Review of " Relaxation times of Arctic mixed phase clouds to short-term aerosol perturbations under different surface forcing" by Gesa K. Eirund, Anna Possner and Ulrike Lohmann

This manuscript deals with the impact of short-term aerosol perturbations on Arctic mixed-phase clouds (MPCs). Based on an observed case from the ACCACIA campaign, the authors use large-eddy simulation to investigate how the structure and microphysical properties of the modelled MPC change with changing cloud condensation nuclei (CCN) and ice nucleating particle (INP) concentrations, changing surface fluxes (characteristic for a surface covered with ice or open water) and air temperature. The manuscript is in general well written and contains interesting results. However, there are a few points that I think should be clarified before the manuscript is accepted for publication.

Major comments:

1. Introduction: It is not clear to me *why* a short-term aerosol emission perturbation should generate a *long-term* cloud response. Since (accumulation mode) aerosols are efficiently scavenged by precipitation, I don't see a clear reason why the response would last longer than a few hours if you have precipitating clouds? I was actually quite surprised that you did indeed see a quite pronounced response up to almost a day after the perturbation. I think that a motivation would be good to include in the introduction.
2. Model description and setup: The model description and simulation setup is incomplete. I would suggest adding information on:
    a. Which hydrometeors are considered? Is it only one category for liquid and one for ice or do you have separate categories for cloud droplets, rain drops, pristine ice, graupel/hail, snow,….? And what is really q_c and q_i?
    b. Are any secondary ice formation processes represented in the model?
    c. At which altitude is the model top?
    d. At which latitude is the case simulated?
    e. What is the time step and how long is the integration time?
    f. What is the assumed habit of the ice crystals?
    g. Is the sounding performed over ice or open water?
    Related to the last comment, I'm not convinced that it's appropriate to use the same sounding to initialize the "open water" and "ice surface" simulation. I would assume that a sounding over ice looks quite different to a sounding over water?
3. Evaluation of background state:
    a. The definition of the cloud extent is confusing (but it may become clearer if point 2a above is clarified). Does the limit of q_c applied to define the cloud boundary include rain? In Section 3, you say that "Our model successfully simulates a liquid-topped MPC with ice sedimenting out of the liquid layer in both control simulations, according to observations". But this structure is not really clear from Figure 4. Looking at figure 4, I seems like you have a cloud between approximately 600 and 1500m (in the ocean case) and that the rest is falling precipitation?
    The cloud illustrated in Figure 12 is also quite different from the clouds plotted in Figure 4, the simulated cloud over the ice surface is not substantially thinner than the cloud over ocean (at least not according to the values in Table 2).
    b. Droplet number concentration: It is not clear to me how you could possibly get a droplet number concentration of 63 or 110 cm^{-3} (as observed over ocean and ice,

respectively) in your simulations if you have a CCN concentration that is only 49 cm^{-3}. On the other hand, the CCN concentration plotted in Figure 2b is approximately 100 cm^{-3} at ~2000 m. Why is the concentration so high at his altitude? This seems to be much higher than the cloud top, so I don't think it could be transported CCN?

4. Robustness to perturbations in microphysics: Figure 6 shows that the LWP increase with increasing CCN concentration is more sustained over ocean than over ice. The authors also discuss this result on page 11-12, but I cannot really find an explanation/hypothesis to why this is the case? And, as mentioned above in comment 1, I was actually surprised to see that the CCN perturbation response was sustained for such a long time after the initial perturbation – in particular for the open ocean case.
5. Discussion:
    a. The simulated transport of CCN out of the cloud layer is interesting, but also a bit puzzling to me. In another Arctic mixed-phase cloud study, Igel et al. (2017) found that entrainment/mixing *from* the free troposphere could actually be an efficient *source* of aerosols/CCN to the mixed-phase cloud layer. I think this would be worth mentioning/discussing. I'm not sure why you (and Solomon et al., 2018) get a different response, but one possibility could be that the cloud simulated by Igel et al. extended into the inversion, and that the inversion layer and lower free troposphere was actually moister than the below-cloud layer. Another possibility could be that Igel et al., impose a vertical gradient in their aerosol concentrations (based on observations). In general, I think it would be interesting to see how efficient the CCN recycling is in your study.
    b. Another thing (related to the above point) that would be interesting to know is how well COSMO-LES simulates the entrainment processes at the cloud top. Do you have any observations of TKE dissipation from ACCACIA that you could compare with?
    c. In general, the authors could extend and contrast their results to other studies on aerosol effects on mixed-phase clouds. The increase in ice water content and subsequent decrease in liquid water content with increasing INP has been described by e.g. Avramov and Harrington (2010), Ovchinnikov et al. (2014), Young et al. (2017), Stevens et al. (2018). The increase in liquid water path with increasing CCN has been discussed in Stevens et al. (2018)

