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Interactive comment on "The effect of secondary ice production parameterization on the simulation of a cold frontal rainband" *by* Sylvia C. Sullivan et al.

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General Comments:

The authors conduct a numerical modeling study of secondary ice production and its possible effects on surface precipitation, based upon observations of the 3 March 2009 cold frontal passage across Southern England analyzed by Crosier et al. (2014). They address three kinds of secondary ice production: rime-splintering, shattering of freezing raindrops, and collisional ice breakup, with several variations of coefficients that control the magnitude of each process. By enhancing the magnitudes of each process, often to the extent above that represented in past studies, or justified by laboratory experiments, they find (unsurprisingly) that the ice number concentrations can be greatly

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increased beyond that expected from primary nucleation. Through some (very rough) comparisons with the observations, they are still unable to replicate the maximum observed ice crystal number concentrations, and yet, estimates of the ice production rate actually exceed those based upon the observations. This disparity suggests that there might be some issues in comparing the observations and simulations, and/or that the simulated cloud dynamics are significantly different than in the observed clouds. The authors find an increase in surface precipitation of ∼20% for the simulations that maximize secondary ice production, and advocate from that result that parameterizations of these processes should be included in large-scale models.

Specific comments:

The authors are tackling a very difficult problem here, and studies like this are important and necessary. However, more care must be taken in what they can and cannot conclude from this study. I have some serious concerns with how the authors conducted some of the analyses, and/or their interpretations. In the manuscript, some very important details are omitted, that make it difficult to understand their interpretations.

Overall, I would like to see this study move forward, but feel that it would be of greater use to emphasize the temperature and dynamical regimes over which each of the secondary ice production processes is dominant, and how those differences assist (or do not) the formation of additional precipitation. That might be a more useful place to start when advocating that some of these processes be included in larger-scale models, as it would help focus case studies of the type of weather phenomena where they could have the most impact.

1. If observations and simulations are compared in this way, particularly when convective elements are contained within the weather phenomenon of interest, then it first must be demonstrated that the clouds and precipitation due to the cold frontal passage in the control case are consistent with that observed, and if not, to state clearly how they differ, and continue compensating for those differences when comparing the observed

and simulated microphysical development. For example, a figure showing simulated radar echo can be produced, and shown alongside Crosier's Fig. 3, to understand how the general structure might differ. The timing is also important: if observations over a given time are averaged and compared with the simulations, any issues in doing so must be known. A small paragraph summarizing the dynamics of the observed clouds, based on the analysis of Crosier et al., would also be helpful in "setting the stage" for the reader, regarding the types of clouds (strengths of updraft speeds measured by aircraft, cloud top temperatures) being considered here.

2. The routes from ice to precipitation discussed in the introduction, shown in Fig. 1, and later discussed with respect to effects of the secondary ice upon precipitation, are not inclusive of a major route from ice crystals to precipitation: enhanced rimed ice/rimed snow/graupel/frozen raindrop formation that can melt to become surface precipitation. In Fig. 1, it is somewhat suggested by (3), but the arrow isn't drawn as leading to acceleration of precipitation like (2). Such an analysis of that route to precipitation is completely omitted in the manuscript. Why? Even if no graupel were observed, the Crosier et al. paper discussed the importance of rimed snow, and noted that the aircraft did not sample the stronger convection where graupel might have resided. The authors only discuss in this study the possible effects on the Bergeron process leading to precipitation, but that would be more important in the stratiform precipitation regions, and not as much in the convective regions of the cold front band, where the heaviest precipitation will fall. I would think that the precipitation enhancement seen in Fig. 5 is due to rimed particles, not from an enhanced Bergeron process.

