Review of “A representation of the collisional ice break-up process in the two-moment microphysics scheme LIMA v1.0 of Meso-NH”

Major comments

This work implements a parameterization of collisional ice breakup (CIBU) into the LIMA mesoscale model. The simulations are well-planned and some of the results are interesting, but the manuscript needs significant work. First, there is no discussion at all of the LIMA scheme into which the CIBU parameterization has been implemented. Is this a bin or bulk scheme; what are the different classes of ice hydrometer; and what are there threshold sizes? The scheme needs to be explained for the reader to understand the results. Then I understand that the location and synoptic environment of the STERAO case study are available in Skamarock et al. 2000, but these are crucial to this study and an overview should be given here as well.

Then the parameterization itself is not especially sophisticated. Even the limited laboratory measurements of collisional ice breakup suggest that there are strong temperature dependences of the fragment number. And bigger snow-aggregates break up into more pieces, no? In which case, there should be some kind of aggregate size dependence in the fragment number.

I am particularly concerned by some of the altitude / temperature dependence in the results. For example, ice mixing ratio from this CIBU process is peaking at 12 km, certainly corresponding to cirrus formation and quite cold temperatures. But these secondary ice processes have been discussed for mixed-phase conditions at much lower altitudes and warmer temperatures. The discrepancy in nucleating particles and ice crystal concentrations is at these lower altitudes, so what exactly is the CIBU parameterization intended to explain?

This leads in to my final point, which is that no comparisons to data are made. Are there precipitation or ICNC data from the STERAO case? If so, some attempt should be made to assess whether the new parameterization is yielding more or less accurate precipitation rates or crystal numbers. This will justify a number of currently unsubstantiated statements throughout that certain results are “plausible” or “excessive” or “satisfactory” (Lines 191 to 193, 200, 276).

Specific comments

Then a number of details need clarification:

Line 27 – “The CIBU process was not perceived as a particularly important feature in cloud physics.” Here it is unclear to me in what context CIBU has been perceived as unimportant. In general, in cloud microphysics schemes? If so, please state that explicitly.

Lines 30 to 31 – “CIBU process is *very likely* to be active when cloud conditions are deemed favourable.” I do not think that the two proceeding citations validate this statement. Some additional discussion, and perhaps other citations, is needed of what these favourable conditions are.

Lines 57 to 59 – This sentence could use rewording, for example “An empirical but realistic CIBU parameterization is implemented in the well-suited LIMA scheme and interacts with other microphysical processes (heterogeneous ice nucleation, H-M process, etc.) to determine the concentration of small ice crystals.”

Line 61 – What does “erosion” mean here? Reduction of number?

Line 69 – "nucleation process yield" It would be clearer to say "scaled by the ice number concentration from nucleation".

Lines 73 to 74 – Sullivan et al. 2018 doi 10.5194/acp-18-1593-2018 would be another appropriate reference.

Line 81 – What does "covering" mean here? Including? Can you give an estimate of the average size of the large graupel particles? Or the lower threshold size for this categorization? This especially needed to assess the appropriateness of the assumption in line 94.

Lines 85 to 86 – Again it is unclear what this means: "particle sizes are taken to stay within a range of substantial occurrence of CIBU." Please make it more specific.

Line 92, Equation 2 – Please define Π.

Line 104 – Please cite the source from which you get your ice collisional efficiencies.

Line 106 – What is D_{trough}? It does not seem necessary to add a variable name.

Line 110 – Two parameters, i.e. both $D_{s,max}$ and $D_{g,min}$, cannot be dictated by a single equation.

Line 112 – "Least favourable situation" is unclear here. "Least favourable" for a large contribution from CIBU to ICNC? Why would you be considering this "at ground level" where temperatures will generally not permit ice formation in any case?

Lines 144 to 146, Equation 4 – My recommendation would be to move all of this to Appendix A. Otherwise, a large number of undefined variables appear all of sudden.

Lines 153 to 154 – What is the "local mean mass of the pristine ice crystals"? On what does this depend? What is "ice debris"?

Line 172 – What does "along the main diagonal" mean? The location of the 10 July 1996 thunderstorm needs to be included.

Line 176 – The acronym PPM needs to be expanded.

Line 182 – If the aerosol concentrations "have no importance for the simulations", perhaps Table 1 can be omitted.

Line 188 – This is a nice result, but it would be clearer to show difference fields in Figure 1b-d.

Figures 3, 4, and 5 – Again this is your call, but I think it would be easier to see the impact with difference fields of mixing ratio (taken from the base case).

Figure 7 – Here, I think you really need to show difference fields. Otherwise, you force the reader to flip back and forth with previous figures to make the comparison.

Section 3.1 – To me, it would make more sense to begin with the changes to ice metrics and microphysics because these should be directly impacted and to follow with precipitation because this link is indirect.

Lines 234 to 236 – You need to mention that the acronyms are given in Table 3 here.

Line 242 – 0.2 **x** 10^{-3}

Figure 9 – Why is nucleation - HINC, HIND, and HONC – not included in this Figure? These seem to be the tendencies one would most like to compare with CIBU.

Figures 9 to 11 – Are these domain-averaged? Or shown for a single grid cell?

Line 273 – N_i (N_{sg} = 0) The parentheses are important.

Around Line 277 – There needs to be discussion about why CIBU ice mixing ratio peaks at higher altitudes than does the CIBU ice number concentrations. Are the snow-aggregates at higher altitudes bigger? Otherwise, it is not clear to me what is going on here.

Line 305 – This behavior is not difficult to interpret. It results from the tradeoff between homogeneous and heterogeneous ice nucleation. Until there is quite a large IFN concentration, additional particles will suppress homogeneous nucleation and reduce ICNC.

Figure 14 – It is harder to interpret your results when you switch between L^{-1} and kg^{-1}. In particular, I am confused by some enhancement values in panel d. For example the peak N_i for N_{IFN} = 1 L^{-1} is 1000 kg^{-1} which is more or less 1:1, no? Why does the enhancement in yellow go up to 18? What am I missing?

Line 328 – “shocks” is generally used for electrostatic phenomena. “Collisions” is better.