MAJOR COMMENTS (copied from initial review for clarity)

I will start this review by confessing that I am an observationalist, not a modeler. I bring an obvious bias into this review, which is that I couch my evaluation of this work in terms of data collected in clouds, not numerical simulations of clouds. My main concern with this manuscript is that I cannot determine how this parcel model relates to an updraft in a cloud. Presumably, a parcel model is intended to represent the evolution of an undiluted parcel of cloud as it rises in the atmosphere. However, it is not clear what the prognostic microphysical variables are in the model. Presumably the model predicts N_{ice} for each of the categories because this is shown in the "ice generation function" equation, but what about mass? It's not even clearly stated if it's a bulk or bin microphysics scheme. It's also not clear what the "ice generation function" itself is, and what the units of G_{ice} are. I'm assuming this is dN_{ice}/dt from all microphysical processes, but it's not clear. The Sullivan (2017 – JGR) reference is in review and of no help. Even if the Sullivan JGR paper becomes available, at a minimum the manuscript should state what the model predicted variables are, and how they are being solved in the model numerically (e.g., what kind of time stepping method, the time step, etc.). It would also help if the manuscript gave the evolution equations for the model predicted variables.

The manuscript shows no drop or ice particle size distributions and no liquid water or ice water contents as a function of temperature. Also, the observations that I am most familiar with suggest that clouds with cloud-base temperatures colder or equal to 0 C, which are all of the cases examined here, do not produce cloud drops large enough to support drop shattering, and generally not even large enough to support rime splintering. Large drops (drizzle and rain drops) are what the literature (e.g., Koenig 1963, 1965; Hobbs and Rangno 1990, Rangno 2008, Lawson et al. 2015) associates with drop shattering and rapid glaciation. The data suggest that the formation of millimeter-diameter supercooled drops requires cloud base temperatures warmer than approximately +18 C (291 K) and broad (> 50 µm diameter) cloud base drop distributions. Albeit, the requisite relationship between CCN and cloud base temperature is yet to be accurately quantified. Also, the coalescence process is key to the formation of supercooled large drops. Nowhere in the manuscript can I find how coalescence is handled in the model (except that K_x is a gravitational collection kernel in Eq. 1). One aspect of the simulations that does appear to be consistent with the observations is that rime-splintering takes place only in clouds with very weak updrafts (e.g., Heymsfield and Willis 2014). However, it is not clear in the manuscript exactly why this takes place in the simulations. Before I can recommend publication, the manuscript needs to provide an explicit description of the model, and the evolution of the parcel in terms of microphysical parameters (liquid and water size distributions, LWC, IWC as a function of temperature). I understand that this may be a bit artificial given the six categories of particles, but an attempt must be made, and the results should be compared with observations.

MAJOR COMMENTS (from review of revised manuscript)

The authors have adequately explained several of the assumptions governing the simulation 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. 2017 September JAS) showing that the rapid formation of ice in convective clouds is 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 temperatures 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 drop 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 under sampling (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.

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

p. 1 Line 4: "Break Up" is not a good term for ice-ice collisions, because drops also break up. I suggest that you find a more descriptive term that applies only to ice. If the term "break up" has to be defined as ice-ice collisions here and everywhere else in the manuscript.

“*Breakup*” was used because preexisting work on this process generally employs this term, e.g., Yano and Phillips *JAS* 2001, Phillips et al. *JAS* 2017, Field et al. *Meteor. Mono.* 2017. But we understand that this terminology may cause confusion with droplet breakup. We have gone through and changed all instances of “*breakup*” to “*collisional breakup*”.

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.

p. 2, Line 7: Add references; there are several.
We have added Scott and Hobbs 1977, Phillips et al. 2001, and Fridlind et al. 2007 to the citations for frozen droplet shattering.

In my opinion you have chosen poorly. Scott and Hobbs and Fridland 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.

p. 2, Lines 16-17: This is contradictory. In the previous sentence, you reference Field as reporting many uncertainties in the physics of secondary ice production, and then go on to state that small-scale models provide a good tool to estimate variability in secondary-produced ice. The model is only as good as the physics it contains. With the acknowledged vast degree of uncertainties, how can one have any confidence in the model results? If the model results are to be useful, then the physical uncertainties have to be emphasized. Also, sensitivity tests should be run to show how the physical uncertainties impact the results. At a minimum, a disclaimer of this sort needs to be inserted at this point in the manuscript.
We do not believe that these statements are contradictory. Investigating how a given output varies with uncertain parameters is an important application of models. And particularly for small-scale, more controllable models, output variation with adjustable parameters can be well-understood. This kind of work allows experimentalists to focus on measuring the most influential parameters and provides a test-bed for parameterizations prior to implementation in large-scale models. This utility of small-scale models is summarized in the IPCC Assessment Report 5: “high-resolution models enhance our understanding of cloud processes [as] an important tool in testing and improving parameterizations of cloud-controlling processes.”
As you note, sensitivity tests should be run with the small-scale model to understand the process and parametric uncertainties. Sections 3.1.1 and 3.3 contain these tests. We run simulations for different formulations of the physics of frozen droplet shattering. And then we investigate the sensitivity to adjustable parameters in the fragment generation functions (particularly F_{BR}, T_{min}, sigmoid versus polynomial forms for droplet shattering, and $p_{sh(max)}$).
We clarify the utility of small-scale models in this paragraph: “*Laboratory and in-situ data of these processes are difficult to obtain, and their fragment generation functions and temperature dependence remain uncertain [Field et al. 2017]. Given these uncertainties, implementation of secondary ice production parameterization in large-scale models would be premature. Instead,*

small-scale, more controllable models provide a means of estimating variability in output secondarily-produced ICNC with these parameters, as well as the minimum number of INP needed to initiate secondary production."

