Comments on “The impact of CCN concentrations on the thermodynamic and turbulent state of Arctic mixed-phase clouds”

This manuscript is trying to explore the impact of CCN concentration on the development of low-level mixed-phase clouds during the spring season in the Arctic. Data from the recent ACLOUD field campaign are valuable and are used to initialize the setup for the demi-Lagrangian Large Eddy Simulations. Sensitivities modeling studies are performed for two scenarios to study the effect of CCN concentration on mixed-phase clouds, precipitation, and turbulence. Aerosol-cloud interaction in the mixed-phase clouds is very important but is still far from clear. Therefore, this modeling work along with the unique observational dataset might help to bridge the gap in this field. However, the current version is not well written. I find a bunch of typos and grammatical errors in the manuscript. In addition, I find two “main” conclusions in this paper controversial: (1) A lower CCN concentration results to a faster glaciation of the cloud due to stronger precipitation (which contradicts the general idea of cloud invigoration); and (2) The increased amount of ice sublimation will increase TKE in the boundary layer (which contradicts the idea of PBL stabilization due to sublimation). Please find my major comments below, especially 1,3,7. I’m not saying that what most people, maybe just me, believe is correct. Results are interesting here, but I’m not fully convinced by the explanation. I am also confused by some results. I think the main reason is that lots of details of the model setup are not clearly described. For example, ice microphysical scheme, which is very important to the mixed-phase clouds, is not discussed at all. Without those details, it is difficult for me to understand the results and their interpolations. Therefore, I suggest at least a major revision for this manuscript.

Major comments:

1. One of the main conclusions, as stated in the abstract is that “A lower initial CCN concentration generally results into a faster glaciation of the cloud, leading to faster removal of the cloud water, and also affects the vertical structure of turbulence.” I think this conclusion might lead to misunderstanding. Based on theory, e.g., Korolev and Isaac (2006), the glaciation process is very sensitive to the ice number concentration in the mixed-phase clouds, which has been confirmed in many previous LES studies (Fridlind et al, 2002; Ovchinnikov et al., 2014; Fu et al., 2019… already cited in the manuscript). However, the link between CCN concentration and ice nucleating particle (INP) concentration is still unclear. “A lower initial CCN concentration” corresponds to a lower INP concentration or same INP concentration? I think results are very sensitive to how the model takes care of the ice formation process for a lower CCN concentration, which is not clear in the abstract. For an extreme condition, if INP concentration decreases significantly faster than CCN concentration, it might lead to a condition that only a few ice particles exist in the mixed-phase clouds, and therefore, the glaciation will be slower than before.

2. Line 79: “on relatively short timescales”. How short the timescale is? Within an hour? A few hours? One day? A few days? Please state it clearly.

3. Section 3.5: It is not clear to me how the microphysical scheme deals with ice nucleation? I see later that in section 5.2 (line 493): “Perhaps the main simplification of our study was applying a simple parameterization of Reisner et al. (1998) for ice nucleation.” I think it is too late to make such a statement. In addition, I think it is not good enough to just cite Reisner et al. (1998). Ice nucleation is crucial to ice number concentration in the mixed-phase clouds. Ice number concentration is one of the most important variables that affect the glaciation process (see my comment 1). Please clearly state that how ice particles are formed in the model: from supercooled cloud droplets? from ice nucleating particles? Please clearly state that what controls ice number concentration in the model: fixed number concentration? diagnostic value? prognostic value? Any other process related to ice microphysics: rimming? collision? secondary ice production?... The goal is to make it clear in the manuscript that (1) how ice particles are formed in the model? (2) What controls the ice size and number concentration? (3) and most importantly, when changing CCN concentration in the model, will it also change ice nucleation and ice number concentration?

4. Figure 6: If I understand correctly, DS08 is used to adjust the initial condition for the model setup for RF05, and DS04 is used to adjust the initial condition for the model setup for RF20. If so, it is not surprising to me that those vertical profiles in Figure 6 are close to the sounding. It will be more interesting for me to see the comparison between model and observations at a later time, e.g., DS01 for RF05.

5. Another question is related to large-scale forcing (discussed in section 3). Please clearly state how large-scale forcing is taken care of in the model: the forcing profile is from ECMWF forecast? Which variable is forced: wind speed? temperature? water vapor? What is the forcing time scale? Are the forcing profiles also “corrected”, similar to the adjusted initial conditions? If yes, please discuss more details that how the forcing profiles are corrected in section 3. If no, will the bias in boundary layer depth and temperature affect the results?