Minor comments:

Abstract:

1. Although it's mentioned in the title, I think it should be clarified also in the abstract that you are looking at instantaneous aerosol perturbations.
2. The sentence starting with "Motivated by ongoing sea ice retreat…" does not read very well. It's not clear what you contrast with what and that it is model simulations you are referring to.

Introduction:

3. Page 2, lines 4-6: Deep convective clouds are also mixed-phase.
4. Page 2, line 10: "… potentially causing a warming effect…" – why potentially? Isn't the LW (surface) effect always warming?
5. Page 2, line 15: "… eventually accelerating…" – why accelerating? Isn't it also possible that we could have a negative feedback from clouds?

6. Page 2, lines 25-29: This sentence does not read very well.
7. Page 2, line 33: Why would sea salt and dimethyl emissions dominate ship emissions? Do you mean that these (sea salt and DMS) emissions generally are larger than ship emissions?

Model description and setup:

8. Page 3, line 30: "km" should be "km^2".

Evaluation of background state:

9. Page 5, line 3: I would specify that "both control simulations" refer to the control simulations over open water and ice, respectively (and thereby define *ocean_control* and *ice_control*).
10. Page 5, line 9: "… lower in our model simulations". Lower than what? I assume you mean compared to observations?
11. Figure 1: Perhaps refer to table 2 for the simulations?
12. Table 2: "Cloud extend" should be "cloud extent".

Surface flux impact on cloud dynamics:

13. Figure 3: What time step is plotted?
14. Page 6, line 5: "Cloud extend" should be "cloud extent".

Invariance of results across temperature regimes:

15. Page 13, line 10: I would suggest adding that the RH is kept constant, just as a clarification.

Consistent response independent of perturbation injection period:

16. Page 15, line 9: I would suggest adding "substantial" in between "no" and "change".

Discussion:

17. Page 16, line 15: What is τ?
18. Page 17, line 4: Define WRF?
19. Page 19, line 15: I suggest changing "resembling" to "providing" or something similar.

Reference

Avramov, A. and Harrington, J. Y.: Influence of parameterized ice habit on simulated mixed phase Arctic clouds, J. Geophys. Res., 115, 1–14, https://doi.org/10.1029/2009JD012108, 2010.

Igel, A. L., A. M. L. Ekman, C. Leck, M. Tjernström, J. Savre, and J. Sedlar (2017), The free troposphere as a potential source of arctic boundary layer aerosol particles, Geophys. Res. Lett., 44, 7053–7060, doi:10.1002/ 2017GL073808.

Ovchinnikov, M., Ackerman, A. S., Avramov, A., Cheng, A., Fan, J., Fridlind, A. M., Ghan, S., Harrington, J., Hoose, C., Korolev, A., McFarquhar, G. M., Morrison, H., Paukert, M., Savre, J., Shipway, B. J., Shupe, M. D., Solomon, A., and Sulia, K.: Intercomparison of large-eddy simulations of Arctic mixed-phase clouds: Importance of ice size distribution assumptions, J. Adv. Model. Earth Sy., 6, 223–248, https://doi.org/10.1002/2013MS000282, 2014.

Young, G., Connolly, P. J., Jones, H. M., and Choularton, T. W.: Microphysical sensitivity of coupled springtime Arctic stratocumulus to modelled primary ice over the ice pack, marginal ice, and ocean, Atmos. Chem. Phys., 17, 4209–4227, https://doi.org/10.5194/acp-17-4209-2017, 2017.