<u>Review of "Ice multiplication from ice-ice collisions in the high Arctic: sensitivity to ice habit, rimed fraction and the spectral representation of the colliding particles" by Sotiropoulou *et al.*</u>

<u>Verdict</u>

I recommend this paper be published after major revisions.

<u>Major Comments</u>

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).

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 ?

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.

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.

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.

<u>Detailed comments</u>

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). 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.

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”.

1. Introduction

Line 56: There is a missing reference: Fu et al. is cited but not listed.

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.

Line 69: Where it is written “*Both studies, however, focused on relatively warm polar clouds (-3^{o}C to -8^{o}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.

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.

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.

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 ?

2. Field observations

This is fine.

3. Ice formation in MIMICA

This is fine.

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.

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.

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.

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.

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.

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.

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

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 ?

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 ?