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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Impacts of Secondary Ice Production on Arctic Mixed-Phase Clouds based on ARM Observations and CESM2" *by* Xi Zhao et al.

Anonymous Referee #1

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This manuscript implements new parameterizations of secondary ice production (SIP) including frozen raindrop shattering, ice-ice collisional breakup and the classic Hallett-Mossop parameterization by Cotton et al. 1986 in the CAM6 single column model (SCM) to determine the impact of SIP on Arctic mixed-phase cloud properties compared to observations from the M-PACE field campaign. The authors conclude that SIP reduces the high supercooled liquid bias in Arctic single-layer stratiform mixed-phase clouds and improves the low bias in ice crystals at relatively warm temperatures below the supercooled liquid clouds. They also find that the fragmentation of freezing droplets contributes most to ice production in single-layer boundary layer clouds, while ice-ice collisions and rime splintering contribute relatively less to ice production

in frontal clouds and multilayer stratus clouds, respectively, and primary ice production is more important for cold multilayer and frontal clouds in CAM6 SCM.

The manuscript presents novel results, is important for the improvement of climate models and is very relevant to Atmospheric Chemistry and Physics. I would recommend publication after major revisions.

My most major concern is regarding the M-PACE in situ data that was compared against the CAM6 SCM. These data did not correct for the shattering effect which is known to severely overestimate the ice number concentration by up to two orders of magnitude and likely cause misleading conclusions if not accounted for (Korolev et al. 2011, Korolev et al. 2013a, Korolev et al. 2013b, Korolev & Field 2014). I strongly recommend that the authors use data that have corrected for the shattering effect using both correction algorithms using interarrival time and data that have used antishattering tips.

Also, despite apparent better agreement with M-PACE (noting that the M-PACE data for ice number concentration are incorrect), the poor agreement with ice properties were not noted in the conclusions and abstract and underemphasized in the manuscript. Please revise accordingly.

Minor revisions: - Section 4.1.1: A discussion of why the ice properties are so poorly represented in the model is much needed. Please include.

- What are the initialization and forcing conditions of the model?

- The title would be more accurate if "CAM6 single column model" is used in place of CESM2. Please modify.

- Please include more information about the formulation of the Hallett-Mossop parameterization.

- Lines 35-39: not only is the cloud radiative effect important but also the impact of Arctic cloud properties in climate change scenarios: Vavrus 2004, Zhang et al. (2018), Tan & Storelvmo 2019.

- Line 422: there are more references related to this than the single ones mentioned for the CAM3/CAM5 model: e.g. Klein et al. 2009, Cesana et al. 2015, Tan & Storelvmo 2016, Zhang et al. 2019, Tan & Storelvmo 2019.

- Figure 4a: Why does LWP decrease in the SIP_PHIL experiment? Is this related to the Bergeron-Findeisen process?

- Figure 4b: consider using a nonlinear scale to improve visibility of small values.

- Figure 5 and lines 352-353: To my eye, it is not clear that the "decoupling" is much improved in the SIP simulation; also has a typo in bottom row, should be "LWC", CF is not labelled in first column.

- I suggest Figure 12 go to the Supplementary Info.

- In terms of the writing style, in general, there are too many short subsections that might be better combined into a broader section. Also, the grammar could improve.

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