Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC3
https://doi.org/10.5194/acp-2021-686-RC3, 2021


# Comment on acp-2021-686

Anonymous Referee #3

Referee comment on "Primary and secondary ice production: interactions and their relative importance" by Xi Zhao and Xiaohong Liu, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-686-RC3, 2021

Review for "Relative importance and interactions of primary and secondary ice production in the Arctic mixed-phase clouds" by Zhao & Liu

This manuscript compares the impacts of primary ice production (PIP) and secondary ice production (SIP) as well as their interactions on the simulation of multiple Arctic mixed-phase cloud microphysical and macrophysical properties observed during the M-PACE field campaign. The authors design a set of 10 simulations, 5 of which differ only in their treatment of ice nucleation schemes and the other 5 which utilize the same 5 aforementioned ice nucleation schemes but with representations of SIP via the ice-ice collisional breakup (IC) and rain droplet fragmentation (FR) mechanisms in addition to the Hallett-Mossop process which is represented in all 10 simulations. The authors find that 3 of the ice nucleation schemes that are aerosol-aware (CNT, N12 and D15) exhibit similar behaviour to each other in terms of their simulated ice crystals number concentration vertical profiles, supercooled liquid fraction (SLF), IWP, LWP and relative contributions from primary and SIP rates to the total ice production rate. They also find that these variables are also similar to each other for the other two ice nucleation schemes (B53 and M92). One of the main is that PIP and SIP actively influence each other. The authors also conclude that the aerosol-aware ice nucleation schemes with the IC and FR mechanisms represented best represent the single-layer mixed-phase clouds observed during M-PACE.

This is an interesting and valuable study at the forefront of effort to improve cold cloud microphysics in global climate models and their impact on cloud properties. There are however, a number of ways that the manuscript can be improved, particularly pertaining to the writing including the description of the model used and the experimental design,

description of the observations and grammar. Overall, I recommend major revisions that are provided below.

Major revisions:

- The title is wordy and unclear. Perhaps revise to something like "primary and secondary ice production: interactions and their relative importance"?
- An interesting conclusion of this manuscript is the interaction between SIP and PIP which compete with one another. The suppression of SIP via PIP is intuitive, however, the suppression of PIP via SIP is less intuitive since one would initially expect that more ice crystals nucleated via PIP would allow more SIP. The explanation for the latter phenomenon provided in the manuscript relates to the lack of precipitation particles in B53 and M92 due to the enhanced glaciation of mixed-phase clouds. A description of the graupel scheme (which seems to be diagnostic based on line 364) the authors implemented would help the readers more clearly understand the mechanism instead of referring to Zhao et al. 2021. The mechanism of SIP and PIP suppression could also be summarized in the Abstract. Also, the discussion on lines 73-78 in the Introduction can also be elaborated on in this aspect when describing the work of Phillips et al. 2017b.
- An 80% contribution of SIP to total ice formation seems very large. Are these any observations to gauge how realistic this value is? Similarly, on lines 297-301, are there any observations to gauge how realistic these contributions are? Otherwise, this should be declared in the main text.
- In addition to the graupel implementation mentioned above, the description of the ice nucleation schemes could also be described in more detail. All ice nucleation schemes appear to be implemented as immersion freezing schemes --- please confirm. How are deposition, condensation, and contact freezing represented? To be consistent with the other naming conventions used in the manuscript, I would also recommend changing "CNT" scheme to reflect the reference that was used (was it Wang et al. 2014 or Hoose et al. 2010)? The description of this scheme also does not include the equation and the units of all equations that are provided are missing. For N12, is the dry diameter of dust particles predicted by MAM4? For the D15 scheme, please include more information on the instruments that were used for the measurements and the location where the observations were taken from. To be clear, are marine organic aerosols and sea salt not included as INPs in any of the parameterizations? Please include in the description.
- Lines 96-97: It would be better to clarify that this is the case for the default CAM6 model with MG2 microphysics.
- More on the model description: line 165: What were the aerosols initialized with in SCAM and what are the aerosol types that are represented? Line 168: what aerosol-cloud interactions are represented? g. Twomey, Albrecht, glaciation indirect effect, etc.? Lines 171-172: can the cloud-borne aerosols released as interstitial aerosols be reactivated? Were the simulations not free-running or nudged to MPACE meteorology?
- Line 194: please cite the original source of the observations. The ground-based

observations are not directly comparable with the model and should be stated.

- Line 200: Dividing by a factor of 4 seems very approximate to account for shattering effects. I would suggest using a dataset that has been revised according to the interarrival times for more accurate comparisons (Korolev et al. 2015)
- Why don't B53, B53_SIP, D15 and D15_SIP not appear in Figs. 1 and 2? Please include. Please also include the observations in Fig. 2.
- Fig 5: I find the "enhancement ratio" confusing because the relative enhancement in Figures b-j are compared relative to Figure a, but they all use the same colour bar. Wouldn't it make more sense to use a separate colour scheme for b-j?
- Please include error bars in the observations and all simulations.

Minor revisions:

- Line 12: "of" needed after "importance"
- Line 32-34: another source of ice particles in mixed-phase cloud could be from ice crystals that fell from overlying cirrus clouds.
- Lines 42-43: Ice crystal fall speed is a cloud microphysical process that is also quite important for mixed-phase cloud properties such as SLF according to the CAM5 model shown by Tan & Storelvmo 2016.
- Line 70: "Albeit these studies, how…" is grammatically incorrect.
- Line 188: "rather than" I think should be "in addition to" since Hallett-Mossop is included in all simulations?
- Line 248: suggest replacing "in accompany with" with "accompanied by" and again on line 409.
- Line 370: add "rate" after "nucleation"
- Lines 423-426: Not necessary to discuss here since there is no associated figure and discussion and not central to the manuscript?

References:

Tan, Ivy, and Trude Storelvmo. "Sensitivity study on the influence of cloud microphysical parameters on mixed-phase cloud thermodynamic phase partitioning in CAM5." *Journal of the Atmospheric Sciences* 73.2 (2016): 709-728.

Korolev, A., and P. R. Field. "Assessment of the performance of the inter-arrival time algorithm to identify ice shattering artifacts in cloud particle probe measurements." *Atmospheric Measurement Techniques* 8.2 (2015): 761-777.