3. The implementation of the secondary ice parameterizations in the two-moment Seifert and Beheng scheme are confusing.

a. Why is the second moment not taken advantage of, here? Everything seems to depend upon mass. For example, rime-splintering appears to have a constraint of rimed mass, but for a two-moment scheme, the Cotton et al. (1986) second formulation that uses the number of fragments per number of 25 um diameter drops accreted would be a better prediction. The lab studies have shown that if the rimed drops don't achieve this size, they won't splinter. As implemented here, there is no drop size dependence, so splintering might be greatly overestimated, and some commentary needs to be given in regards to that limitation.

b. Along similar lines, what is the justification for the experiments using broader temperature ranges and/or increased fragment numbers for rime-splintering? That process has been studied much more in the laboratory than others (Hallet and Mossop, and Sauners & Hosseini, AR, 2001). The results here seem to rely on the expansion of this process to a broader area of temperature than appears justified by the laboratory work. The latter study also looks at the importance of fall speed, where graupel is more favored for greater splinter production. Since the simulation has little/no graupel here, then it would seem to imply using smaller splintering rates is appropriate.

c. Also, there is no mention of the recent work on ice-ice collisions, and its parameterization, by Phillips et al. (Phillips et al., JAS, 2017 and 2019), or for shattering of freezing drops (Phillips et al., JAS, 2018). How do their parameterizations compare to those used here, and how might that influence differences in the effects upon precipitation?

4. It is stated that Crosier et al. noted fall streaks at cloud top in the radar measurements. This would seem to imply a seeding mechanism of ice from above that could also have fallen to the observation level of the aircraft, unless this has somehow been ruled out?

5. I would contend that most "larger-scale models" do not have two-moment microphysical schemes, so that the suggestion at lines 21-24 on page 10 are not practical.

6. Comparison of observations and modeling: unless the authors can justify that the simulated movement of the rainband, and its dynamical nature, was very similar to that observed, it would make the comparisons shown in Fig. 4 not very useful. (If the comparison is not "fair", it could even be the case that the model IS producing

sufficient secondary ice, for example, if the dynamical/thermodynamical conditions of the simulated clouds in that region and over that time are different than observed!) The description of the analysis for panels b and c on page 12 is helpful, but the reader cannot see what is being compared (which is why some comparison of the observed radar evolution to the simulated one is needed early on in the paper.) The temperature ranges also need to specified in Fig. 4, too.

7. Ice production rates: it needs to be stated more clearly how these were derived, from both the observations and the modeling results. Right now, it is unconvincing that this comparison is valid. Also, it should be stated somewhere that the CIP-15 observations were corrected using an algorithm designed to remove shattering artifacts, but some still likely remain because anti-shattering tips were not used on the instrument, as stated by Crosier et al.

8. Qualifiers/limitations: need to be stated clearly throughout the paper. For example:

a. The model uses the primary ice nucleation parameterization of Phillips et al. (2008). Since INP measurements were not collected, it is unknown if this is an accurate representation, or not, and this might greatly affect the ratios of secondary to primary nucleated ice, including the possible importance of secondary ice to precipitation.

b. To show an appreciable affect on surface precipitation (20% increase), rime-splintering had to be increased over that typically depicted in models based on the laboratory measurements (e.g. Cotton et al. 1986 parameterization).

c. Reasons for why the other two secondary ice processes might be less important here: (i) minimal graupel, which is important for ice-ice collisional breakup; (ii) limited number of raindrops? (Not sure what else would have limited that process here, but it would be good to know.) Also, it should be explained that the Crosier et al. study noted limited graupel in their observations, and I don't think they found any evidence of shattered frozen raindrops, but they clearly state that the former could have been limited by the inability of the aircraft to fly in the more convective regions.

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d. The enhancement of the updrafts and precipitation, and downdrafts, mentioned on page 14 needs to be backed up with some evidence.

e. Some discussion of model resolution effects should be included, for both properly simulating the microphysics as well as the dynamics.

Technical Corrections:

1. Table 2 and Fig 2 are confusing, since most of these runs are not discussed in the paper. I would suggest only showing those that are discussed here, and just noting somewhere (if important) that other variations did not show much change in the results. Also, if the text could explain the naming convection for the different simulations used here, or new labels that are more intuitive to the particular change in a given simulation used, that would be much easier for the reader to interpret them.

If the major issues in this review are addressed, I suspect that much of the wording will be revised, and thus I will refrain from noting any suggestions at this time.

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