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Comment on acp-2021-888

Anonymous Referee #1

Referee comment on "Aerosol impacts on the entrainment efficiency of Arctic mixed-phase convection in a simulated air mass over open water" by Jan Chylik et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-888-RC1, 2021

Review of a manuscript "Aerosol-cloud-turbulence interactions in well-coupled Arctic boundary layers over open water" by Jan Chylik, Dmitry Chechin, Regis Dupuy, Birte S. Kulla, Christof Lüpkes, Stephan Mertes, Mario Mech, and Roel A. J. Neggers, considered for publication in ACP.

Overall recommendation: publish after major revisions.

This manuscript discusses high-resolution (LES) numerical simulations of an Arctic cloud system observed during the ACLOUD campaign. Overall, I see significant problems with the manuscript and feel considerable revisions are needed before the paper is accepted. Overall, I feel the paper does not present any scientific hypothesis, and the main goal seems to be to demonstrate that the model is capable in reproducing observed cloud features. If this is indeed the case, the presentation does not reach that goal. Below I present my main points and then follow with numerous specific line-by-line comments. Addressing all those should lead to a publishable manuscript.

General (major) comments

- Manuscript title should have "modeling" word in it. Perhaps start with "Modeling of aerosol-cloud…". That said, aerosol-cloud interaction aspect of the presentation is questionable to me, see 3 and 4 below.
- Figure 1 in the manuscript documents presence of complex cloud systems in the area. Is there anything there that resembles the cloud patch simulated by the model? Perhaps a couple figures showing how clouds evolved in time (to ensure the way they are simulated makes sense) would be useful. If this is not possible (i.e., not possible to show that similar structures persisting for a few days), then what is the general motivation behind LES simulations? I understand that having a periodic relatively small LES domain imposes stringent limitations, but these must be at least mentioned when

compared natural clouds and simulated virtual reality.

- I have serious doubts about the mixed-phased microphysical scheme used in the simulation. For warm rain, the scheme applies saturation adjustment. What are simulated and observed droplet concentrations? I assume there are some aircraft observations to consider for simulation validation. For ice, how important is the saturation adjustment that warm rain scheme uses? For instance, sizes of cloud droplets affect ice initiation. Are ice concentrations in general agreement with aircraft observations? Only mass of water and ice are shown in the paper. Also, satellite retrievals should be helpful here as well. Maybe even partitioning between liquid and ice can be obtained from the satellite. Overall, there are much better double-moment mixed-phased schemes available today (not to mention bin microphysics) that should allow more confident simulation of microphysical process. Limitations of the scheme applied in this study needs to be at least mentioned in the manuscript.
- Following 3 above, it is unclear how CCN and IN are prescribed and how they evolve in the simulation. This is important for specific results, see specific comments below.
- When thinking about the simulated case, I wonder to what extent presence of ice in the system is important. From the basic dynamics point of view, I think ice does not matter. Maybe it does for the radiative transfer, but this is not obvious. Such thinking might be along the lines of a hypothesis that modeling can address. That said, my intention is *not* to ask the authors to run a simulation without ice, but just to suggest something for a future investigation.
- I find the technical quality of the presentation relatively low. Some figures require adjustments to make them legible when included in the printed version. See specific examples below. Some statements in the text require revisions, again, see specific examples below. Overall, the conclusion section is relatively short and perhaps reflects on the rather "thin" outcome of this study, at least as discussed in the current version.

Specific minor comments

- L. 48: "primitive equations" are typically referred to as the set of mesoscale hydrostatically-balanced equations. Please rephrase.
- L. 127: "…our main objective is to study interactions….". This does not seem a correct statement. Yes, there is a simulation, but the interactions are not studied. And I am not sure what would be needed to make this statement correct. Sensitivity simulations with different CCN and IN concentrations? Different ice initiation mechanisms? More convincing comparison with observations?
- Figure 4. Because the distributions overlay, I think it would be better to plot distributions in two panels. Also, is the information about solubility available (i.e., do all can serve as CCN?). Why such a large difference for aerosols above and below cloud layer? Large-scale advection (for above) and surface emission (for below)?
- L. 172. "upwind scheme". Really? I do not think so. If this is correct, then this is a serious problem.
- Section 3.2. Is there any justification for specific parameter values selected for the simulation? CCN and IN concentrations in particular. Is the way ice is initiated in the model justified?
- L. 238. I assume there is longwave radiative transfer as well. Please mention details.
- L. 251: "Radiation is interactive…" is an incorrect statement. I think this refers to the fact that all hydrometeors are seen by the radiation code. Perhaps some details would be need here.
- L. 256/257. "Continues nudging" and "Newtonian relaxation". Please use one term to avoid confusion. Also, such an approach dumps the perturbations, correct? Within the boundary layer, one can still relax the *mean* towards the observations (i.e., no dumping small-scale perturbations), but I think this not used, correct?
- L. 265: "CCN … can evolve freely". Really? This means that with precipitation the CCN

(and perhaps IN) are being removed. The text further in the draft confirms that. Is this a fair model setup, that is, with changing in time CCN? Should the concentration be maintained? What impact the reduced CCN later in the simulation has? I would expect some differences, for instance, in the rain mass.

- Fig. 4 should include panel boundaries. The text to the right of each panel is too small and will be even smaller in print. Please change. L. 288 mentions diurnal cycle. I do not see it in the panels.
- L. 303. "… simulated profiles agree well with observations…". The relaxation ensures that, at least for some variables, correct?
- Fig. 9. The text explains how the figure is created for the model output. What about the observations? What is the difference between mirac and lidar? Can they distinguish water from ice? Overall, the figure rises more questions and it suggests a poor comparison. What is the reason for showing it?
- Fig. 10. As in 12 above: the comparison is poor. I understand that it is difficult to compare model output averaged in time with a small number of aircraft legs and a few heights, but what do we learn from the figure? And why only mass? I would think considering particle concentrations and sizes would be more informative (e.g., per my points above).
- Fig. 11. Please include panel boundaries. Zero line would help in panel c. How do data from the aircraft are processed (i.e., averaged over what distance)? Do the figure (and Figs. 9 and 10) allow meaningful comparison between model and observations? I am not sure. More details are needed.
- L. 366. Please provide some references in support "textbook" boundary layer and cloud structures.
- Section 5.1, Fig. 12 needs panel boundaries. L. 392: should it be "left to right"? Fig. 13: labels on the color scale are too small. Looking at the panels, the question about the domain size comes to mind. Are there any satellite observations that would support the simulated presence of mostly water in the updraft region and ice at the peripheries? Does that partitioning depend on microphysical scheme assumptions?
- Fig. 14 and reference to Paluch (1979). The analysis is strictly valid for nonprecipitating clouds, correct? Does that explain larger scatter of data points in the upper part (i.e., lower right corner of the left panel)? L. 413 and eq. 1: The liquid-ice potential temperature (as well as liquid water potential temperature) is not conserved when precipitation is present. L. 427: radiative cooling and fallout of precipitation. The caption to Fig. 14: "at a selected level and time point": please provide details. Does the figure change if a different level and time is selected? Similar comments apply to Figs. 15 and 16. Please explain.
- Figs 15 and 16. Please explain the height the data come from. Overall, how Figs. 14 – 16 are created should be explained. Symbols and number along the axes as well as symbols inside should be larger. Besides just discussing what the figures show, what are physical outcomes of the analysis? I feel the discussion is quite thin in that respect. Some of the points made in section 5.4 are difficult to follow without details on how aerosols are treated in the microphysics scheme.
- With all the comments above, I have not read sections 6 and 7.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-888/acp-2021-888-RC1-supplement.pdf