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Comment on acp-2021-686

Anonymous Referee #1

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-RC1, 2021

Review of "Relative importance and interactions of primary and secondary ice production in the Arctic mixed-phase clouds" by Xi and Liu in ACPD, 2021.

In this work, the authors contrasted several parameterizations of primary ice production (PIP), combined with a new set of parameterizations of secondary ice production (SIP) in the NCAR CESM2/CAM6 model. The model simulations are compared with observations from the DOE M-PACE campaign. The scientific questions include: What are the impacts of SIP parameterizations on the simulation results? What are the effects of SIP on PIP? How does the PIP process influence SIP? As the authors mentioned, the interactions of SIP and PIP have not been carefully examined before, and the mechanisms of how they affect each other are still unclear.

Overall, this is a well-written manuscript. It is very easy to follow the simulation experimental design since the logics are very clear and straightforward. The reviewer recommends that the paper be accepted after a minor revision on the following points.

Main comments:

1. About the comparison of ice crystal number concentration (ICNC) between observations and simulations, the observations are restricted to > 100 micron, while the simulations use the entire size range from zero to infinity. Since ICNC is dominated by smaller ice particles, the simulations may overestimate ICNC when a wider size range is used. The reviewer suggests a revision on the simulation dataset to delete ice particles < 100 micron. In addition, a scaling factor of 1/4 is applied to the observations due to potential ice shattering effect. But as the author mentioned, previous studies showed that the scaling factor may be around 1 to 1/4.5. Thus, using 1/4 seems to provide a lower end of ICNC from observations. If the authors apply another scaling factor, such as 1/2, how will it change the result? Some discussions on this sensitivity test can be added.

2. Another main comment is about the mechanism used to explain how introduction of SIP leads to weaker PIP. The authors described this mechanism around line 339 - 346, that is, “Since temperature and supersaturation are similar in these nudged simulations, the decreased cloud droplet number concentration with the introduction of SIP leads to weaker PIP in B53_SIP and M92_SIP”. Can the authors clarify which variables in the SCAM simulation are nudged, such as temperature, U and V wind? Is the specific humidity nudged as well? The reviewer tries to understand why ice supersaturation is similar between the two simulations. If there are more ice crystals produced by SIP, these ice crystals could provide more deposition of water vapor to ice phase, and thereby relaxing ice supersaturation back to ice saturation faster. Then it could lead to a suppression of PIP when ice supersaturation frequency and/or magnitude is reduced, since PIP requires a certain magnitude of ice supersaturation to occur. Also, are the ice crystals formed from SIP able to provide seeding for lower levels when they sediment? Can the seeding lead to suppression of PIP?

3. Following the previous comments on Figure S7, some parts of this figure do not make sense to the reviewer. For example, accumulation mode dust decreases at 880 – 1000 hPa, but increases at 880 – 700 hPa in N12_SIP compared with N12. Why does the accumulation mode dust increase at 880 – 700 hPa in N12_SIP, if the mechanism of SIP is to increase wet deposition (line 341)? In addition, the accumulation mode deposition in panel (e) only significantly increases in N12_SIP near the surface around 980 – 1000 hPa. This pressure level does not match with the location of changes seen in panel (d), and the increasing deposition doesn’t explain the increase of accumulation mode dust at 880 – 700 hPa as mentioned above. The change of coarse mode deposition also doesn’t match with the vertical locations of changes seen in coarse mode dust. Can the authors explain this figure a bit more?

Some minor comments on Figure S7, the (d) panel x axis label is out of bound on the page. Also some x axes are suggested to use the same range for an easier comparison. For example, c, d, and e can use the same scale and unit; f and g can use the same scale.

4. In several analyses, the authors use relative altitude to the cloud layer, that is, 0 refers to cloud base and 1 refers to cloud top. There is no discussion about how this relative altitude is derived. Is it derived based on ground-based observations or in-situ observations? Please clarify.

Minor comments:

1. Several simulations, CNT, N12 and D15, as well as CNT_SIP, N12_SIP and D15_SIP, provide similar results to each other. Can the authors provide some explanations why

these three PIP parameterizations provide very similar results? Is it because they were derived based on similar observation data?

2. Some of the analyses and figures are based on ground-based remote sensing observations (such as Figure 1) while the other ones are based on in-situ aircraft observations. It would be beneficial to clarify in Section 3, such as line 182, which type of observations is used in a specific figure or analysis.

3. Please clarify how the variables related to “rate” are defined in the model. For example, is the variable ice production rate describing the amount of ice crystals (in kg) being produced in every unit mass of dry air (in kg) per second in the entire grid box, or only in the in-cloud section of the grid box?

4. Figure 2, is it possible to add sub-panels of observations to compare with the model results?

5. Figure 3, since the INP concentrations in CNT, N12 and D15 are significantly lower than the observations, can the authors apply a scaling factor to INPs in these parameterizations to match with the observations better, and see how the results change? Also, the reviewer wonders why with such low INP concentrations, these parameterizations are able to produce quite a similar amount of ICNC compared with observations?

6. Figure 5, please clarify how the normalization was calculated in this figure. It seems that the PDF is calculated by the number of samples of each bin divided by the total number of samples in each temperature bin (the sum of % in each temperature range equals one), instead of divided by the total number of samples of the entire temperature range. Is that correct? The reviewer wonders how this figure will change, if the latter type of normalization is also provided (i.e., the sum of % in all bins equals one).

7. Figure 9, for the accretion rate of cloud water by snow, does cloud water include both cloud droplets and rain? Some minor revisions on the sub-title of g and h are recommended. For example, h can be “Droplet number” instead of “Cloud number”, and h can be “Accrete water by snow”.