Response to Reviewer 3

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

Major Comments

The results are impressive with greatly improved agreement to observations when breakup in ice-ice collisions is included. This vindicates the vision of Schwarzenboek *et al.* (2009) who made observations of this breakup occurring in Arctic clouds. It would be nice to compare the current prediction with their observations. If they measured that roughly half of all ice crystals had branches missing, is this consistent with the ice enhancement ratio of 2 measured ? Likewise with Rangno and Hobbs (2001).

We thank the reviewer for his/her comments. Rangno et al. found that about 35% of the observed ice particles have likely been produced by ice-ice collisions. This is generally consistent with the 1.5-2fold enhancement of ICNCs found in our simulations. Schwarzenboek et al. (2009) found an indication of fragmentation in 55% of their samples; however, they could confirm natural fragmentation only for 18%. The fragments generated per collision were estimated to be typically less than 5 in their study (with 1-branch crystals being more frequent). Our model predicts that only 10-12% of the particles contribute to fragmentation but a larger number of fragments (of the order of ~10) is generated per snow-graupel collision. However, Schwarzenboek et al. examined particles with sizes about 300 μm or somewhat larger. In our study, mm-size particles dominate ice multiplication. Thus, generation of more fragments per collision is expected.

A discussion on the ice particle sizes that contribute to multiplication is added in section 4.1. A qualitative comparison of the ICNC enhancement factors found in our simulations and in the results in Rangno and Hobbs (2001) is also offered in the same section, lines 421-424. Differences between our findings and Schwarzenboek et al. (2009) results are discussed in the 'Discussion' section.

There is some uncertainty in the breakup treatment. As a sensitivity test, it might be worth removing the correction factor (to correct for sublimational weakening in Vardiman's data) in the breakup scheme by Phillips et al. (2017a): what is the effect from such uncertainty ? Alternatively, if the number of fragments per collision is altered within the range of uncertainty apparent from the error-bars (a factor of 3 uncertainty) in the plots by Phillips et al., does this drastically affect the cloud simulation ?

We added sensitivity tests in which the sublimation correction factor has been removed from the parameterization. This resulted in explosive multiplication and cloud glaciation for both simulations with dendrites and plates. Activating ice-to-snow autoconversion, and thus enhancing precipitation, prevents cloud glaciation in simulations with dendrites but not for plates. These results are discussed in section 3.3.4

It would be good to include a short model description perhaps near Section 3. After reading the paper, I am still unclear if MIMICA is bin or bulk microphysics and what its microphysical species are. It seems to be bulk microphysics only.

MIMICA includes a bulk microphysics scheme, this is now explicitly stated in Section 3.1 to avoid confusion. Also, a summary of all the included ice-liquid interactions is now given in the same section, while the corresponding formulas can be found in Wang and Chang (1993).

One wonders if sublimational breakup will further improve agreement with the observations

when it is treated in models. If sublimation is happening in the cloud, then this might boost the breakup in ice-ice collisions by weakening the ice.
Examination of the domain-averaged profiles of saturation with respect to ice does not indicate subsaturated conditions within the cloud. This is now mentioned in the 'conclusions' section.

It would be good to apply the theory by Yano and Phillips (2011) to understand why the ice multiplication is weak in these Arctic clouds. You can estimate first the order of magnitude of the time for growth of snow particles to become graupel, given the typical LWC. If one replaces the "small graupel" in the theory by Yano and Phillips by "snow", then that time-scale (tau_g) gives the order of magnitude of the multiplication efficiency (c_tilde) measuring the instability of the system of ice multiplication. The average number of fragments per graupel-snow collision would be needed too. Phillips et al. (2017b) did such estimates for their multicell convective system to estimate c_tilde and so it should be possible to do here. The authors will probably find, if they do this theoretical estimate, that the Arctic clouds are weakly unstable because the LWC is weak.
We derived tau_g from two simulations, which was found to be shorter than in previous studies (7-8 min). For BRDEN0.2 and BRPLA0.2 we estimated $\hat{C}=1.6$ and $\hat{C}=2.2$ respectively. Indeed while $\hat{C}>1$, which indicates that explosive multiplication is possible, these values are substantially smaller than the value $\hat{C}=10$ estimated for a convective cloud by Phillips et al. (2017b) and for warmer Arctic clouds by Sotiropoulou et al. (2020). This is now discussed in the 'Discussion' section.

