Response to Anonymous Reviewer 2

We would like to thank the anonymous reviewer for the careful assessment of our submitted manuscript. We appreciate the willingness to review our manuscript, and the detailed comments provided, which draw our attention the ambiguities in the description of our observations and methodology, as well as to other shortcomings of our manuscript. In the revision of our manuscript, we have tried to address each point. Please find our detailed response to each of your points in the text below.

1 Major Comments

The topic of the presented study is highly interesting and the title is quite intriguing. However, when reading and trying to understand the study and the results, I am not impressed. The manuscript is in quite a bad state with lots of repetition, spelling errors, poor quality figures, missing figure etc. This is a misuse of the time of a reviewer. In the current state, it is not possible to review the science and I therefore recommend rejection of this manuscript. I hope the authors take the time to do a thorough rewrite, I am happy to review it again provided it is presented with the details and explanations that is needed to assess the quality and validity of the work performed. Below I illustrate my decision with some examples, they are far from exhaustive though.

We thoroughly appreciate the assessment by the reviewer that the topic of our study is highly interesting. It is clear that the assessment of the first submitted version was quite critical. What we take away from this review is that the overall quality of the text is simply too far below the acceptable standard for scientific publication, and that this is the main reason for the reviewer to recommend a rejection. While not intended as an excuse, it is worth noting at this point that this paper is the first by the corresponding author as a lead-author, which might explain some of these deficiencies. In the revision of the manuscript we have taken great care to achieve an acceptable standard. Many changes have been made to the text, the figures, and the content. One of the main goals was to remove the inconsistencies as identified by the reviewer. We sincerely hope that these changes will be sufficient; in that sense we are grateful for the willingness of the reviewer to again review the revised manuscript.

1.1 Title

The title contains Arctic Mixed-Phase Clouds which in my view leads to the long-lived persistent clouds that are found in the Arctic. The simulation, however, are based on observations that are classified as cold-air outbreak days. Although the clouds in cold- air outbreaks also can be of mixed-phase, which is common in Cu clouds, the title is still a bit misleading.

The clouds that were the centrepiece of this research are really mixed-phase clouds, as is clearly evident from the measurements performed during the ACLOUD campaign (Wendisch et al., 2018). There were a number of previous studies that talk about convective mixed-phase in the Arctic, such as Fridlind et al. (2007) and Verlinde et al. (2007). While RF05 is a clear case

of a cold outbreak, case RF20 focus on a very different scenario — it is a case of multilayered cloud in the absence of rapid surface heating.

1.2 Observed Weather Scenarios

The presentation of the cases that the study is based on does not give enough in- formation to be convincingly chosen. The background information is scattered and not coherent. Figure 1 claims to present the "mesoscale weather situation" and shows MODIS Aqua views and the flight tracks. It is not even clear what clouds we are looking at and how this relates to the design of the experiments.

The description of the cases has been thoroughly rewritten. Firstly, we have added a motivation for the choice of cases to the introduction (lines 72-76). Secondly, with an aim to improve the clarity of the case description, we have modified the caption of the figure showing the MODIS satellite products (Figure 1). Much more information is now provided, such that these cases are better motivated and should also be reproducible by independent researchers. For example, initial profiles are now shown for multiple variables. The MODIS images are now better described and the markers for location have also been improved. In these satellite images, clouds can indeed be hard to distinguish from sea ice; however, the main purpose of including these images is to illustrate the cases the best as we can; we do not have access to better data products.

1.3 Aerosols Observations

Figure 2, where are the observations taken?

We have tried to improve the clarity of the description of the locations of aerosol observations. In the revised version of the manuscript, we have explicitly indicated the geographical locations of aerosol measurements (Figure 1).

1.4 Aerosols Statistics

Figure 3, there is a mixture of statistical methods in this figure, using medians and in- terquartile ranges are used if data is non-gaussian. Why then plot standard deviations, if that is what is meant by "standard 1.5 range"?

