Final author comments (ACs) for manuscript acp-2021-888

General response to all reviewers

We would like to thank the reviewers for carefully reading and assessing our submitted manuscript. We conclude from these reviews that the LES simulations we conducted of the boundary layer clouds observed during ACLOUD RF20 are in principle worthwile, and have scientific value. We also appreciate the positive comments on the basic configuration of the LES experiment, and its evaluation against the observational datasets.

However, both reviewers also have major concerns, which need to be addressed before publication is possible. In our understanding, the most serious issue seems to be the perceived lack of a clear scientific hypothesis and associated analysis that is worth citing, that goes beyond just showing that the LES reproduces the observations. Or in other words, that the scientific outcome is "too thin". A second major concern is the technical and scientific quality of the presentation, which was judged to seriously lack at various points.

To adequately address these concerns we propose to introduce substantial changes in the manuscript. Firstly, we would like to adopt a different main science question or hypothesis, which is that the aerosol concentration in an Arctic air mass significantly affects the efficiency of radiatively driven top-entrainment in heating the convective boundary layer. The new analysis will thus replace the mixing-line and scatter plot analysis that was included in the first submission (Section 5 named "Results II: Joint-pdf analyses"), which received considerable criticism in the review. Note that the basic evaluation of the control LES experiment against observational datasets from ACLOUD will be maintained.

The new hypothesis is motivated by the following points. Firstly, it can well be tested with the currently adopted experimental setup. Secondly, while the efficiency of entrainment in heating the boundary layer has been studied for subtropical warm stratocumulus clouds (Stevens et al., 2005), this is not yet the case for Arctic mixed phase clouds. Thirdly, ongoing work on this LES case by the authors since submitting this manuscript has yielded some new insights into this topic. We find that the heating by radiatively driven entrainment plays a key role in the heat budget of the lower atmosphere, as well as the surface energy budget. Sensitivity experiments we conducted also show that the CCN content of the Arctic boundary layer significantly changes the entrainment efficiency, affecting the lapse rate at lower levels.

The heat budget and lapse rate play a crucial role in Arctic Amplification, making this behavior relevant and of interest to the community. To our knowledge these insights are novel, and have not been published before. We hope that the new focus, hypothesis, associated analysis and results add the extra value to the paper that was asked for, and will make it worth citing. Apart from these major changes, we also tried hard to improve the presentation of the results and the quality of the figures. In combination, we hope these changes can address all concerns of the reviewers.

A detailed response to all comments by both reviewers is provided below.

Response to Reviewer 1

We are glad to read that the reviewer in principle finds the manuscript publishable, pending a few major revisions. Briefly summarized, the main issues identified by the reviewer include the following:

1. No scientific hypothesis is presented

2. The main goal seems to be to reproduce the observed clouds

3. The presentation does not reach that goal

The substantial changes in the revised manuscript as already described above in our general response to all reviewers should be sufficient to address the first point. These changes were also motivated by the comments of the second reviewer. To briefly summarize, the new focus is the heating efficiency of radiatively driven entrainment in mixed phase clouds, with the associated hypothesis that this process is significantly affected by CCN. The new focus and hypothesis are now also described more clearly in the introduction. Concerning the second issue, we now state more clearly and exactly what we expect from the model evaluation against the observations. We hope we can thus avoid raising the wrong expectations. The evaluation method is similar as applied in previous LES studies of this kind. Finally, to address the third point we put great effort into improving the presentation, both in content and technical quality.

Overall, we hope that these adjustments are sufficient for addressing the main concerns of the reviewer. We are looking forward to the assessment of the revised manuscript.

Response to general (major) comments

1) Modeling not in title / Aerosol-cloud interaction aspect is questionable

As requested, we modified the the title to include the word "simulation".

The aerosol-cloud interaction that is examined in this study is now specified more accurately: the impact of aerosol on the heating efficiency of radiatively driven entrainment in Arctic mixed-phase clouds.

2) Complex cloud systems / Time evolution of clouds / General motivation behind LES simulations

A detailed evaluation of the geometry and time evolution of individual cloud structures in the simulation is not the objective of this study. Like most previous LES studies of Arctic clouds, the evaluation of model performance is mainly done in a statistical way. That means we seek agreement on bulk properties such as cloud mass, phase and height. This is sufficient for showing that the control LES run reproduces the observed mean structure of the observed boundary layer and clouds, and that it can serve as a reference experiment in further sensitivity runs.

