Reviewer 1 Comments

The authors have adequately explained several of the assumptions governing the simulations that were not previously elucidated. This helps the reader to understand the mechanics of the model. They have also addressed several, but not all of the concerns expressed in my previous review. However, one very basic and important point is not addressed in the revision. The paper does not adequately represent observations, and it does not adequately explain why the simulations differ from observations. In particular, I point to a recent paper (Lawson et al. September *JAS*) showing that the rapid formation of ice in convective clouds is s a strong function of cloud base temperature, and that secondary ice via drop shattering does not occur at cloud base temperatures colder than about 273 K. The simulations suggest that (p. 18 line 8) drop shattering (DS) occurs at "simulated cloud base" temperatures of 260 K and warmer. Data in Fig. 8 of Lawson et al. (2017) show that a cloud with a base temperature of about 259 K does not produce drops larger than 40 microns, and there is no indication of secondary ice production. Indeed, supercooled liquid water is measured at 237.7 K. Also, in their reply and the manuscript (Section 3.1) the authors state that "*we show the N_{ice} evolution from a 'warm-base convective' sensitivity run in Figure S7. Here the same threshold behavior occurs once the parcel reaches cold enough temperature for droplet freezing, but there is no N_{ice} decrease beforehand because ice nucleation begins later, and no graupel has begun to fall out.*"

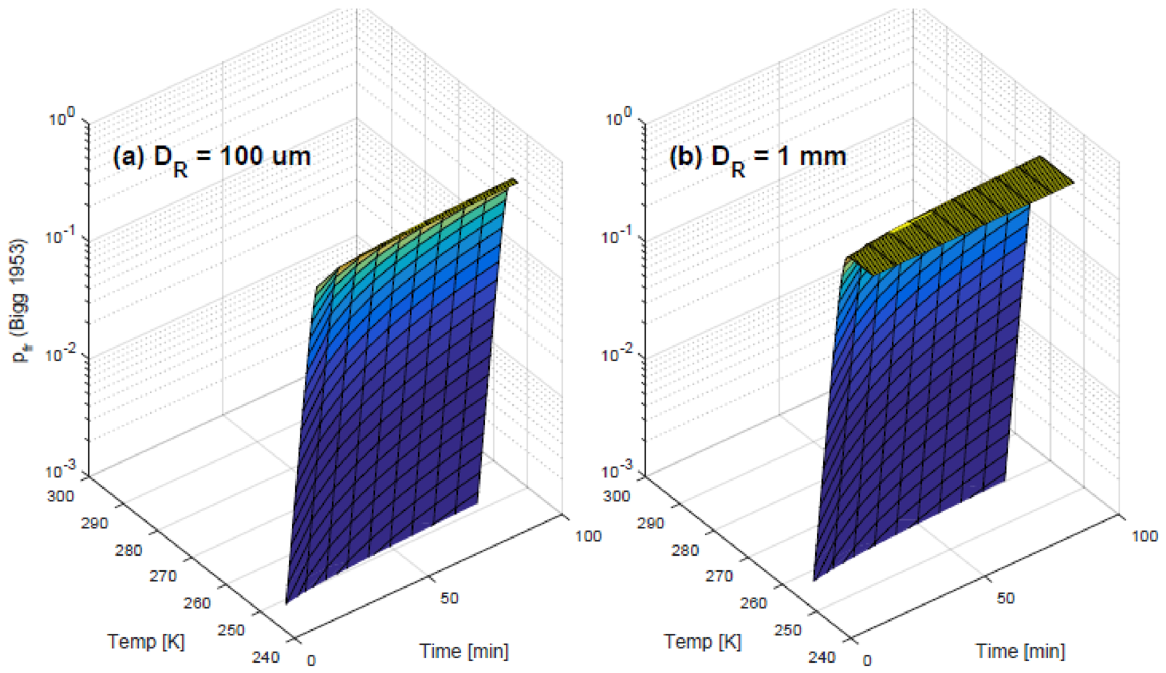
In contrast, the observations suggest that the probability of secondary ice production increases proportional to the fourth power of drop radius. Starting at a warmer cloud base means there is more cloud depth for the coalescence process to occur, which results in the formation of larger drops and a higher probability of dorp shattering and ice production. As far as I can tell, this is not represented at all in the simulations. Also, the observations suggest that the DS process does not depend on the formation of graupel.

Perhaps due to the artificial nature of the model, which assumes six hydrometeor categories that are each monodisperse, the simulations cannot hope to reliably represent the observations. If that is the case, then the authors need to compare the simulation results with recent observations and explain why the model differs. At the very least, the paper needs to adequately explain how the assumptions in the model impact the results. Or conversely, since we also know that observations are not perfect due to instrumentation uncertainties and undersampling (i.e. in-situ instruments only measure a tiny fraction of the cloud and do not represent a true Lagrangian view of the updraft), the manuscript should explain why the observations are not representative of reality.

Since this manuscript will eventually be published, my desire is that the authors take my criticisms in the manner they are intended, i.e., to improve the paper by including possible counterpoints to their arguments, which are largely focused on the model results and not always on how well they represent real clouds. We don't know how the actual process(es) of secondary ice production will be revealed in the next few years, or decades, when both measurements and models improve, so reporting both the model results and other possibilities provides a more complete picture.

Thank you for your rereading of and feedback on our work. We had included Table 2 to compare observations with our simulations. We have moved the contents of this table into the text of an *Observational comparison* section (§4) in the hope that this format is more accessible.

