This is my last review of this manuscript: I will decline to review this paper again. I strongly feel – as I said in my previous review, included below – that the process worked poorly for this submission. I was not able to find responses to my comments from the previous cycle. I requested major revision and the authors included only cosmetic adjustments to the previous version (modifying a few figures), basically ignoring almost all of my specific comments. My recommendation is to reject this paper and start the discussion phase again so the reviewers can see how the authors responded to their comments. I appreciate that the Editor may disagree with my recommendation and accept the manuscript. Either way is fine with me. I have to add that I am disappointed with the process and will seriously consider any further review invitations from ACP.

I am uploading this comment together with me previous review (from June 2022) below for the Editor convenience.

Review of a manuscript "Aerosol impacts on the entrainment efficiency of Arctic mixed phase convection in a simulated air mass over open water", previously titled "Aerosol-cloud-turbulence interactions in well-coupled Arctic boundary layers over open water" by Chylik et al.

Overall recommendation: publish after substantial (major) revisions

This is a revised manuscript that I reviewed last fall. The paper has been significantly revised and has a new title. With all the changes, the paper should be treated as a new submission, requiring a careful reading from scratch. Overall, I feel the discussion of the revised manuscript should start once again (and be published with comments and responses available online), and not be treated as just a check if the revisions addressed the reviewers' concerns. So either the Editor should reject the original submission and request a new discussion phase, or the authors should withdraw the paper and resubmit the new version for the discussion. In a nutshell, I feel something went wrong, especially considering the length of time between the original submission (November 2021) and submission of the revision (May 2022).

As for the science, the new emphasis provides a better focus of the paper. However, I see significant problems with the technical aspects of the manuscript as presented in my major points and specific line-by-line comments. I have to add that the authors ignored some of my specific comments as a few of those below address the same issues as in the previous review.

General (major) comments

1. This is the same as in my previous review: Manuscript title should have "modeling" word in it. Perhaps start with "Modeling of aerosol impacts…".

2. I find the analysis of the assumed initial CCN concentration interesting and worth pursuing. However, I feel the explanation of the simulated dramatic impact is mostly missing. For instance, even without the feedback into dynamics, increasing droplet concentration has to lead to the increase the reflected solar radiation as in the classical Twomey effect. This is not even mentioned in the manuscript. Of course there is a significant dynamical feedback as shown, for instance, by Figs. 11 and 12. The dramatic increase of the LWP feeds back on the Twomey effect, correct? Is that all what happens? I am not sure. For instance, with the low CCN (10 per cc), is there any impact on the precipitation initiation and fallout? In ice-free clouds, low CCN typically implies more drizzle and rain, and thus reduced LWP. How this is modified by the presence of ice? Is the ice initiation in the parameterized microphysics affected by mean droplet size, presumable larger in the low CCN simulation? In a nutshell, I find the new emphasis of the paper

interesting, but the analysis of the results leaves much to be desired. This is why I think the paper should go to the open discussion phase again so the authors can openly reply to the reviewers' comments.

3. Fig. 1 in the manuscript is as in the previous version. I commented on it in my previous review and the authors tried to respond. My suggestion is to show a small fraction of the figure, focusing on the area targeted in the LES study. The rest of the figure, difficult to see what it shows, is irrelevant. For instance, where are land boundaries? Another possibility is to add a second panel with the enlarged area targeted by the modeling study to better document cloud systems there.

4. This is following point 6 from my previous review. I still claim that the technical quality of the presentation needs to be improved. Some figures require adjustments to make them legible when included in the printed version, some figures can be improved, some show quantities that are not clearly defined, some captions remain unclear. See specific examples below.

Specific minor comments

1. Caption of Fig. 3 is incorrect (see panels b and c, and their description). Please correct.

2. Fig. 4. The caption says "histogram" but the vertical axis says "pdf". So which one is it? Pdf is a histogram with each bin value divided by the bin width. Also, I feel the caption should say that this is the histogram of time during which a given concentration was observed. I feel more details of those observations would be needed. For instance, what was the time resolution of those observations (1sec? 10 sec?).

