Review for: Impact of ice multiplication on the cloud electrification of a cold-season thunderstorm: a numerical case study, by Yang et al.

The reviewer thanks the authors for their diligent efforts in addressing the comments. The revised manuscript has significantly improved, and only a few minor suggestions are outlined below:

1. A general comment is that the revised manuscript now includes 21 figures, which makes it challenging for readers to focus on the key takeaways. I recommend considering the relocation of certain figures – particularly those not central to the paper or discussed minimally in the manuscript (e.g., Figure 1, 2, 11, 12) – to the Supplementary Material in order to enhance clarity.

Reply: We appreciate your comment. According to this comment and comment 3, Figures 11 and 12 have been removed from the manuscript. Now there are 19 figures. Figures 1 and 2 show in detail the weather conditions of this winter thundercloud, we prefer to keep them in the text because we think they are important for case description.

2. In line 230 you mention "the good performance of WRF": considering the observed discrepancy between model and observations illustrated in Figure 5, using a phrase like "composite reflectivity is simulated reasonably well" might be more accurate.

Reply: Thank you for your comment. "The good performance of WRF in modeling the composite reflectivity and the improvements by SIP provide us the confidence to investigate …" has been changed to "Based on the facts that composite reflectivity is simulated reasonably well and the SIP processes result in improvements, we are confident to investigate …".

3. Related to the comment #1 raised above, I am not sure whether Figs. 11 and 12 contribute significantly to the paper, especially considering the absence of measurements for comparison. Figs. 8 and 9 seem sufficient to discuss the WRF sensitivity to various SIP processes. Additionally, the production rates of SIP processes presented in Fig. 13 seem more valuable for interpreting the observed ice enhancement than Figs. 11 and 12.

Reply: We agree. Figures 11 and 12 have been removed from the manuscript, and the related text are revised.

4. When activating all SIP mechanisms in the 4SIP sensitivity simulation, you mention that the ice enhancement "maybe weaker than the impact of a single SIP process" (Lines

313-314). Why do you think this happens? I propose considering the addition of a subplot in Fig. 13 to illustrate the synergistic impact of all SIP processes in the 4SIP simulation. Contour lines can be used to indicate with different colors when each SIP mechanism included in 4SIP surpasses a predefined threshold (for example: 0.1 #/L/s for rime splintering, 0.01 #/L/s for droplet shattering or collisional break-up and 10(-4) for sublimational break-up). This subplot could reveal whether one SIP process dominates, potentially reducing the cloud liquid water content and thereby diminishing the impact of the remaining mechanisms.

Reply: Thank you for your comment. The figure is updated accordingly, which shows the temporal variation of the mean ice production rate by different SIP mechanisms. By using the thresholds you suggested, the figure looks messy, thus we use a single threshold of 3×10^{-3} $L^{-1}s^{-1}$. The results show the rime-splintering dominates the secondary ice production between 0 and -10 C, potentially reducing the cloud liquid water content and thereby diminishing the impact of the remaining mechanisms. The ice production rate by sublimational breakup is so small that it never meets the threshold.

