MAJOR COMMENTS

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.

SOME SPECIFIC COMMENTS

Following are some specific comments. Until the major comments are addressed, I am not willing to go through the manuscript with a fine-toothed comb, as I assume that the paper will be significantly modified. The comments below are intended to give the authors some idea of the type of modifications that are needed.

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 retained, then it needs to be defined as ice-ice collisions here and everywhere else in the manuscript.

p. 2, Line 7: Add references; there are several.

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.

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 conversation 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.

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.

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

p. 10, Line 17: Lawson et al. (2015) explicitly states that rime-splintering is not responsible for the observed secondary ice process. Delete this reference.

p. 11, Lines 4 – 7: What are the justifications for these assumptions and modifications?

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 model, and if so, how?

p. 16, Line 8: “warm cloud base”. All cloud bases cited in the paper are < 273K, so there are no warm cloud bases.