Recent LES studies of this kind include Ovchinnikov et al. (2017) and Stephens et al. (2018), in which multiple LES codes took part. Often the lack of observational data is a problem in such studies. The in-situ cloud data from ACLOUD RF20 already allows us to perform a much more thorough evaluation. However, a detailed evaluation of individual cloud geometry and life-cycle would require additional data that were not collected during the campaign.

To avoid raising too high expectations, in the revised manuscript we explain in more detail what we expect from the evaluation, and also put our study in the context of previous LES studies of Arctic clouds. We also express more clearly in the introduction what is the motivation of doing LES of this case: i) an understudied cloud regime, ii) a key area for air mass transformations by low level convection, iii) availability of unprecedented measurements, and iv) opportunities for defining a representative control case for impact studies of CCN on entrainment.

Concerning the time evolution, it should be noted that we use a composite case setup. This means that forcings are time constant, and reflect a time average. As a result, the experiment is most useful for investigating how the boundary layer equilibrates.

3) Doubts about microphysics scheme / Microphysics evaluation

In our opinion the criticism on the microphysics scheme used in our LES is not really justified, for the following reasons. In contrast to what the reviewer states, the scheme is actually state-of-the-art. The scheme is double moment, avoids using a temperature-dependent phase function, and treats CCN prognostically. In these aspects the scheme is already much more sophisticated than other LES models used in recent peer-reviewed studies (e.g. Ovchinnikov et al., 2017; Stephens et al., 2018; Zhang et al., 2022).

The main question underlying this comment seems to be what level of sophistication in the microphysics scheme is sufficient for achieving our science goals. This does not necessarily have to be the most complex approach, such as a bin-microphysics scheme, which is hard to constrain with observations and can thus introduce its own unwanted uncertainties. Our results show that the double-moment mixed phase scheme we use reproduces the observed basic cloud structure, mass and phase to a satisfactory degree. That is sufficient to support the studies of entrainment efficiency, which is mainly driven by radiative cooling linked to liquid cloud mass. This justification of the microphysics approach has been added in the revised manuscript.

In principle, we share reviewer's concerns about the saturation adjustment and we originally considered addressing the droplet condensation growth in the discussion. However we deemed it too technical and too detailed for the scope of the article. There are two main reasons for applying the saturation adjustment: 1) consistency and 2) time scale involved. Firstly, the saturation adjustment as applied is part of the scheme (Seifert and Beheng, 2006) and has been used in number of studies of mixed-phase clouds. Secondly, the time steps in our simulations are within the range 0.7–8.1 s. If we consider the droplet growth as described by Lamb and Verlinde (2011) and Pruppacher and Klett (2012), we can estimate the growth speed of cloud droplets in the moist updrafts here. The growth speed implied by the saturation adjustment is within the bounds, and the main factor controlling the size of the droplets is the number of CCN activated. That said. If a simulation examined strongly stratified clouds in tenuous regimes (and thus encountered both short timesteps as well as higher supersaturations due to slower growth rates of larger cloud droplets), then it would be reasonable to test the sensitivity of saturation adjustment vs the explicitly calculated condensational growth of the cloud droplets. However that is not the case here.

Please note that the availability of in-situ observations on both liquid and ice mass, as well as aerosol, is already a big step forward compared to previous LES studies of mixed phase clouds, which often lack one of these datasets or have to completely rely on remote sensing data. That novelty is now mentioned more clearly in the manuscript. The detailed evaluation of cloud droplet and ice concentrations that the reviewer asks for is unfortunately not possible, because the aircraft data is too incidental and sparse for this purpose. For these reasons we decided to evaluate the model in terms of mean (bulk) properties, and not higher order statistics.

4) It is unclear how CCN and IN are prescribed and evolve

In response to this comment we have substantially rewritten the description of the treatment of CCN and IN in the simulation. The time evolution of CCN in the simulation is now also documented.

5) The importance of ice

Cloud ice is important in this case, because a significant part of the total condensate is in frozen state. This applies to suspended (cloud) ice, but also the precipitation in the cloud layer. The ice phase thus plays a key role in the humidity budget of the boundary layer. Similarly, the formation of ice affects the radiative budget, by removing liquid cloud mass which is most important for the longwave radiative cooling that drives the turbulence near the thermal inversion. For these reasons it is important to account for ice in the simulations. We have added this explanation to the manuscript.

