Response to Reviewer 1

We are grateful to the reviewer for several constructive comments and suggestions that have helped us improve our manuscript. Reviewer's comments are given in red and our response follows in black.

1. Comparison between the simulated ice number and observed ice number should be included in the study since this is the aim of this study. If the observation data for ice number is not available for this case, the author should use a different case that has this useful observation data. Otherwise, it is hard to justify if the modification in the model leading in the right direction. Lacking this comparison makes the paper less convincing to readers.

Unfortunately measurements of cloud particle number concentrations were not measured during ASCOS. Such measurements have been collected during Arctic flight campaigns, but these are generally conducted at lower latitudes (e.g. ACCACIA, M-PACE, RACEPAC). Investigations focusing on lower-latitude clouds have been performed (e.g. Sotiropoulou et al. 2020) and indicated a possibly critical role of the examined process. However understanding microphysical interactions over the high Arctic and over multi-year ice-pack is particularly important and that is why ASCOS data (collected at ~87°N) have extensively been used for microphysical investigations and model intercomparisons (e.g. Lowe et al 2017; Stevens et al. 2018; Christiansen et al. 2020), even though there are no detailed microphysical measurements. Thanks to previous studies, a good understanding of how different treatments of ice nucleation and CCN activation impact cloud macrophysical properties has already been established. Here we aim to build on existing knowledge and further quantify the possible impact of SIP. Furthermore, the results can be compared to previous investigations of this process, which also used macrophysical quantities to evaluate the performance of their parameterizations due to a lack of ICNC measurements (e.g. Fridlind et al. 2007; Fu et al. 2019).

2. The scientific contribution is not significant enough for this paper. The implementation of the secondary ice production processes to the model is clearly shown in your previous paper. Just several sensitivities tests are not enough to support a whole research story. More deep analysis should be conducted, like give a physically-based explanation of changes in LWP and IWP, not only just describe the figures feature.

Note that this is the first attempt to describe the process interactively in MIMICA (a parcel-model based parameterization was applied in the previous study). We believe that this work will be useful as a guide for how these processes can and should be considered in global models. Nevertheless, thank you for this comment, as it made us look into the feedbacks between ice multiplication, precipitation, changes in size distributions and sublimation in the subcloud layer more carefully. These parameters are now shown in Figures 4 and 5.

3. The "spectral representation" in the title and "Sensitivity to the representation of the ice particle spectrum" in Page 12 (Line 401) are confused to readers. The representation of the ice particle spectrum indicates the size distribution function, just as the authors described in the paper Line 153 "size distributions are defined by generalized Gamma functions". I think the author did a sensitivity test about the threshold value in the cloud ice and snow autoconversion process, not about the size distribution function. I suggest

modifying the title and the subtitle.

The new title is: 'Ice multiplication from ice-ice collisions in the high Arctic: sensitivity to ice habit, rimed fraction, ice type and uncertainties in the numerical description of the process'. This also refers to the new sensitivity tests that concern uncertain parameters of the break-up description, whose conduction was suggested by Reviewer 3.

Minor comments:

1. Page 3 (Line 100) what is the uncertainty range of the instrument and the observation data?

The uncertainty in LWP and IWP, i.e. the macrophysical quantities used to evaluate the results, is already stated in Section 2. We further added uncertainties in radiosonde measurements and CCN measurements, which were used to initialize the simulations. Finally, we now also state the vertical resolution for radar measurements, which indicates the uncertainty in defining cloud boundaries (cloud top and base height).

2. Page 10 (Line 325) "Planar ice is expected to generate more fragments per collision compared to plates if the diameter of the particles and the collisional kinetic energy are the same (see equations 6-7 in Appendix B). " you mean "dendrites ice is expected to generate more fragments per collision compared to plates"?

We apologize, this statement is wrong and has been removed. The same diameter does not imply same collisional kinetic energy, as terminal velocities are differently parameterized for the two ice habits.

3. Page 10 (Line 309) "while the ICNC enhancement from break-up is shown in the Supplementary Information (Text S2, Fig. S2)" I think a X-Y Figure (similar as Figure 2) shows the total ice enhancement is quite important, this figure show be shown in the main text. I also suggest adding a figure shows the comparison between the observed ice number and simulated ice number concentration.

Following the reviewer's suggestion we now have included a figure that shows the mean ICNC and IWP enhancement in the main text (and removed the corresponding figures from SI). Unfortunately there are no observations of ice number concentrations as discussed above.

4. Page 10 (Line 330) "This variability indicates that precipitation processes (i.e. the precipitation sink) are more effective". Author indicated that the decrease of cloud ice in Figure 3b is due to precipitation sink, but why the graupel number still increase in Figure 3d? considering the graupel has a larger fall speed parameter, should precipitate more quickly compared with cloud ice.

Thank you for spotting this, this statement was indeed wrong. Increases in cloud ice number concentration result in more cloud ice-drop collisions (thus graupel formation) and cloud ice aggregation (thus snow formation). This means that any Ni decrease that follows a Ni enhancement is due to cloud-ice depletion through snow and graupel formation (not through precipitation). This is why fluctuations in Ni correlate with fluctuations in Ng and Ns in Figure S2 (which corresponds to the old Figure 3 in the previous manuscript). Due to a larger number of figures being included in the main text to study the influence of precipitation, sublimation and particle size, we have moved this figure to the supplementary information.