3. L 144: "We speculate…": Do model results show that aerosol processing is responsible for local fluctuations? If so (as suggested by Fig. 12c), "speculate" is a poor word.

4. Section 3.2, microphysics. It is not clear if all ice variables (cloud ice, snow, graupel) include prediction of both mass and number, that is, as in a complete double moment scheme. It would be appropriate to mention this explicitly.

5. Section 3.3 misses information about the longwave radiation. This was my point 6 from the previous review.

6. Around l. 245 and related to my previous point 8. I am not sure why "Newtonian" is important to say wrt to the relaxation. It would be more important to explicitly state that the relaxation dumps small-scale perturbations, in contrast to the approach when the mean is relaxed to the prescribed profiles. BTW, such relaxation also serves as gravity wave absorber, correct? So why an extra absorber is added in the top 1 km of the domain? Maybe because of a much shorter time scale is used there.

7. L. 258: Considering statistics shown in Fig. 4 from observations, I would like to see similar statistics from model simulations. In particular, what is the range of CCN spatial fluctuations simulated by the model?

8. L. 262: "…using a maximum initial cloud droplet number concentration…". I do not understand what this statement refers to. Please explain.

9. Fig. 5. The most appealing feature of the figure is the sharp maximum of cloud water near the cloud top (also evident in Fig. 8). This is in contrast to warm stratiform clouds that typically feature a uniformly increasing cloud water with height (e.g., Fig. 4 in Stevens et al. 2005 cited by the authors). Is that because

of the presence of ice? If one adds ice and water in Fig. 8a, would the total nonprecipitating water follow close-to-linear profile? Also: the variable show is the mixing ratio, not "mass concentration".

10. L. 322: Why does the secondary ice production allow formation of large and heavy ice hydrometeors?

11. Figs. 9 and 10. The figures show box-whiskers data for the observations, and only mean for the model. Can box-whiskers plots be done for the model as well? I appreciate difficulties in comparing a 1D flights with 2D model data, but I feel comparing statistics would make the analysis better. L. 344 states that comparing particle size distributions is impossible, but comparing spatial variability of mass mixing ratios (and maybe concentrations) would make sense.

12. L. 359: Something is missing in this sentence.

13. Figs. 11 and 12 shows the dramatic impact of assumed initial CCN concentration on cloud macro- and microphysics. However, the analysis leaves me wondering where this impact comes from. BTW, liquid cloud fraction is not defined. For instance, is it calculated locally and then averaged horizontally, or is it based on the horizontally-averaged water and ice mixing ratios? L. 385 suggests the latter per "area fraction". Please make it clear.

14. L. 394. How important is the fact that the entrained air is warmer when the boundary layer grows deeper in the 1000 per cc simulation? This aspect is not mentioned in the discussion and I feel it should.

15. Fig. 13 and L. 399: I think "evolutions" would be better than "time series".

16. How is a1 shown in Fig. 13 defined?

17. Figs 14 and 16. Please plot bars side-by-side, not one behind the other. Initially I thought there was some logic in having some bars wider than others, only to notice that they were simply partially hidden. How is dF_{rad} in Fig. 14 exactly defined? How are LW and SW in Fig 16 exactly defined? For instance, are those across the entire boundary layer, or just near the inversion? The latter drives entrainment, correct?

18. L. 405: "This suggests that ice formations…". Yes, I agree, but it would be nice to see how. This is where the analysis needs to be expanded.

19. L. 421: "Fig. 14f confirms that entrainment rate increases…". I feel additional analysis should explain what physical mechanism(s) are involved.

20. L. 440. One may wonder if the classical Twomey effect is sufficient to explain 200 W m^{-2} difference. I doubt it. The dynamical feedback is likely the key. See major point 2.

21. Since the SST is prescribed, I find the Eq. (3) and its discussion irrelevant. My suggestion is to focus on the dynamical and microphysical effects steaming from the factor 10 CCN concentration change.

22. In view of the above comments, I did not read sections 6 and 7. I expect these will change significantly after all above comments are addressed.