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Interactive comment

Interactive comment on "The impact of Secondary Ice Production on Arctic Stratocumulus" *by* Georgia Sotiropoulou et al.

Anonymous Referee #1

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Verdict

I think the paper should be published subject to major revisions.

Major Comments

The structure of the results section could be improved. First, it would be better if there were a comparison of a control simulation with observations (model validation). The control run should be the most realistic of the simulations that the authors can manage, which would be expected to include breakup. Second, once the control run is validated, then the sensitivity tests should be shown, excluding the various processes. Also the description of the simulations with various IN assumptions is vague, with the temperature of each active IN concentration being not mentioned. Also, it is a struggle

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Discussion paper

to reconcile the model with observations in Fig. 9b. One or two simulations without breakup seem more accurate than those with breakup. Yet in the abstract you write the inclusion of breakup brings the model into agreement with observations for the case.

Detailed Comments

Abstract

The term “droplet-shattering” is used where I think it would be more accurate to say “drop-shattering” or “rain/drizzle-drop shattering”. The type of shattering that the authors refer to is of drops > 0.05 mm in diameter, while cloud-droplets are typically smaller than this. Cloud-droplets are < 0.05 mm diameter and are observed not to shatter or splinter.

1. Introduction

Line 168: I thought Savre had developed an ice nucleation scheme with the MISU group. So I wonder why it is not being applied here.

2. ACCACCIA

Page 4, “between 10-11 UTC” should be “between 10:00 and 11:00 UTC”.

3. Models and Methods

It is written “Ice nucleation is also parameterized following Morrison et al. (2011): if ICNCs fall below the prescribed INP concentration (NINP), they are nudged upward towards the INP value. ” But this seems less accurate than tracking the number concentration of IN lost by activation with a separate prognostic variable, as pioneered by Cohard and Pinty in the 1990s. Computational cost would be minimal. To avoid confusion, it would be a good idea to paraphrase that the “active IN concentration” (I prefer this phrase over “INP” concentration since it is self-evident that the IN is a particle and what is important is the activity spectrum; there is no single number for the concentration) is prescribed from the DeMott 2010 parametrisation informed by total aerosol measurements of the ACCACIA case.

The DeMott scheme has no dependence on aerosol chemical composition and size. The scheme implicitly assumes that only dust is the IN species, since concentrations in the measurements setting up the DeMott scheme originally involved dust dominating the sizes > 0.5 micron. How does one know that bio-IN were not dominating the IN activity in this case ? Or soot from biomass-burning ? One wonders if another scheme with aerosol chemistry/size dependencies might be more accurate.

A sensitivity test with respect to choice of IN scheme would be a good idea.

There must have been IN measurements in the Arctic in different years, so it would be best to include in the paper a plot of the active IN vs temperature comparing your scheme with the IN measurements from other Arctic campaigns in summertime of various years.

The breakup scheme is based on Takahashi 1995. But they observed collisions between two giant ice spheres (2 cm), one of which was rimed. Phillips et al. (2017a) when building their breakup scheme interpreted these as representing graupel-graupel collisions because the bulk density of the colliding spheres was that of pure ice, not graupel-snow collisions. Can the author comment on this ? Have the authors rescaled the Takahashi data to account for the typical sizes of the graupel in Arctic clouds.

The breakup scheme by Phillips et al. (2017a) is based realistically on collision kinetic energy and temperature, with different treatments for each permutation of species of collisions (graupel-graupel, graupel-snow, snow-snow) etc. It would be better for the authors to upgrade their treatment of breakup.

The authors write “The mean observed INP concentration is 0.006 L-1 and never exceeds 0.05 L-1, while the mean and maximum observed ICNC for the same period is 1.43 L-1 and 17.8 L-1, respectively, suggesting substantial ice multiplication. ” But the authors need to say what conditions of temperature and humidity are used to define these active IN concentrations.

4. Results

The conclusion stated in the Abstract is plausible: “In contrast, break-up enhances ICNCs by 1-1.5 orders of magnitude, bringing simulations in good agreement with observations”. However inspection of Fig. 9b comparing predicted and observed ice concentrations shows that the control run without ice multiplication is an order of magnitude too low and with it is an order of magnitude too high.

It seems confusing that the run without ice multiplication is referred to as the “control” and is depicted with a dashed line rather than a full line.

I wonder if the over-prediction of breakup is due to inaccuracy in the formula. Were the Takahashi observations re-scaled for the smaller particles relative to the lab experiment ? Takahashi did when he applied his own lab data to provide estimates for natural clouds.

5. Discussion

The paper by Schwarzenboeck et al. (2009) was seminal and totally relevant as a motivation for the present study. So, there needs to be a more thorough description of their analysis and how they arrived at their estimate of about half (20-80%) of all ice particles being naturally fragmented. They were aware of the shattering bias issue quantified by Field et al. and Korolev et al., and did a diligent study. A few more sentences describing the paper are needed.

Line 449: The comment about the fallout time-scale not being objectively defined could be misinterpreted. What the authors intend to say is that in their own model, the fall-out time-scale can have values in a wide range (there is a similar timescale parameter in the Yano-Phillips theory).

An order of magnitude estimate of the ‘multiplication efficiency’ (tilde c) for breakup in the model would be helpful, using the formula for it from Yano and Phillips (2011). Although their theory was originally for graupel-graupel collisions, Phillips et al. (2017b) argued it also applies to graupel-snow collisions with a few changes of the parameters. The multiplication efficiency then implies a time-scale for the growth of ice concentration. Does the simulated time-scale of the explosion match the modified theory ?

Does the theory predict that the Arctic clouds simulated is in the unstable regime of the phase-space ?

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