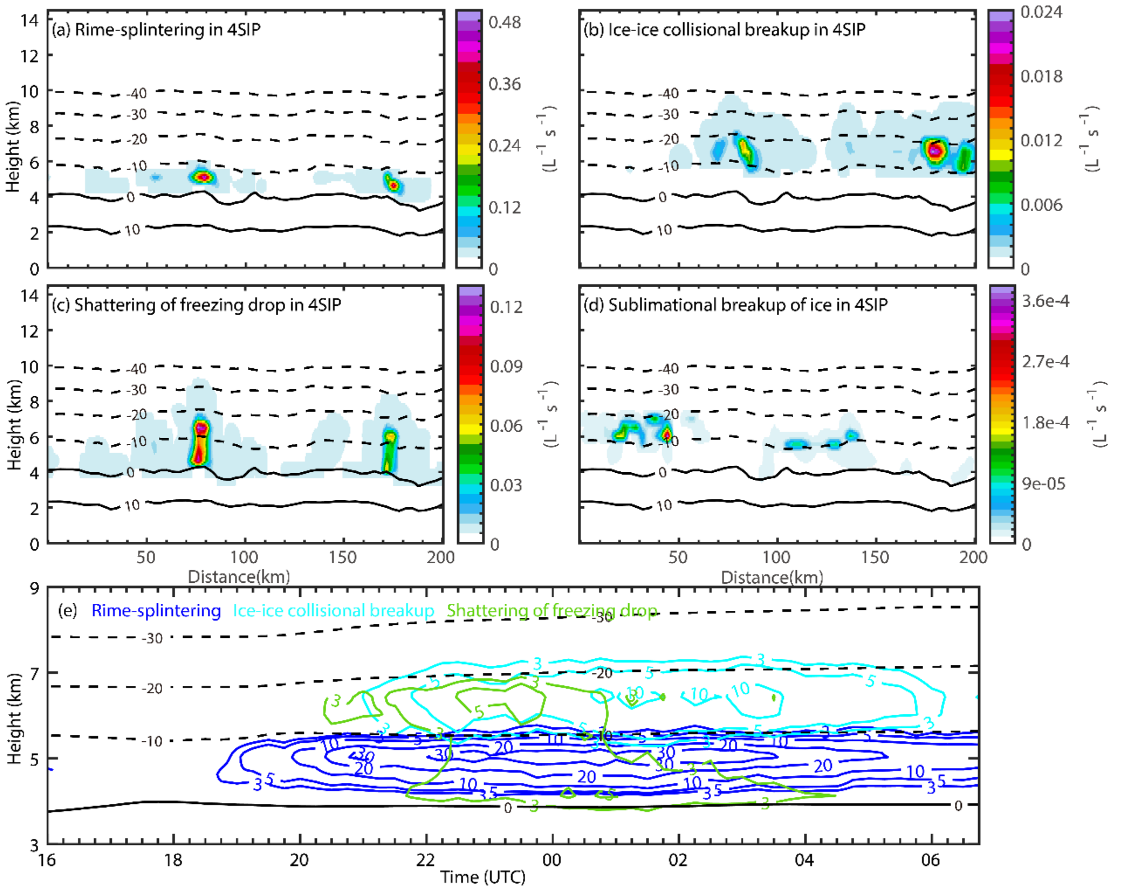

Figure R1: Cross-sections of the secondary ice production rates by different SIP processes resulting from the 4SIP experiment at 01:00 Nov. 28th. (a) rime-splintering, (b) ice-ice collisional breakup, (c) shattering of freezing drops, (d) sublimational breakup of ice, and (e) the time-height diagram of the mean ice production rate by different SIP processes. Contour levels are 3×10^{-3}, 5×10^{-3}, 10×10^{-3}, 20×10^{-3}, and 30×10^{-3} $L^{-1}s^{-1}$, the ice production rate of sublimational breakup of ice is so small that it never meets the lowest contour level

5. In your manuscript, it is noted that a rimed fraction of 0.2 was prescribed in the IC and 4SIP experiments, with the efficiency of the ice-ice collisional break-up process being sensitive to the choice of this parameter (Lines 543-544). I would suggest emphasizing this important assumption not only in Section 4 but also in Section 3, particularly when discussing the limited efficiency of the collisional break-up mechanism in comparison to, for example, rime splintering.

Reply: Thank you for your professional comment. This following discussion has been added to Section 3.

Ice-ice collisional breakup is more intense in regions with high ice/snow concentrations (Fig. 12f, l), its secondary ice production rate is much smaller than that of rime-splintering. However, it should be noted that the efficiency of ice-ice collisional breakup is related to the rimed fraction (Karalis et al., 2022; Sotiropoulou et al., 2021), A sensitivity test shows using a larger rimed fraction (0.4) can result in a stronger impact of ice-ice collisional breakup on cloud microphysics, but it is still much weaker than that of rime-splintering.