Detailed comments

Abstract

I am not sure if it is entirely accurate to say that habit and rimed fraction are "poorly constrained". Habit is something observe-able in the aircraft data (e.g. observations of axial ratio of ice particles from aircraft flights are sometimes used for model validation). Perhaps what is meant here is that most models do not have the detail required to predict these explicitly. Some models do have the detail (e.g. Hebrew University Cloud Model, which has a bin microphysics scheme with dendrites, columns etc as separate species and rimed fraction).
Since a dendrite is a type of planar particle (axial ratio < 1), it might be more accurate to describe these two habits as "non-dendritic planar" particles and "dendrites".
Thank you for this clarification. This statement has been removed from this section. We now simply discuss the fact that while most bulk microphysics schemes do not predict ice habit and rimed fraction, according to our results this is not detrimental for the representation of ice multiplication due to break-up. This is particularly important for climate models, which often employ more simplified bulk schemes (e.g. Morrison and Gettelman 2009). Finally, the term 'planar' has been replaced with 'non-dendritic planar' throughout the text.

1. Introduction

Line 56: There is a missing reference: Fu et al. is cited but not listed.
The reference has now been added

Line 59: The paper by Schwarzenboek *et al.* (2009) is by far the most important work underpinning the present study. So it needs more detail in description of how they observed breakup in the Arctic. Need to describe how they distinguished between artificial breakup on impact with the aircraft and natural breakup in the cloud before sampling.
We added a paragraph in the introduction that describes the results of this study:

'Schwarzenboeck et al. (2009) found evidence of crystal fragmentation in 55% of their in-situ samples of ice particles collected with a Cloud Particle Imager during ASTAR (Arctic Study of Aerosols, Clouds and Radiation) campaign. However, natural fragmentation could only be confirmed for 18% these cases, which was identified by either subsequent growth near the break area or/and lack of a fresh break-up line (which indicates shattering on the probe). For the rest of their samples, artificial fragmentation could not be excluded. Moreover, their analysis included only crystals with stellar shape and sizes around 300 μm or roughly larger. This suggests that the frequency of collisional break-up in Arctic clouds is likely higher in reality compared to what is indicated in their study'

Line 69: Where it is written "*Both studies, however, focused on relatively warm polar clouds (-3°C to -8°C), where rime-splintering is also active",* the impression is conveyed that the H-M process is comparable to the ice-ice collisional breakup. But when one reads the papers cited one sees it was only weakly active. Clarify.
It is now clarified that rime-splintering was weak in both studies. However, in Sotiropoulou et al. (2020) the combination of both rime-splintering and collisional break-up was essential to explain observed ICNCs, while in Sotiropoulou et al. (2021) rime-splintering had hardly any impact.

Lines 56 and 57: Both lab/field studies by Vardiman and Takahashi et al. underpinned the Phillips et al. scheme and both involved some uncertainties. It would be a good idea to mention key issues with their experiments. For example:

- _First, the particles sampled by Vardiman were on a mountainside, apparently below cloud-base, and so there was likely some sublimation before impact, which may be have weakened them. Phillips et al. (2017) had to correct for this, by adjusting the fragility coefficient inside the exponential function of the scheme. It is a large correction.
- _Second, Takahashi et al. did not observe collisions between two riming particles, but rather observed a riming ice sphere colliding with an ice sphere predominantly in vapour growth (not riming). Thus, there are issues of representativeness. However, in real clouds, graupel falls in and out of zones rich in liquid, so the Takahashi-type collisions between graupel may be representative in a sense in view of the nonlinearity of ice multiplication.
- _Third, we do not have observations of columns or needles breaking up, so the Phillips scheme just treats them as if they are (non-dendritic) planars. It is not ideal.

