

Review of Sensitivity of precipitation formation to secondary ice production in winter orographic mixed-phase clouds

Sylvia Sullivan (Referee)

Referee comment on "Sensitivity of precipitation formation to secondary ice production in winter orographic mixed-phase clouds" by Zane Dedekind et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1326-RC1>, 2021

Dedekind et al. implement secondary ice parameterization into a nonhydrostatic model and investigate the impact on simulated ice crystal number concentrations and microphysical tendencies, as well as precipitation patterns, over a region with significant topography and relative to a variety of measurements. This study is an important contribution to clarifying the interactions between ice microphysics, particularly poorly constrained secondary production processes, and surface precipitation intensity. The figures are beautifully done. I have some major comments, primarily to improve the readability of the results sections and further justify some statements there. A number of minor comments are also included in an annotated PDF.

Major Comments

- While the processes (e.g. temperature dependence and mechanism) of rime splintering and collisional breakup are described in lines 48-57, frozen droplet shattering is only mentioned in passing in the introduction (lines 69-72). Even if frozen droplet shattering is not influential in this case, I would still devote another sentence or two to describing it after the other SIP processes.
- I had difficulty understanding the aerosol treatment described in lines 137-145. Was a representative aerosol profile derived from the values in different temperature ranges cited (> 261 K, 258-261 K, etc.)?
- I struggled to get the takeaways from Sections 3.1.1 and 3.1.2. Three suggestions in this regard.
 - - You start by discussing the ice crystal shape classification in lines 231-235 but then transition to modeled versus measured. I would move anything about the shape classification to where you discuss it further (lines 257-272). Given how little rime splintering changed the simulated ICNC, is it not surprising how large the percentage of observed rimed crystals is in Figure 3b? I felt Fig. 3b warranted more discussion.
 - - It would be easier to make the model-measurement ICNC comparison visually if

the values of Figure 3a were actually atop those of Figure 4a.

- - It was not clear to me what changed between 12:00 and 13:00 UTC (i.e. Figs 4 and 5). In particular, I thought it was interesting that γ_{BR} controls the vertical structure of ICNC and secondary production rates in Fig. 5 but not Fig. 4. Do you have a hypothesis why this is so?
- Given the discussion of updraft throughout (e.g. in regard to WBF or around lines 305-313), profiles or maps of vertical velocity would be helpful to see whether reduced precipitation in the breakup simulations is due primarily to microphysical or dynamic factors or both.
- I would choose a metric other than the spatial Pearson correlation coefficient for precipitation evaluation; otherwise, the r^2 values in Table 3 seem to contradict the statement that "COSMO benefits from the inclusion of collisional breakup processes in simulating precipitation." You could, for example, calculate the statistical distances between the distributions shown in Figure 8 with the Kullback-Leibler divergence. There is a scipy Python package here:
https://docs.scipy.org/doc/scipy/reference/generated/scipy.special.kl_div.html

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2020-1326/acp-2020-1326-RC1-supplement.pdf>