Responses to Referee #01

« A representation of the collisional ice break-up process in the two-moment scheme LIMA v1.0 of Meso-NH » by Hoarau et al.

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.

Concerning the host microphysics scheme, it is true that we provide no extensive description of the LIMA scheme as we refer to Vié et al. (2016). We wished to describe our implementation of CIBU in a <u>brief paper</u>. However, it was clear enough that LIMA was a 2-moment bulk scheme. It was also our idea without a new lab. dataset, to include CIBU as simply as possible in a bulk scheme to see <u>some consequences on the precipitation</u> and the growth of the ice phase (the small crystals) depending on break-up efficiency i.e., the number of fragments produced per collision.

➔ We added a few sentences in the last paragraph of the introduction to recall the processes to generate ice crystals in the bulk scheme LIMA.

In contrast to previous modelling studies (analytical solution in Yano and Phillips (2011, 2016) and the parcel model of Sullivan et al. (2017)), our purpose here was to suggest a way to include CIBU in a standard bulk scheme and so to encourage other similar microphysics scheme to account for this process in our state of knowledge of this phenomenon.

The choice of the STERAO case is purely illustrative as we could run any academic or real meteorological case.

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.

Based on the few available data (Vardiman (1978), Takahashi et al. (1995)), it was hard to suggest a much complex parameterization. Precisely here we worked on the critical parameter α, the number of fragments per collision defined in Eq. 2, which multiplies the importance of CIBU. Then we found that limiting α is necessary both to enhance the concentration of the small ice crystals and to alter not too much the precipitation at the ground. We don't consider any temperature effect, not mentioned in Vardiman (1978). Temperature plays a crucial role in ice nucleation, with assistance of ice forming nuclei (IFN), in the Hallett-Mossop process of droplet riming and possibly in the raindrop shattering by freezing (but, the parameterization of this process by Lawson et al. (2015) didn't include a temperature effect).

In the case of CIBU, it is clear at first sight that it is the possibility of collisions between dense graupel and fragile aggregates that governs this type of ice multiplication process. Without new laboratory experiments, one can only speculate on the true dependence of the temperature and the size of the aggregates. As we integrate the collision kernel over the size distributions (Eq. 3) of the graupel and the aggregates, we include somehow a size effect. Note also that intuitively the number of fragments should depend more on the radial location of the impact of the colliding graupel on the aggregates. This means that only a bulk approach, here the evaluation of a mean α coefficient, is helpful in this situation as first we are more interested by the consequences to include or not a CIBU-like effect in a bulk microphysics scheme.

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?

The enhancement of the small ice crystal mixing ratio (Fig 3) at the 12 km level is not very surprising because the upward transport in the STERAO convective cells is very efficient (the vertical velocity reaches 40 m/s see Barth et al., 2007). We feel that this is a good point when besides we notice no dramatic change in the aggregate and graupel mixing ratios (Figs 4-5). Of course the CIBU process needs the simultaneous presence of aggregates and graupel which are peaking close to 9 km height (Fig. 8). As CIBU is independent of the temperature in our case, we don't favour the ice multiplication through CIBU at very cold temperature. It is true however that it is a possible way to check the CIBU efficiency by examining the persistence of detrained cirrus clouds from convective areas.

We see no conflict between ice nucleation and CIBU in the glaciated regions of the convective cells. Our representation of the nucleation is adapted from Phillips's empirical scheme of 2008 with a careful budget of the IFN as we consider the available and the nucleated IFN of several origins (here a dust mode and a BC mode, see Vié et al., 2016). So ice nucleation is governed by the temperature and the abundance of IFN while, independently, CIBU is the result of the simultaneous presence of aggregates and graupel particles. It is true also that ice crystals coming from ice nucleation are transported too at higher levels to populate cold regions well above 10 km high. So CIBU is an alternative to ice nucleation to increase the small ice crystal concentrations when IFN are limited. There is no malice behind that.