Thank you for all these points! These key problems regarding the Vardiman and Takahashi et al. studies are now discussed in detail in the Introduction section. The simplification regarding the treatment of column and needles as planar ice is also explicitly stated now.

Despite such biases, Yano and Phillips (2011) argue that errors in the breakup rate per particle actually are not so important, because an explosion of ice concentration occurs anyway provided a threshold is surpassed. In future work, one hopes that MIMICA can predict rimed fraction somehow. It might be more accurate to say something to the effect that these quantities are not explicitly predicted by most cloud models currently.
The explicit treatment of rimed fraction is planned as the subject of future studies. However, the general low sensitivity of our results to rimed fraction (as long as sufficient snow formation is allowed) is very encouraging regarding the representation of this process in less detailed bulk microphysics schemes. This is now discussed in the 'Discussion' and 'Conclusions' section. However we acknowledge that the explicit prediction of rimed fraction is likely critical in conditions characterized by larger multiplication efficiency of the break-up process.

Line 71: The simulated range of in-cloud temperatures is stated. But it is more important to know the actual cloud-top temperature of the cases. So we are now simulating clouds with tops in the dendritic regime where we expect more fragmentation?

This statement is now modified to indicate the cloud-top temperature range: -9.5°C to -12.5°C. Both plates and dendrites can form in this range. -12°C is used as threshold in Phillips et al. (2017a) to separate the temperature ranges that likely favor non-dendritic or dendritic ice habits (with planar shapes being somewhat more likely).

4. Results

4.1 Sensitivity to ice habit

Line 288: There may be a typo or error here: "*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 …"*. Those two equations are for non-dendritic planars and dendrites respectively. A plate is a special type of (non-dendritic) planar. In this section, it needs to be mentioned that the non-dendritic planars occupy a wider range of temperatures than the dendrites (if this is so here), which boosts the impact from non-dendritic planars.

Thank you, the statement was indeed wrong and has been removed. In MIMICA the characteristic parameters in mass and terminal velocity relationships remain constant throughout the simulation. This means that the ice habit remains constant and does not change as a function of temperature. However the examined temperature range is generally limited anyway as mentioned in our previous reply to a comment above.

4.2 Sensitivity to rimed fraction

Line 358: Why is cloud-ice supposed to have as high a rime fraction as snow ? Riming does not start until sizes of a few hundred microns typically (PK97). Need to denote the size range of "cloud-ice" here.

Indeed this is a simplification. However snow is treated as aggregate in the default MIMICA version, which means that cloud-ice can freely grow to large sizes without necessarily being converted to snow (since cloud ice-to-snow autoconversion is not treated). Offline estimates of the mean particle diameter indicated two modes in the relative frequency distribution of this parameter (Figure 1). The first one indicates small cloud particles ~200-250 μm and the second one mm-particles (found in the lower portion of the cloud). The fact that an increase in rimed fraction only affect the second mode of the distribution suggests that it is the mm-particles that contribute to collisional break-up. This mode has a comparable size to the snow category, thus the simplification of assuming the same rimed fraction for both ice types is not unreasonable. This is now discussed in section 4.1 (note we have now merged the subsections that concern ice habit and rimed fraction).