We thank the reviewer for pointing out that the formulation "standard 1.5 range" is ambiguous. After considering various options, we have modified the Figure 3 so the whiskers now indicate the 5th and 95th percentiles.

1.5 Demi-lagrangian

The explanation and motivation for the demi-Lagrangian method cannot be understood. A statement like "has been widely used outside the Arctic" must be followed by refer- ences.

Indeed, the demi-Lagrangian method is not new concept, and it has been widely used outside of Arctic. We have included many references to such studies, including Liu et al., (2004) and Richardson et al. (2007). The demi-Lagrangian configuration means that only part of the column is aligned with the flow: in our case, the lower atmosphere, where the mixed-phase clouds reside.

The benefit of this frame of reference is that it minimises the amplitude of the large-scale advective tendencies that have to be prescribed; as a result, uncertainty in the case setup is reduced. This motivation and explanation is now added to the text (lines 196–216). For further explanation of the approach, we refer to Neggers et al. (2019).

1.6 Dropsonde

On Page 10, you write that you are referring to DS01 and DS08 but in the Figure it says DS01 and DS07.

The typo in the legend of the figure has been corrected, it is now referring to dropsondes DS01 and DS08.

1.7 Adjustment of Initial Conditions

The alterations that are done for RF05 are huge and still the comparison with the DS01 is way off although you write "generally agree". Where is the comparison of the vertical profiles? What stratification, winds, and RH do you have? Are we even close to reality? Do we have any idea if the turbulence in the LES is generated by the correct processes to be able to compare with reality and thus analyse any sensitivity?

We agree that the alterations are substantial. However, this is unavoidable and a necessary evil whenever large-scale model data are used to drive and LES experiment, given the well-known significant biases in the state of the lower atmosphere The absence of dense observational networks simply leaves us no other choice. While the original Figure 4 was misleading, we have instead included overview of the initial profiles.

To meet the reviewer's demand for a comparison of vertical profiles between LES and observations, Fig. 6 has been added. Indeed the match is not perfect; but we do not agree it is "way off" either (which in itself is a subjective and this non-scientific statement). For example, i) the inversion heights are pretty close, ii) so is the free tropospheric state, and iii) the vertical structure below the inversion is similar. Only for water vapour do somewhat larger deviations exist. However, in our experience in comparing LES to observations, deviations of this magnitude are pretty typical. As previous LES studies of a similar nature have shown, a less than perfect agreement with observations does not complicate the use these simulations for gaining insight into how nature might work; as long as they are in the right ballpark. This is exactly the purpose of this study; not to achieve a perfect match with observations, but to learn from behaviour of mixed-phase microphysics in a resolved, turbulent environment that is more or less representative of the situation probed by the aircraft during ACLOUD.

Finally, concerning the comment on the generation of turbulence, in these situations of mixed-phase clouds over open water there are various sources: i) wind shear (mechanical turbulence), ii) surface buoyancy flux and iii) long-wave cooling at cloud top. All three processes are represented in the simulations; accordingly, one can conclude that the turbulence is generated by the right processes.

1.8 Coefficients for Ice Microphysics

Table 1 consists of coefficients that you do not explain at all what they are used for. Are they all unitless?

All coefficients in the table are now explained in the text and have the right units. We have checked the literature and added the appropriate units to the shape and size parameters. Since the part of the methodology on microphysics was thoroughly rewritten, the table is now located the appendix A1

1.9 Figures 5

Figure 5 and others, do you really think that it is appropriate to provide four significant numbers for the cloud liquid water content?

The organisation of the figures in the manuscript has been thoroughly modified, better grouping them around a specific topic and also leaving out figures and panels that were not essential. We have also improved the labelling of the colourbars of the contourplots. We have replaces previously arbitrary values that are multiply of 0.04 and increased the number of labelled values.In the similar manner, we have also adjusted figures for concentration of cloud ice (now Figure 5.b-c).

1.10 Figure 13

Figure 13, caption explains panels e and f. The figure only contain a-d.

We have corrected the text of the caption (now Figure 12) to refer to panels a-d instead of c-f