Thank you also for pointing us to the newly published Lawson et al. article and for your encouragement to revisit the droplet shattering formulation. The original model described in the Sullivan et al. *JGR* manuscript did not include this process, and its representation was the least refined of the three. So we have reworked it to yield more realistic behavior. Previous simulations were insensitive to initial temperature first because of the extreme temperature dependence of the Bigg 1953 freezing parameterization. To illustrate, here are the output probabilities for droplets of 100 micron and 1 mm diameter:



Independent of the parcel's initial temperature (or droplet size), freezing probability would be quite small until a temperature of at least -20°C. Then because the model only considered coalescence of small and medium droplets, the large drop size increased due only to condensational growth. So the model did not represent the continued coalescence that Lawson et al. 2017 pinpoint as crucial to initiate secondary production.

A framework with three, monodisperse liquid hydrometeor classifications can never fully represent this coalescence, and so in this sense, the model is indeed artificial. However, observed trends can still be qualitatively reproduced by adjusting the droplet freezing probability and large drop size evolution:

(1) We replace the Bigg freezing probability with a formulation based upon work by Paukert et al. 2017 (doi: 10.1002/2016MS000841). We assume that 10% of the ice-nucleating particles, as predicted by the DeMott et al. 2010 parameterization, do not freeze until small droplet collisions form a rain drop; the primary nucleation rates are correspondingly decreased. Then if about 100 such collisions form a raindrop (Paukert et al. 2017 show a two order-of-magnitude difference between the rain drop and collected particle number concentration in their Figure 5b.), the freezing fraction of the rain drop population is given by 10 N_{INP} / N_d.

(2) Coalescence of large drops with one another is incorporated. 5% of the large drop population undergoes coalescence per minute. This reduces the large drop number, and the liquid mass is redistributed over the remaining drops. Another important factor is the addition of a size threshold for the shattering probability: it remains zero unless the droplet diameter is greater than 100 um.

These adjustments introduce two new parameters: the number of INP remaining in a raindrop and the percentage of the large drop population undergoing collision-coalescence. With these updates, we have redone the droplet shattering simulations in all three sections and simulate behavior more in-line with observations. Enhancement now only occurs if the parcel is initiated above the freezing level and if the updrafts are somewhat higher, as can be seen in Figure 1b, Figure 2, and Figures 3c and 4c. We also have updated the analysis throughout, noting the importance of some representation of the warm rain process to have realistic T_0 dependence (for example, around lines 1 to 10 page 8 and lines 5 to 10 on page 12 among others). Figure 8 and 9 schematics have also been updated to reflect the need for warmer T_0.

Several of my previous specific comments were addressed, but some were not. See the following annotation.

p.1, Line 4: Drops collide and can breakup. Why not call this ice-ice collisional breakup, or at least define it as ice-ice collisions in the beginning of the paper.

We understand that more precise terminology is best and have gone through and changed all instances of "*collisional breakup*" to "*ice-ice collisional breakup*". When the process is first introduced on page 2, line 11, we state that it involves "breakup upon mechanical collision of ice hydrometeors."

p. 2, Line 7: In my opinion, you have chosen [your citations] poorly. Scott and Hobbs and Fridlind et al. are mainly theoretical / modeling studies. There are better papers that deal directly with laboratory experiments and field observations of drop shattering. The first in-situ photograph of a fractured drop, at least to my knowledge, is shown in Cannon et al. (1974). The photograph was collected from a film camera mounted on a sailplane spiraling in the updraft of a cumulus cloud. Korolev et al. (2004) showed the first CPI images of fractured drops in both laboratory experiments and from in-situ measurements. Rangno (2008) gives a nice summary and shows images of fragmented drops, pointing out that H-M is not active in these convective clouds with secondary ice production. Wildeman et al. (2017) shows excellent high-speed video of millimeter drops fracturing (photos in his paper; videos on his website). These references are found at the end of this review.

Given that the work is theoretical, we retain the Fridlind et al. citation, as an example of previous modeling work on droplet shattering. We remove the Scott and Hobbs one, as well as the Phillips et al. 2001 one, which deals primarily with rime splintering. Then we add your suggestions; thank you for these, particularly the Wildeman et al. 2017 one.

p. 2, Lines 16-17: This verbiage still does not justify the use of small-scale models if the physics do not adequately represent reality. This is like saying we can see the specimen better with a high-power microscope, but in actuality the specimen is not within the field of view. I suppose this is the basic rift between observationalists and modelers, who call their results data while observationalists call the results output. Since this is a modeling paper, I guess the modelers get to voice their opinion.

One has to begin somewhere in modeling the physics of a system. Certainly the model has many assumptions, but then there is measurement error in in-situ data or simplification in laboratory experiment set-up, as you acknowledge above. Better physical understanding is an iterative process between measurements of increasing accuracy and models of increasing insight in our opinion. And we would hope that, rather than feeling at odds, the observational and modeling communities cooperate in this iterative process. We leave the statement of the study premise as is.

p. 3, Eqn (1) and discussion: The model description is now clearer.

Thank you. We have also moved Table S1 to the main manuscript, so that details of the model formulations are more readily available.

p. 11, Lines 4-7: To my knowledge there has not been a quantitative measurement of the number of fragments produced per shattering event in the levitation experiments. I suggest that you present both the estimate you report from the lab experiments and the estimate from Lawson et al. (2015).

The fragment number formed per frozen droplet is shown from the Lawson et al. parameterization with various leading coefficients in Figure S2c and those based upon droplet levitation experiments are shown for the two sets of parameters used in Figure S3. More rigorous methods of fragment counting are being developed for the droplet levitation experiments, but the sigmoidal functions are based upon the qualitative observation that the Lawson et al. parameterization underestimates fragment number from small droplets (D ~ 100 um).