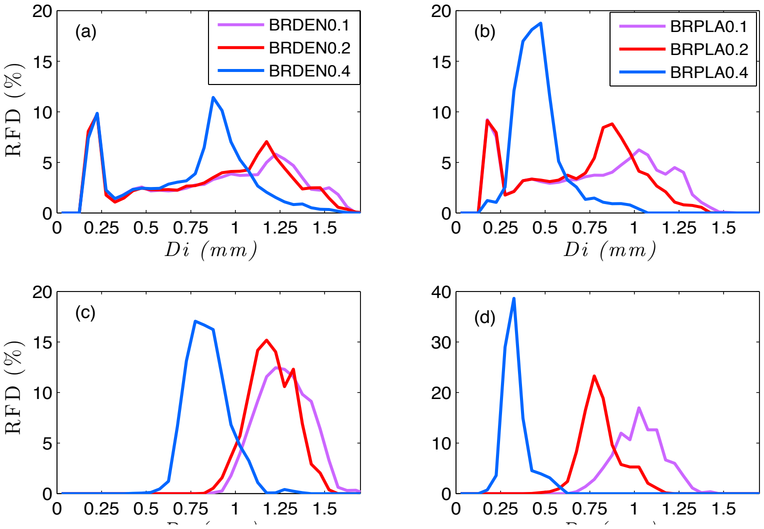

Figure 1: Relative frequency distrubtion of the mean (a, b) cloud ice and (c, d) snow diameter for simulations with (a,c) dendrites and (b, d) plates. Purple, red and blue lines correspond to a prescribed rimed fraction of 0.1, 0.2 and 0.4 for the cloud ice and snow particles than undergo break-up.

4.3 Sensitivity to autoconversion

What is the difference in microphysical processes that cloud-ice and snow are participating in? This seems to be the reason for the sensitivity of this size threshold. I think the best treatment of this autoconversion is from Ferrier (1992) as it preserves the slope parameter when converting cloud-ice to snow.

A summary of the interactions between liquid and ice particles is now offered in section 3.1. However we did find the reason behind the large sensitivity of the multiplication efficiency to the size threshold adapted for cloud-ice-to-snow autoconversion. As pointed out by reviewer 1, the characteristic parameters used for the graupel terminal velocity in the default MIMICA version are large (about one order of magnitude larger than in other stratocumulus schemes). Decreasing the a_v parameter by a factor of ~10 (adapted from Morrison et al. 2005) has a negligible impact on simulations that do not account for collisional break-up. However, since collisional kinetic energy impacts the multiplication efficiency, these changes have a substantial impact on simulations with active break-up. 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, where explosive multiplication occurs, the sensitivity of fragment generation rate is even larger: a maximum rate of 73.6 $L^{-1}s^{-1}$ was found in the old simulation, while now it has decreased to 12.84 $L^{-1}s^{-1}$. A notable 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 adapted, the enhancement can be significantly larger. This is why a low separation diameter (125 μm) for cloud ice and snow resulted in more multiplication than when adapting the 500-μm threshold and thus limiting break-up of snow; note that snow-graupel collisions are a main source of fragments (Figure 3). 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 321-322, while only results for the 500-μm threshold are shown in the relevant figures (note that autoconversion results are discussed in section 4.2 in the revised manuscript).

To conserve the highest moments of the ice particle spectrum, Ferrier et al. (1994) assumes that the number of cloud ice are approximately constant by converting only a few large ice crystals into snow. Thus snow formation does not prevent the accumulation of ice crystals within the cloud layer (since these are not depleted through the autoconversion process) and consequently does not prevent excessive multiplication and cloud glaciation. The simulations with the Ferrier scheme are not shown since they are similar to the runs without autoconversion; however the results are now discussed in section 4.2.

5. Discussion

Line 458: The rimed fraction noted in this sentence does not seem so low in actuality: *"Uncertainties in ice habit are in general not important as long as a low rimed fraction (~0.2) is assumed*". The Phillips et al. (2017a) scheme recommends a default value of 0.1 for the rimed fraction for snow > 1 mm being linearly interpolated to zero at sizes of 0.1 mm (cloud-ice). They actually simulated the rime fraction in their models and 0.1 was more or less what was predicted for a cold cloud-base.

Note that riming is treated differently among models. This is the reason why substantial differences in the distribution of cloud ice content among the different ice types is found for different models (Stevens et al. 2018). This is now discussed in section 3.3.3. MIMICA allows graupel to form from cloud ice particles as small as 150 μm, while accretion efficiency increases with size. Nevertheless, we added simulations with a prescribed rimed fraction of 0.1; the results are very similar to the simulations with $\Psi=0.2$.

Could there be some compensation of errors among different parts of the microphysics? It is possible that, although MIMICA now appears to be a fine model, the current state of knowledge in laboratory observations of ice microphysics is still limited. Any model is only as good as the empirical basis underpinning it.

Compensation errors are common in models, so this is possible. This can be particularly true for bulk microphysics schemes, where non-physical thresholds are used to separate cloud ice, snow and graupel particles; these thresholds are often tuned differently among different schemes. However this is something that cannot be inferred from our simulation results.