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)

We acknowledge that no comparisons to data are made also because there is no case study yet showing unambiguously that CIBU was strongly operating. The few ICE-T cases reported by Lawson et al. (2015) concluded on the importance of raindrop shattering because of the presence of spicules on frozen drops seen on CPI images. Clearly, missing arms on aggregates are probably more difficult to detect in the same way. Only Hobbs and Farber (1972) reported evidence for CIBU with a formvar replicator. Our feeling is that ice multiplication does exist in clouds (Leroy et al., 2015; Ladino et al., 2017) but without CPI images it is difficult to assess that it is solely the result of collisional ice break-up or raindrop shattering by freezing. As a result, work is underway to include this last process to complete the panoply of extra ice crystal sources in the clouds simulated by the LIMA scheme in Meso-NH.
To conclude and also to account for remarks of the 2nd reviewer, we justify our parameterization of CIBU (for 2-moment bulk microphysics scheme) by the need to introduce new mechanisms to explain "anomalous" high ice water concentrations but under the constraint of minimizing perturbations to the production of precipitating hydrometeors. This is the starting point of our study to check the value of the critical parameter α. We agree to remove most of the unsubstantiated statements in the revised version of the paper.

Additional references:
Barth, M. C., Kim, S.-W., Wang, C., Pickering, K. E., Ott, L. E., Stenchikov, G., Leriche, M., Cautenet, S., Pinty, J.-P., Barthe, Ch., Mari, C., Helsdon, J. H., Farley, R. D., Fridlind, A. M., Ackerman, A. S., Spiridonov, V., and Telenta, B. 2007: Cloud-scale model intercomparison of chemical constituent transport in deep convection, Atmos. Chem. Phys., 7, 4709-4731, https://doi.org/10.5194/acp-7-4709-2007.

Lawson, R.P., S. Woods, and H. Morrison, 2015: The microphysics of ice and precipitation development in tropical cumulus clouds. J. Atmos. Sci., 72, 2429–2445, https://doi.org/10.1175/JAS-D-14-0274.1
Leroy, D., Fontaine, E., Schwarzenboeck, A., Strapp, J. et al., "HAIC/HIWC Field Campaign - Specific Findings on PSD Microphysics in High IWC Regions from In Situ Measurements: Median Mass Diameters, Particle Size Distribution Characteristics and Ice Crystal Shapes," SAE Technical Paper 2015-01-2087, 2015, https://doi.org/10.4271/2015-01-2087.
Phillips V.T., P.J. DeMott and C. Andronache, 2008. An empirical parameterization of heterogeneous ice nucleation for multiple chemical species of aerosol. Journal of the Atmospheric Sciences 65(9): 2757–2783.

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.

We simply meant that the CIBU process is never taken into account explicitly in a microphysics scheme (bulk or bin) probably because its importance is overlooked in cloud physics. This observation justifies our present modelling study in GMD.

Correction: "…the CIBU process was overlooked in cloud physics. So to our knowledge a contribution of CIBU is never accounted for in the vast majority of the currently used microphysics schemes."

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.

The referee is right, the sentence is awkward. So we suggest replacing it by:

"… the CIBU process is very likely to be active in case of inhomogeneous cloud regions where ice crystals of different sizes and types are locally mixed."

Then we introduce CIBU as the result of collisions between hydrometeors of different types.

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."

We follow the suggestion to write:
"Here, the goal is rather to implement an empirical but realistic parameterization of CIBU in the well-suited LIMA scheme to cooperate with other microphysical processes (heterogeneous ice nucleation, droplet freezing, H-M process, etc.) to determine the concentration of small ice crystals."

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

Here "erosion" means the mass loss of ice of the aggregates. This word is used sometimes in this context.

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

We agree, change made.

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

We agree to add this new 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.

Initially we used "covering" because the "snow-lightly rimed" category of ice hydrometeor (aggregates) is wide enough to collect big pristine crystals (D>150 µm) coming from water vapour grown pristine ice crystals and assemblages as a result of ice aggregation with light rime eventually. The sentence is rewritten as:
"… here we consider collisions involving two types of precipitating ice: small aggregates gathering pristine ice crystals larger than 150 µm and large graupel particles."
In CIBU we integrate over the particle size distribution (PSD) of the graupel for sizes larger than D_{gmin}=2 mm while we are doing the same for the PSD of the snow-aggregates but for 0.2 mm < D_s < 1 mm, so we reasonably assume that D_g > D_s most of the time because the particle size is raised to power 2.

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.