6) Technical quality low / Conclusions too short / 'Thin' outcome

Thanks for pointing out the technical shortcomings, this helped us a lot in improving the figures and presentation. The concluding section has also been expanded, and now focuses better on the new insights obtained in relation to the new main hypothesis.

We understand from the reviewer's comment that original sections 6 and 7 were not reviewed. But the key analysis and results were actually presented in exactly these two sections. Accordingly, in our opinion the verdict of "thin outcome" is not supported by the content of the review. Please note that the revised paper has been significantly altered, in particular concerning the main hypothesis and analysis. Accordingly, we hope that these adjustments have made the outcome of the revised manuscript much more substantial.

Response to detailed comments

1. Sentence has been rephrased

2. Sensitivity simulations with different CCN and IN concentrations are now included, so that the interactions are properly studied

3. There is value in overlaying pdfs in the plot, it is also a technique that is often used. Information about solubility is added. The main reason for the difference in aerosol above and below the clouds is their removal by precipitation. This is now mentioned, and the evolution of CCN structure is now also documented.

4. The description was corrected. Scalar advection is represented by he centered difference method. We refer to Heus et al. (2010) for the full details.

5. As explained in Section 3.2, the initial CCN concentration is guided directly by the in-situ observations by the P6, as shown in Fig. 4. We do not have measurements of IN concentrations available, for this we follow previous studies. Please note that in using in-situ observations of CCN concentrations we already go far beyond previous LES studies of this kind, which often just use climatology.

6. A description of the long wave radiation is added

7. This statement has been modified, and details have been added to the description.

8. We now consistently use "continuous nudging". However, it should be mentioned at least once that this nudging is Newtonian. The nudging is only applied above the themal inversion height, not below. Indeed the perturbations are simply removed.

9. Yes, the CCN can evolve freely. While no sources of CCN are included (this would be purely speculative), the drop in CCN level during the simulation time is small enough not to matter. This is now mentioned in the text.

10. This type of plots, with a shaded background but no solid axes, has been accepted for publication in numerous journals, and also adheres to the ACP authors' instructions. The grid lines are clearly visible, and the contrast with the white background serves as the panel boundary. The text size in the figure labels has been increased, as requested.

11. As stated above, relaxation is only applied above the thermal inversion, not below it. This method thus prescribes the boundary condition for turbulence, not the turbulence itself. This also means that below the inversion, the agreement between model and measurements is not trivial. We now explain this in more detail in the text.

12. The figure has been removed in the revised manuscript

13. We do not agree, the comparison is actually quite good. With only a few flight legs available, it is much more difficult to quantitatively compare concentrations, compared to "simpler" bulk properties such as mass. For this reason we decided to only evaluation cloud mass and ice, as also explained in our general response. As requested, we further improved the figures. For example, we added the mean observed values, to allow better comparison between model and measurements.

14. The time height figures have been improved, including their description.

15. The word "textbook" has been changed

16-18. These figures have been removed in the revised version.

19. The second part of the results section has been substantially changed.

Response to Reviewer 2

Response to general (major) comments

We thank the reviewer for the extensive assessment of our manuscript, and for the constructive feedback provided. On the positive side, we are glad that the reviewer agrees that the clouds observed during ACLOUD RF20 are relevant in the context of Arctic climate change, and that this motivates detailed scientific study. Our LES simulations are also considered to be extensive, and the model is judged to be a very capable of reproducing the observed conditions. That said, the reviewer also has some major concerns about the submitted manuscript. To briefly summarize, these issues include:

1. The description and data presentation are not good enough to make results reproducable
2. The description of some aspects of the model and the numerical experiment are not adequate enough
3. A central science question is lacking, and the scientific analysis is limited, in particular concerning aerosol-cloud interactions. Overall the paper does little more than demonstrate that the model's CCN sinks do indeed remove CCN
4. Part II of the result section focuses too much on a single convective event, which might harm representativeness and statistical significance.
5. The analysis does not make enough use of conditional sampling

We find this feedback really helpful. First and foremost, it has made us reconsider the general structure of this study, in particular what type of science question should be addressed and which analysis should be included. The reviewer is right in stating that the simulation described in the manuscript is perhaps most useful to serve as a 'baseline' or control experiment, given the good agreement with the observed thermodynamic and cloudy state. In the revised manuscript, a set of sensitivity experiments is included that deviate from the control setup at crucial points, designed to test a newly formulated hypothesis (as described above in the general response).

