REVIEW FOR:

Impacts of Secondary Ice Production on Arctic Mixed-Phase Clouds based on ARM Observations and CESM2
By Xi Zhao et al.

General Comments:
This study explores the impact of three secondary ice production (SIP) mechanisms on Arctic clouds observed during M-PACE in CAM6. CAM6 already includes a description of the Hallet-Mossop process, while the authors have implemented two additional mechanisms: drop-shattering and collisional break-up. Their results indicate that the additional parameterizations improve the representation of Arctic clouds, by reducing biases in liquid and ice content. Moreover, both the vertical distribution and magnitude of ice crystal number concentrations is improved with the activation of SIP. Drop-shattering is found to be most important SIP mechanism in boundary layer clouds, while primary ice nucleation dominates ice formation in deep cold clouds. The study suggests that including additional SIP mechanism in global climate models can substantially improve the representation of mixed-phase Arctic clouds. Given the fact that the poor microphysical representation of these clouds in GCMs is a main source of uncertainty in future projections of the Arctic climate, the scientific impact of the study is significant and thus I recommend it for publication. My only main comment concerns the technical implementation of the processes and to which extent this is consistent with the rest of the Morrison-Gettelman (MG) microphysics scheme.

Main comment:
In the standard MG scheme snow-snow collisions, snow-ice collisions and snow-rain also lead to aggregation/accretion. Are these processes still active in the modified scheme? For example pracs and npracs is the accreted mass and number concentration predicted by the scheme for snow-rain collisions. Are these parameters generally consistent with mass and number predicted by the bin framework? Please provide details about how existing collisions in the standard scheme are combined with additional parameterizations to ensure consistency in mass transfers.

Minor comments:

Section 2.2b:
- How parameter 'rim' is treated? Not explained.
- *Figure 1:* a planar or a dendritic ice habit was eventually assumed in the presented simulations, since MG does not predict shape?
- *Equation 4:* why sticking efficiency is included in the calculation? (I assume the default 0.5 value of MG is applied). I think mechanical break-up occurs when ice particles grow rimed branches that break after collisions with other frozen

hydrometeors. Is accretion/aggregation a prerequisite for this mechanism?

Section 2.2c:

- Why rhoi is set to 920 kg/m^3 for this process and not be consistent with the rest of MG code? I think rhoi is set to 500 kg/m^3 in the default model version. Unless here it set to 920 kg/m^3 for the whole scheme and not only for this particular process.
- Big fragments are added to snow or cloud ice? Please clarify.
- Is a minimum raindrop size threshold used in mode 2 for the process to be activated? I think in Phillips et al. (2018) a minimum size of 150 μm is assumed to initiate the mechanism. Anyway, if no threshold is used, please clarify.

Section 3.2:

Please state the instruments' uncertainty. Also different retrievals seem to have been applied for the measured variables in Figure 4. Please provide the corresponding references in this section. It would also be nice if a short description on the differences between these algorithms is provided here, along with the estimated uncertainty for each retrieval.

Section 3.3:

What happened to the CTL_no_HM experiment? I cannot find relevant results in any of the Figures or Tables. It would be very interesting to include this simulation in the paper too.

Section 4.1:

Lines 326-327: The simulated LWP is overestimated during the "multilayer stratus" and "frontal cloud"

Actually LWP in Fig 4a seems simulated reasonably well, especially in SIP_PHIL. I would say that it is in the second half of the single-stratocumulus period that LWP is substantially overestimated

Section 4.1.3:

Maybe LWC should be also shortly discussed in this section, since it is shown in Figure 7

Section 5:

Lines 558-560: I think MG assumes that rime-splintering only occurs when cloud droplets collide with snow. While for example Morrison scheme also includes raindrop-ice interactions in the Hallet-Mossop process. This means that the overestimation in the H-M efficiency due to lack of size dependency might be balanced by an underestimation of the raindrop splintering production in MG. Also I wonder about what is the impact of the fact that the bulk approach is used to represent H-M, while a bin approach is used for the rest of the processes. Would a 'bin representation' increase the efficiency of H-M? I am not suggesting that the authors should also adapt a bin approach for H-M for consistency, but it would be really interesting to know how this modifies results. Nevertheless this is something that could be discussed along these lines.