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.

p. 3, Eqn (1) and discussion: Eqn (1) is far too arcane to understand what is going on in the model. The reference to Sullivan et al. (2017) is of no use since it is under review. There are several unanswered questions. What are the units of G_{ice}? What is the role of coalescence and how is it handled? What is the cloud base drop distribution? Are CCN included? If so, how? Why don't small ice and small drops appear in Eqn (1)? Also, the number of secondary ice particles produced is only one issue. The mass of ice is of equal if not more importance. If large (millimeter-diameter) supercooled drops are rapidly freezing, as seen in the observations, then the *conversion* of water to ice (and eventually back to water in the form of rain), is more significant than the number of ice particles. Show the results also in terms of water and ice mass.

As described above, we have worked to make the model description more clear without restating what has already been published in the model development manuscript. In particular, we have more clearly stated the purpose and the units of the ice generation function and expanded its mathematical explanation with two additional equations. Small ice and droplets do not appear in Equation 1 because they play no role in any of the processes that are a source of small ice crystals.

Then we have emphasized that there are no size distributions involved; the monodisperse radius or axis of each hydrometeor class is evolved in time. The model contains no explicit aerosol. We add this statement and an in-line equation for primary nucleation before the statement that "*the droplet generation function consists simply of droplet activation, calculated from a Twomey power-law formulation*." So droplet number is calculated from supersaturation rather than a CCN number. Then we have added more detail for the coalescence formulation to Section 2, as discussed in the response to your major comments. And additional supplemental figures now show the ice mass mixing ratio for all default simulations, as well as the ice crystal radius evolution.

The model description is now clearer

p. 3, line 27: 237 K is not the homogeneous freezing temperature of pure water. The generally accepted value in the literature is 235.15 K. The AMS Glossary of Meteorology states that homogeneous nucleation occurs near 233.15 K.

Thank you for pointing this out. We write "*or a reaches a temperature of 237 K above which no homogeneous nucleation occurs.*"

p. 7, Fig. 2 Captions: How were the values of 2 and 10 fragments per drop chosen? How is the dependence on drop size handled?

Two was chosen as the minimum number of fragments into which a droplet could fragment. Ten was chosen as an upper bound because it represents an order of magnitude increase upon each fragmentation. In what was formerly Equation 2 (now Equation 4), $\aleph_{DS(coll)}$ contains the droplet size dependence: $\aleph_{DS(coll)} = F_{DS} (2r_R)^4 p_{sh} (T)$. So the fragment number is quartic in droplet size, as in Lawson et al. 2015. This equation was also given in Table S1.

p. 10, Line 17: Lawson et al. 2015 explicitly state that rime splintering is not responsible for the observed secondary ice process. Delete this reference.
Yes, thank you for catching this. Lawson et al. 2015 did emphasize the importance of the liquid phase to secondary ice production, but not to secondary ice production from rime splintering.
p. 11, Lines 4-7: What are the justifications for these assumptions and modifications?
Droplet levitation experiments at the Karlsruhe Institute of Technology are the basis for these modifications to the fragment generation function. In particular, these experiments indicate that the Lawson et al. parameterizations underestimates the fragment number generated for smaller droplets (D ~ 100 um) and overestimates the number for larger droplets (D ~ 1 mm). The sigmoid function addresses both of these concerns. Changing the exponent in the polynomial form addresses a potential overestimation for larger droplets only.
In Table S1, where we give the explicit functional forms of these modified fragment generation functions, we cite "Droplet levitation experiments", but we also point this out in the text now.

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).

p. 12, Lines 1-5: The production of ice in this scenario may be of some interest, but of more interest to cloud physicists is how the ice and water mass budgets evolve. Please show these.
p. 14, Line 5: This is the first mention of CCN. Were CCN used in the model, and if so, how?
To the statement that "*the droplet generation function consists of droplet activation, calculated from a Twomey power-law formulation*", we have added in Section 2 that "*droplet number is calculated solely from supersaturation rather than a CCN number*" because aerosol is not treated explicitly in our framework.
p. 16, Line 8: "warm cloud base". All cloud bases cited in the paper < 273 K, so there are no warm cloud bases.
Yes, accurate wording here would be "***warmer** cloud base*", i.e., those parcels that are initiated from relatively warmer subzero temperatures. We have changed this to "*warmer subzero cloud base temperatures*" in a few places.

References

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