We meant that a way to impose an impact velocity of the graupel larger than 1 m/s is to integrate over the PSD but with an appropriate range of size. We felt that the choice of D_{smin}, D_{smax} and D_{gmin} is a good compromise.
We modify the whole sentence in the following way:
"For the sake of simplicity and because the impact velocity of the graupel particles should be well above 1 m s^{-1} to remain in the break-up regime of the aggregates, the particle sizes are selected to enable a substantial occurrence of CIBU."

Line 92, Equation 2 – Please define Π.

Sorry for the typo, one should read π instead.

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

We take the collision efficiency equal to one for the sake of simplicity and because we assume that for $D_{smin}<D_s<D_{smax}$, there is no lateral deflection of an aggregate (trajectory) when hit by a larger graupel particle. We offer no other explanation (see also Chapter 14 of Pruppacher and Klett, 1997). Note however that ice-ice collection processes are more dependent on the sticking efficiency which is temperature dependent in LIMA as revised in Ferrier et al. (1995), see also Phillips et al. (2015).

Line 106 – What is Dtrough? It does not seem necessary to add a variable name.

D_{trough} is the name given by Field (2000) in his Fig. 5 to separate the small pristine ice regime from the "modal" snow-aggregates.

Line 110 – Two parameters, i.e. both Ds,max and Dg,min, cannot be dictated by a single equation.

That's true but we had to make a choice because we are describing a bulk parameterization which is indeed sensitive to the contrasted properties of the aggregates and the graupel. Furthermore as it is clear that CIBU is not a threshold process (as it is the case for the autoconversion of the droplets for instance) there is an acceptable uncertainty for the choice of these parameters provided that the impact velocity is larger than 1 m s^{-1}.
A more elaborated choice for D_{smax} and D_{gmin} values could be based on the graupel-aggregate collision kinetic energy CKE per surface area of the aggregates (Phillips et al., 2015) but there is no clear indication of what reference to take to scale this parameter. In our case with D_{smax}=1 mm and D_{gmin}=2 mm, one gets $CKE/(\pi/4D_{smin}^2)=0.038$ Kg s^{-2}.

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?

The least favourable condition in this case is when an aggregate of size D_{smax} is hit by a small graupel of size D_{gmin} leading to the minimal impact velocity V_{sg}. We replace "the least favourable situation gives V_{sg}=1.26 m s^{-1}" by "one gets V_{sg}>1.26 m s^{-1}". We refer to the ground level because V_{sg} is always larger aloft.

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.

We don't agree to move Eq. 4 (and Eq. 6) to the appendix A. The moments of the complete and incomplete gamma function are easy to identify. We suggest to modify line 142: "With the definitions of the moments $M^{INC}_{x}(p,X)$ of the incomplete gamma law given in Appendix A, …"

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

We suggest to remove the word "local" and to replace "ice debris" by "ice fragments" for a better understanding.

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

The convective bubbles are arranged according to Skamarock et al. (2000) in order to maintain the multicellular convection (that becomes supercellular at the end) as long as possible in the computation domain. The chosen STERAO case is a very classical one to test parameterizations in the context of continental high CAPE (Convective Available Potential Energy). The true location of the storm is of secondary importance. We modify the text in the following way: "The simulations were initialized with the sounding of northeastern Colorado given in …" and "… along the main diagonal of the horizontal X, Y plan in the wind axis.".

Line 176 – The acronym PPM needs to be expanded.

PPM is Piecewise Parabolic Method a finite volume transport scheme. Done.

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

Table 1 is necessary for those who wish to redo the simulation. We reword the sentence: " …, the characteristics of the five aerosol modes are standard for the simulations shown here …"

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

We don't agree because differences of precipitation fields are more confusing to comment with positive and negative isocontours. We think that using the same color scale as it is in Fig. 1, is more demonstrative to underline the decrease of the precipitation when N_{sg} increases.

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).

We give the same response to the preceding question because we tried to plot difference fields but with less clarity.

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.

Well that's true but in a final publication, the figures are inserted in text body.

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.

Unsurprisingly we knew that the critical parameter N_{sg} was monitoring the increase of the ice concentration N_i as much as wanted. So then a strong issue was to avoid too much perturbation to the simulated precipitation at the ground level when CIBU was activated. We add this constraint because microphysics schemes that don't include CIBU, are now running quantitative precipitation forecasts. For this reason we put in the foremost of Section 3.1 the limitation of *Nsg* in the revised version of the manuscript.

