domain size in Section 2.3?

We agree that it is more suitable to provide this information in the *Simulation setup* section and have moved it there. Otherwise, we believe that the contents of Sections 2.1 and 2.3 are clearly separated.

Please clarify a few model or simulation configurations issues: Is it correct that the time step is 2 s for MIMICA? What about RAMS? The terminal speed is only introduced for MIMICA, what about RAMS? What are the references for the terminal speed formulas used? Is the 0.2 L-1 ice number concentration based on ASCOS observations? Need reference. Why is divergence set to 1.5E-6 s-1? Is the same value used for all model layers?

It is correct that the time step in MIMICA is ~2s as stated in the manuscript. In RAMS, the time step is 1.5s and this is now explained (Sect. 2.1).

Information about the terminal fall speed in RAMS as well as the references for both models have also been added (Sect. 2.1).

We have added the Stevens et al. (2018) study as a reference for the ice crystal number concentration (Sect. 2.3).

In both models, the large-scale divergence is constant in the whole model domain (at all model levels) and the value is chosen to produce a stable cloud layer (cf. Stevens et al., 2018). The explanation together with the reference has now been added in the text (Sect. 2.3).

L130: Is the wet deposition calculated for any tracers in either model?

No, this has not been included as stated in the Section 2.3 (i.e. the aerosol size distributions are prescribed).

L346: Since they are simplified LW cooling calculation that only depends on LWC, why not using same formula for both MIMICA and RAMS?

As mentioned before, we wanted to use models with their standard configurations. Using two different models allows us to evaluate if the results are dependent on the details of a specific model or if we can draw more general conclusions. The additional tests that we have done with e.g. the simplified LW cooling calculations (in MIMICA and RAMS) or a fixed vertical grid spacing (in MIMICA) are meant to indicate which differences in the model configurations lead to the discrepancy in the simulated results. We think this information could be useful for the future modelling studies.

L349: "Stable cloud base", do you mean "steady"?

This sentence has now been removed from other reasons.

L383: "Fig. 5", should be Fig. 6?

Yes, this was a mistake in typing. However, this paragraph has been modified in the new version of the manuscript and the sentence has been removed.

Examples of poor writing:
L265: "total ice", do you mean "total ice mixing ratio"? There are a few "total ice" throughout the text.

We agree that “total ice mixing ratio” is more precise than “total ice” and have changed it in that place. However, we did not find any other example of incorrect use of the “total ice” term throughout the text. For instance, in the sentence: “The ice crystal deposition and sublimation rates are higher in RAMS than in MIMICA since the two models partition **the total ice deposition** differently among ice hydrometeor categories (not shown).” or in the sentence: “Examining **the total ice deposition/sublimation rates** would most likely lead to similar rates between the two models, but these rates are not available in MIMICA”, the term “total ice” is used as a possessive adjective and not as a noun. The models partition the (total) ice differently and have different (total) ice deposition/sublimation rates**.** The simulated (total) ice is then expressed with a mixing ratio variable.

L377: “available water vapor” what is this?

We have modified the sentence to: ”A higher number of cloud droplets decreases the maximum supersaturation and the amount of water vapor in the cloud available for activation of smaller particles.”

Suggestions on technical issues:

Please consider marking cloud top and base whenever relevant.

We have added the cloud top and cloud base heights in all figures where it could be done.

Colors are saturated in some figures, e.g., Fig. 3f. Please adjust.

We have adjusted it.

L292: “from the different model descriptions”, should be “configurations”?

“model descriptions” has been changed with “model configurations”.