Secondly, we agree that the mixing line analysis (part II of the results section) was not substantial enough, and needs more work. We are still of the opinion that the conserved variable diagrams, as well as the other scatter plots, are an efficient way for understanding and visualizing the aerosol-cloud-turbulence interactions in the simulation. However, the application to a single case at a single level is indeed not a quantitative, statistically significant analysis. Accordingly, to give this analysis the space it deserves, we decided to take the mixing line and scatter plot analysis out of this manuscript, and to reserve it for a future publication.

Instead, the sensitivity of the entrainment efficiency to CCN concentrations in mixed-phase convection over open water is now adopted as the central science question of the revised manuscript. This process has been studied for warm clouds (Stevens et al., 2005), but not yet for cold mixed-phase clouds. We find this impact to be significant across the range of CCN concentrations typically observed in Arctic air masses in the area of interest. The efficiency of entrainment also plays an important role in the boundary layer heat budget, as well as the surface energy budget. Both matter for Arctic Amplification and sea ice melt, making this a relevant topic. We investigate in detail how this process works, reporting some unexpected insights.

The adoption of a new main science question and associated analysis make the use of conditional sampling less relevant. The same applies to the time evolution of individual clouds. We agree that these are interesting topics in themselves, but consider them future research topics for now.

Apart from thus rethinking the general structure and content of the paper, we have also made an effort to follow up on the recommendation of improving the figures and descriptions, and bring them up to a scientifically acceptable standard. The title of the manuscript was altered to better reflect the new content. We hope that together, these substantial changes adequately address the reviewer's concerns, and have made the manuscript acceptable for publication in the ACP.

Response to detailed comments

Line 30-31: This part of the introduction about cloud classification has been rewritten

Line 48: Phrasing has been altered, as requested

Lines 80-86: Repetitive text has been removed

Line 97-98: Changes applied as requested

Figure 7: The spread is only minimal, which is a non trivial result in itself. So we maintained the percentile shading technique, also because in the next figure, which uses the same technique, the spread is a lot larger. This way, the plotting style is consistent across multiple plots.

Figure 9, and line 328-331: Figure 9 has been removed. Averaging times are now clearly indicated in the other figures.

Figure 11, and line 360: The profiles represent the domain average over a horizontal slice of grid boxes, covering a specified time period. The description has been altered, as requested.

Figure 11, lines 366-367: This figure has been modified, as well as its description, as requested.

...

The detailed comments from "Figure 12" to "Line 525" all refer to figures and text that have been completely removed in the revised manuscript.

...

Line 531: We now refer to the use of reanalysis data as a possible way to establish how typical the stagnant wind conditions occur in the region.

Figure B1: Changes were made as requested.

References

Heus, T., van Heerwaarden, C. C., Jonker, H. J., Siebesma, A. P., Axelsen, S., van den Dries, K., ... & de Arellano, J. V. G. (2010). Formulation of and numerical studies with the Dutch Atmospheric Large-Eddy Simulation (DALES). *Geosci. Model Dev*, *3*, 415-444. https://doi.org/10.5194/gmd-3-415-2010

Lamb, D., Verlinde, J. 2011: Physics and chemistry of clouds. Cambridge University Press.

Ovchinnikov et al., 2014: Intercomparison of large-eddy simulations of Arctic mixed-phase clouds: Importance of ice size distribution assumptions, *Journal of Advances in Modeling Earth Systems*, 6, 223–248, https://doi.org/10.1002/2013MS000282

Pruppacher, H.R. and Klett, J.D., 2012: Microphysics of Clouds and Precipitation: *Reprinted 1980*. Springer Science & Business Media.

Seifert, A., Beheng, K., 2006: A two-moment cloud microphysics parameterization for mixed-phase clouds. Part 2: Maritime vs. continental deep convective storms. *Meteorol. Atmos. Phys.* **92,** 67–82. https://doi.org/10.1007/s00703-005-0113-3

Stevens et al., 2005: Evaluation of Large-Eddy Simulations via Observations of Nocturnal Marine Stratocumulus*, Monthly Weather Review*, **133**, 1443 – 1462, https://doi.org/10.1175/MWR2930.1

Stevens et al., 2018: A model intercomparison of CCN-limited tenuous clouds in the high Arctic, *Atmospheric Chemistry and Physics*, 18, https://doi.org/10.5194/acp-18-11041-2018

Zhang et al., 2022: Seasonal cycle of idealized polar clouds: Large eddy simulations driven by a GCM. *Journal of Advances in Modeling Earth Systems*, 14, e2021MS002671. https://doi.org/10.1029/2021MS002671