5. Page 32 (Line 1000) In Table 1, the parameters av for graupel is set to be 199.05 in the model, However, the av is usually set to be 19.3 for graupel, and is 114 for hail (Morrison et al, 2009). So, 200 seems too large for me, is any citation here to support that the Arctic graupel has big value av?

In Milbrandt and Morrison (2013), the a_v parameter is set to 62.92 for graupel particles with a density of 50 kg m^{-3} (see Table 2 in their study) and 189.02 for a density of 850 kg m^{-3}. However in many other microphysics schemes a substantially lower a_v is assumed. We could not find terminal velocity parameters specifically constrained for Arctic graupel in the literature, but since convective motions in the Arctic are weak it does make more sense to adapt the lower values.

Following the reviewer's suggestion, snow and graupel parameters in the mass-diameter and fallspeed-relationships have been replaced with those from the Morrison scheme in the revised study. While this has a negligible effect on the CNTRL simulation, it has a greater effect on ice multiplication, since fragment generation is a function of collisional kinetic energy. For moderate ice production the effect was weak. For example in BRDEN0.2 the maximum total fragment generation rate was 1.4 L^{-1}s^{-1} while now it does not exceed 1.1 L^{-1}s^{-1}. In BRPLA0.4 however, where explosive multiplication occurs, the maximum fragment generation rate was 73.6 L^{-1}s^{-1} in the old simulation setup while now it has decreased to 12.84 L^{-1}s^{-1}. An important impact was also found in simulations with active cloud ice-to-snow autoconversion. Enhancing snow formation results in enhanced ice multiplication; however if large terminal velocity parameters are used, the enhancement can be significantly larger. This is why adapting a low separation diameter (125 μm) for cloud ice and snow resulted in substantially more multiplication than when adapting the 500-μm threshold and thus limiting break-up of snow; note that snow-graupel collisions are the main source of fragments. In the new simulations with more moderate terminal velocities, enhancement of break-up through autoconversion results in moderate increases in fragment generation. For this reason the sensitivity of our results to the choice of the cloud ice-to-snow critical diameter has substantially decreased. This is now stated in lines 329-330, while only results for the 500-μm threshold are shown in the relevant figures.

6. Page 36 (Line 1070) In Figure 2, does this mean observed LWP and IWP does not change during this time period? This figure is kind of confused, I suggested use time-series of observed LWP and IWP with uncertainty.

Note that the Large Eddy Simulation does not account for changes in the large-scale forcing and aerosol conditions and thus eventually develops a cloud in a quasi-equilibrium state. In reality the 'steady' stratocumulus cloud lasted only for about twelve hours, while aerosol conditions likely changed substantially after this (Stevens et al. 2018). And even within these 12 hours vertical displacements associated with changes in the vertical large-scale forcing were observed, which cannot be captured by any LES model (see Figure 11 in Stevens et al 2011). Moreover, the model requires a relatively long spin-up time to develop its physics, so observation-model comparisons at each timestep are not very consistent. Thus LES simulations are in a sense semi-idealized. For all these reasons we use the macrophysical statistics from the 'steady' cloud layer period to evaluate our simulations (this is explained in lines 150-155). Point observations are, however, presented in the study in the RFD plots (Figures 9-10) to evaluate phase-partitioning in the model.

7. Page 36 (Line 1070) From Figure 2, the simulated LWP decreased by 50 g m^{-2}, but IWP only increased by 5 g m^{-2}. Does this means the total condensation is decreased? Or precipitation is increased?

Both precipitation and sublimation in the sub-cloud layer increased. The feedbacks between ice multiplication and these processes are now discussed more extensively in the revised text (Section 4.1 / Figure 4).

8. Page 37 (Line 1095) Figure 3e does not have a black line, does this mean control simulation do not has snow?

Yes, snow number concentrations do not exceed threshold values (10^{-4} L^{-1}) in the CNTRL simulation. This is because snow is only treated as aggregate in the default MIMICA version and cloud ice–cloud ice collisions are not favored in the CNTRLDEN simulation. Once break-up is activated, multiplication of cloud ice results in more collisions between these particles and promotes snow formation. This is discussed in lines 372-377 in the revised text.

9. Page 37 (Line 1095) Figure 3 shows graupel is the dominant ice-phase particles, it is 2 orders of cloud ice and is 3-4 orders of snow. Is that true for Arctic cloud? Graupel is the dominant ice particles in the Arctic cloud? Or it is a model dependent result? I think snow and cloud ice should have the largest fraction of total ice.

Graupel can be dominant in some cases in Arctic clouds (e.g. Fitch et al. 2020), although graupel formation has been linked to the existence of convective cells in the past (Lawson and Zuidema 2009). A recent study however suggests that static destabilization through cloud-top radiative cooling can favor graupel formation in Arctic boundary-layer clouds. Nevertheless, for the examined case graupel formation is indeed a model-dependent result. Polarimetric radar measurements are not available to evaluate this behaviour. In the ASCOS intercomparison project (Stevens et al., 2018), where five models with bulk ice microphysics were compared, COSMO-LES produced only cloud ice. WRF simulated only snow, while COSMO-NWP and UM-CASIM resulted in both snow and little cloud ice, with snow being very little in the former. Note that all these models were constrained with the same primary ice production rate. MIMICA was the only model that produced graupel, however it was among the models (including UM-CASIM) that resulted in more realistic IWC values (see Figure 11 in Stevens et al. 2018), while all other models predicted very little ice content. The fact that ice type is model-dependent and the reason why MIMICA promotes riming compared to other models is now discussed extensively in lines 303-314.

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