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

We agree and we add " … (10 minute average again and the nomenclature of the processes provided in Table 3) …"

Line 242 – 0.2 x 10-3

Corrected here and elsewhere.

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.

Nucleation is an essential contributor to the ice concentration but not to the ice mixing ratio because the early ice crystals are very small until they grow by water vapour deposition.

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

We explain (Lines 224-226) how we select the cloudy columns to generate the profiles of Figs 9-11. We average over all the three main cells.

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

Sorry for the mislocation of the closing parenthesis. Corrected.

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.

If we compare the profiles in Fig. 8 (mixing ratios) and in Fig. 12 (concentrations), we can see that the "cloud ice" peaks are located at the same height (12 km "N_{sg}=0" case, 11 km "N_{sg}=random" case and 10 km "N_{sg}=10" case). So the question is more to understand why the profiles of the CIBU contribution seem out of phase when looking at the mixing ratio tendency $\partial r_i/\partial t|_{CIBU}$ in Fig. 9 and at the number concentration tendency $\partial N_i/\partial t|_{CIBU}$ in Fig. 13 (both are red coral curves). As written Line 153, $\partial r_i/\partial t|_{CIBU}$ is taken as the minimum between the limiting value $\partial r_i/\partial t$ given by Eq. 5 and $\partial r_i/\partial t$ estimated as (r_i/N_i) x $\partial N_i/\partial t|_{CIBU}$ where r_i and N_i are local characteristics of the cloud ice field (it is implicitly suggested here that the ice fragments produced by CIBU follow the local size distribution of the small ice crystals). So essentially because r_i is very low below 6 km, even where $\partial N_i/\partial t|_{CIBU}$ is high, $\partial r_i/\partial t|_{CIBU}$ remains low. Above 9 km, both r_i and N_i are reaching higher values so $\partial r_i/\partial t|_{CIBU}$ is increasing.

Concerning the snow-aggregates, we don't consider the total concentration N_s as a state variable in LIMA. These particles are characterized by a single moment, the mixing ration r_s, while N_s is parameterized as $C\lambda^x$ as recalled at Line 148.

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

We modify the end of the sentence to make it clearer (line 306): "… because of the non-monotonic trend of the N_i profiles with respect to N_{IFN}." and we add a sentence "Here this is equivalent to computing an IFN nucleation efficiency" to introduce Fig. 14c at Line 308.
We don't see why homogeneous and heterogeneous ice nucleation should cooperate. They are independent processes. However the proportion of nucleated IFN doesn't change very much when N_{IFN} spans over 6 decades.

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 Ni for NIFN = 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?

We provide the simulation results in # kg^{-1} while the forcing conditions of the initial IFN concentrations are given in # dm^{-3} unit which is more intuitive. Sorry for the "L^{-1}" unit in the title box of Fig 14a.
In panel d) the CIBU enhancement ratio shows a maximum for N_{IFN} = 1 dm^{-3} (yellow curve) at an altitude of 9 km so in this case the simulation with CIBU is leading to an ice concentration N_i which is nearly 20 times larger than N_i of a similar simulation but run without CIBU. Of course the profiles in panel d) rely on the profiles shown in panel a), here giving $N_i \approx 900$ kg^{-1}, and in panel b) with N_i less than 100 kg^{-1} but hard to see ! Note that this is only a snapshot and that ice crystals are also produced by Hallett-Mossop process and removed by aggregation and so on.

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

We replaced word "shocks" by "collisions" at the same place.

Additional references:
Ferrier, B. S., W.-K. Tao and J. Simpson, 1995: A double-moment multiple phase four-class bulk ice scheme. Part II: simulations of convective storms in

different large-scale environments and comparison with other bulk parameterizations. J. Atmos. Sci., 52, 1001-1033.

Phillips VTJ, Formenton M, Bansemer A, Kudzotsa I, Lienert B. 2015. A parameterization of sticking efficiency for collisions of snow and graupel with ice crystals: Theory and comparison with observations. J. Atmos. Sci. 72: 4885–4902.

Pruppacher, H. R., and J. D. Klett, 1997: Microphysics of Clouds and Precipitation, 2nd rev. edition, Kluwer Academic Publishers, 954 pp.