6. Line 380-390: The rimming rate is higher for lower CCN concentrations, which explain why precipitation is stronger and LWP is lower for relatively clean case (as shown in Figure 11). This result is consistent with the warm cloud microphysics: cleaner clouds prefer larger cloud droplets, and thus benefit rimming process and precipitation. However, my question is that will it also work for the mixed-phase cloud? There are lots of study talking about cloud invigoration by aerosols in the cold cloud, and results from those studies are in general opposite to the conclusion in this study. So the question is how general results in this study are? I guess it strongly depends on the cloud microphysical scheme. But the cloud microphysical scheme is not clearly discussed in the previous section (see comment 3).

7. Line 430-434. I’m not fully convinced by the statement here, because sublimation of ice particles, in general, occurs in the subsaturated region with respect to ice (below the mixed-phase cloud base). Sublimation in the sub-cloud layer will cool the lower part of the atmospheric boundary layer, and thus stabilize the boundary layer (smaller TKE). If what you propose is true, I don’t quite understand (1) why sublimation occurs in the cloud layer and (2) why sublimation will enhance TKE physically, beyond the statistical correlations shown in Figure 16. Note that cloud top and cloud base heights are similar in the ensemble for RF05, but there is a difference in

the ensemble for RF20 (Figure 15). So is the weaker turbulence for a relatively polluted case for RF20 due to the thinner cloud layer? Another thought is that how about the effect of the temperature anomaly? The significant difference in water vapor for RF20 (Figure 13) is probably due to the temperature difference because water should be saturated in the cloud region and the saturated water vapor only depends on temperature. I think it would be nice to show the temperature as well, similar to qv (Figure 13) and TKE (Figure 15).

8. I strongly suggest the author check gramma carefully before submission. I find several typos and grammatical errors. Some of them including some minor comments are listed below.

Minor comments:

1. Line 4: change “ice clouds” to “mixed-phase clouds”?

2. When citing multiple papers at one place, please combine them in one bracket. For example, the first sentence should be “…(Walsh and Crane, 1992; Wendisch et al., 2013)” instead of (Walsh and Crane, 1992)(Wendisch et al., 2013).

3. Line 23: “is likely to”, change “is” to “are”.

4. Line 52-54: Rephrase the first sentence “While the aerosols…”. It is not clear.

5. Line 76: change “mixed phase clouds” to “mixed-phase clouds”

6. Line 102: change “were” to “was”

7. Line 104: change “provided” to “provide”

8. Line 115: “in both” is not clear to me.

9. Line 198: missing “.” after “absent”

10. Line 281: Define those microphysical parameters, at least in the Appendix.

11. Line 293: change “ise” to “is”

12. Table 2: Since for control simulation, RF05 and RF20 are the same. Why there are no ccn20 and ccn40 for RF20, and no ccn250 for RF05?

13. Line 564: delete “the” before “a lower cloud droplet”

14. Line 314: Change “two” to “three”?

15. Figure 5 caption: green line in Figure 5b indicates DS04? Similarly, in Figure 7, the green line in the left indicates DS08? It is not clear in the caption.

16. Line 352: Add “Figure” before “5.b”

17. Line 358-360: “Figure 10.a shows that…”. The statement seems not correct. Figure 10 a. shows that almost all CCN are activated for ncc20 and ncc40. Please check.

18. Figure 13 caption: doesn't make sense. There is no (e) and (f). Please check.

19. Line 377: Delete one "to"

20. Figure 14 caption: "The dotted lines in (c) and (d) than…" Delete "than"

21. Figure 14 caption: Add "and" between a) and b)

22. Line 399-400: Delete "is effect".

23. Line 411: "…firstly, the decrease in CCN…" is not clear to me. "decrease in CCN" is not "the impact of CCN concentration". It is the "impact of precipitation". Please rephrase this sentence.

24. Line 419: "This is also reflected in the faster the higher cloud tops in in model runs with higher CCN" is not clear to me. Please rephrase it.

25. Line 458-459: "we find that a higher CCN concentration leads to a higher number of cloud droplets, and subsequently suppressed ice formation and increased LWP." Please discuss whether it is a general statement or relies on the model setup.

26. Figure 16: (a)(b)(c) eat some space of x label.

27. Line 536: add space between "can" and "be".