Need to mention possibility of other overlooked SIP processes also playing a role in Arctic clouds. See Field *et al.* (2017). For example, sublimational breakup might be important for Arctic clouds, since downdrafts only need to descend by a few hundred meters to go from being water saturated to ice saturated if adiabatic with constant vapour mixing ratio. There are other ideas, such as the notion of enhanced supersaturations in the wake of falling precipitation particles, which was mentioned at AGU this year.

We added a paragraph regarding the potential influence of sublimation break-up and blowing snow in the discussion section:

'Moreover, while processes like rime-splintering and drop-shattering are clearly ineffective in the examined conditions, the contribution from other SIP mechanisms has not been investigated, e.g. blowing snow and fragmentation of sublimating particles (Field et al. 2017). Sublimation of cloud ice particles can occur if cloud conditions become subsaturated with respect to ice; however a preliminary inspection of the domain-averaged supersaturation profiles did not reveal any such evidence. Furthermore, blowing snow is associated with relatively high wind speeds (Gossart et al, 2017), while during the examined ASCOS case the maximum wind speed never exceeded 5.2 m s^{-2} *in the boundary layer.'*

Unfortunately, currently we have no consensus about the possibility of activation of additional INPs in transient supersaturations in real cloud conditions

Do the present results accord with aircraft observations by Schwarzenboek et al. who published a histogram of missing branches per particle in Arctic clouds ?

Schwarzenboek et al. (2019) examined ice particles with sizes around 300 μm or somewhat larger and found that a maximum of ~5 fragments are generated per collision. However, they emphasize in their study that the findings are representative only for the specific flight conditions and cannot be generalized for any other ASTAR flights. Thus it is even more unlikely that these results are representative for ASCOS. In our simulations up to 13 fragments can be generated upon snow-graupel collisions, which is substantially larger than the findings in Schwarzenboek et al. (2019). However given that snow particles in MIMICA reach mm-sizes (Fig. 5), model estimations are not unreasonable. A related discussion has been added in the 'Discussion' section on lines 547-550, although no direct comparison between ASCOS simulations and ASTAR data can be conducted.

6. Conclusions

Line 535: Rimed fraction is noted as a poorly constrained yet very sensitive variable for the scheme. A problem here is that it is easy to predict rimed fraction explicitly: you just include a passive scalar for the rime on snow per unit mass of air and then diagnose the rime fraction as a function of size (see Appendix Aa of Phillips et al. 2017b (Part 2)). When will rimed fraction be predicted instead of prescribed in model development ?

Rimed fraction is not a very sensitive variable; simulations with dendrites give similar results independently of the prescribed rimed fraction. The only set-up that is very sensitive to rimed fraction is BRPLA0.4, thus only if highly rimed plates are assumed. This results in accumulation of many ice crystals in the cloud and eventually glaciation. But if the precipitation sink is enhanced through cloud ice-to-snow autoconversion in this set-up, the rimed fraction does not cause substantial changes in the cloud macrophysical state anymore.

The fact that our results show generally low sensitivity to the prescribed rimed fraction is positive news for larger-scale models, which employ bulk microphysics schemes that do not predict rimed fraction. Even more so, for climate model schemes like Morrison and Gettelman 2009 that do not even account for rimed particles (graupel). However, we acknowledge that this conclusion likely concerns only conditions with weak efficiency of break-up, as those examined here. Rimed fraction is expected to play a more critical role in more convective conditions and its explicit prediction is included in future model development plans. This is now discussed in the 'conclusions' section.

Appendix

When the Phillips scheme is applied, is there a temporary grid of size bins constructed so as to apply the breakup scheme for each colliding bin-pair?

The microphysics scheme already includes bulk descriptions for the interactions between the different ice types and within the same ice category, as aggregation is accounted for in the model. For consistency with the rest of the code, the same relationships are used to describe ice-ice collisions for ice multiplication. Thus a bulk (instead of a bin) approach is used for all processes in the model, including SIP. This is now explicitly stated at the beginning of the Appendix to avoid any confusion.