

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-686

Anonymous Referee #2

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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-RC2>, 2021

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Review of Manuscript # acp-2021-686 in ACPD: "Relative importance and interactions of primary and secondary ice production in the Arctic mixed-phase clouds" by Zhao and Liu.

### General comments:

The authors examined five different ice nucleation schemes and secondary ice production (SIP) processes in the simulations of Arctic mixed-phase clouds during the M-PACE campaign using single column mode of CESM2 CAM6 model. They concluded that the simulations using aerosol-aware ice nucleation schemes and including SIP processes resemble the observed single-layer mixed-phase clouds during the M-PACE. In these simulations, SIP plays a key role, and there is a competition between ice nucleation and SIP. Overall, the manuscript is well organized, and the logic is clear. However, there are several concerns that should be clarified before considering the manuscript for publication. The reviewer would recommend major revision for this manuscript in case the authors need more time for revision.

### Major comments:

- Analyses: The analyses in the manuscript are full of qualitative phrases. Some examples are listed in the minor comments. Please conduct quantitative analyses.
- How did the authors attain the simulated ice crystal number concentration (ICNC) for comparison with observations? Did the authors consider snow particles? Because observations should include all types of ice particles, the authors should include all ice categories for comparison. Meanwhile, in the comparison only the observed ICNC with sizes larger than 100 microns are considered, while the entire size range of simulated ICNC is used. So, the comparison is also unfair. Please use the same size range of all types of ice particles for comparison.

- Lines 199-203: "M-PACE observed ICNCs were scaled by a factor of 1/4", have the data collected by the authors been scaled by a factor to remove the shattering effect during the data quality control? Are the conclusions sensitive to this correction factor?
- The reviewer was surprised as the results shown in Figures 1 and 2. N12 (N12\_SIP) seems to be the same as CNT (CNT\_SIP), but their INPs are obviously different in Figure 3. Why?
- It is not clear that how the authors attained the INP number concentrations from observations and simulations especially for B53 scheme. Did the author conduct a fair comparison between them? Did the authors include all types of ice nucleation for comparison? Please provide a more detailed description.
- "Section 4.3 Interactions between PIP and SIP": SIP suppressed the PIP. Did the authors consider whether some setups in the microphysics scheme lacking physical meaning result in or enhance this suppression? For example, suppression is due to decreasing difference between total ice nucleation number from parameterization and increasing ice particle number. Please provide a discussion.
- Some "rate"s in the manuscript are confusing. If the reviewer understood correctly, the production rates in the manuscript are mainly for ice mass based on Figures 8-10. The question is how IIC increases ice mass? The "ice" in the manuscript all means "cloud ice" and does not include "snow"? If yes, following comment #2, different categories of ice are defined artificially in microphysics schemes, and it might not be true in observations. The authors should clarify it. The reviewer would recommend conducting analyses including simulated snow particles.

#### Minor comments:

- Lines 107-108: Please describe how the graupel mass and number are diagnosed in the scheme briefly.
- Lines 209-225: Please quantify the analyses, e.g., percentage of enhancement, reduction, "largest", "smallest", "modest", "closest", "significantly decreases/increases", ...
- Lines 233-234: "appears an inversely linear relationship", "this relationship is not as clear", do they have statistical significance?
- Lines 234-238: Please quantify the analyses, e.g., "reduces dramatically", "much higher", ...
- Lines 253-264: Why is SIP not active in B53\_SIP and M92\_SIP? Is there a maximum threshold of ICNCs in the microphysics scheme?
- Line 269: Please quantify "slightly higher"
- Lines 281 and 283: Please quantify "overestimate", "predominantly"
- Lines 287-288: How about the TWC in these simulations?
- Lines 294-308: It is confusing whether the authors talked about ice number or mass in Figure 7. If the authors talked about ice mass in Figure 7, how do IIC contribute to ice mass?
- Line 328: Please quantify "substantially weakened"
- Lines 342-343: Based on Eq. (5), M92 seems dependent on supersaturation not temperature and cloud droplet number concentration.
- Lines 362-367: Please quantify the analysis.
- Figure 1: Please provide uncertainties of these observations.
- Figure 4: How did the authors determine the cloud top and cloud base for observations and simulations?
- Figure 5: x-axis in (h), "CTL" -> "CNT"? What are the bin sizes for x and y variables?
- Figure 7: "total ice production rate", is the "production rate" for mass or number?

- Figure 9: "(h) accretion rate of cloud water by snow", how about the accretion of rainwater by snow?
- Figure 10: (c), (e), (F), IIC influences the mass mixing ratio of ice particles?