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 <u>concentration</u> but not to the ice <u>mixing ratio</u> 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.

Responses to Referee #02

« 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 paper describes a new implementation of a collisional ice break parameterization in a two moment microphysics scheme. This particular secondary ice formation mechanism is very poorly understood, and modelling studies are necessary to ascertain whether it can have an impact on mixed phase cloud microphysics. The subject of the paper is thus quite suitable for GMD. However, the analysis is too limited and the results are unclear. The language is very hard to follow, which makes the results harder to understand and review. The writers are strongly urged to make the best effort possible at improving the readability of the manuscript by revising the language.

The major shortcoming of the paper, which is recurrent throughout all of the analysis carried out, is the lack of conducting proper diagnostics to establish that the results are robust. There is very little description of the test case used, which is not acceptable given that the results are very specific to the details of the experimental setup. The writers are urged to dedicate a full section at describing the experimental setup so the reader can have an idea of how susceptible the simulation is to the microphysical changes incurred. My impression is that the simulation is very dynamically forced so one does not expect changes in the dynamics that would feed back into the microphysics which would make comparison of the microphysical fingerprints difficult. The writers must show that this is the case, or if it is not, then conduct the appropriate analysis on the dynamic-microphysical feedbacks.

The study relies on two main ideas. First, a collisional ice break-up (CIBU) mechanism should occur in natural clouds where shocks between ice crystals are very frequent (sometimes leading to cloud electrification) while this process is not considered in microphysics schemes. Second, including CIBU should increase the number concentration of the small ice crystals by two to four order of magnitude (Ladino et al, 2017 for tropical clouds) but this effect should not be too much detrimental to the genesis and to the amount of precipitation at ground level. So the tuning of the CIBU scheme, here finding empirically appropriate values to the N_{sg} parameter as in other studies, is therefore necessary but is a difficult task: taking N_{sg} >10 starts to dramatically reduce the precipitation but N_{sg}<0.1 leads to no noticeable effects to the ice crystal concentration in clouds.
The authors support the idea that after Vardiman (1978), it is urging to renew the lab experiments to investigate CIBU and to fix up the limit of the number of

fragments after collisions. However, in the meanwhile, it is pertinent to examine the possible consequences of CIBU in current microphysics schemes and to propose a simple parameterization of CIBU based on state variables of a 2-moment microphysics scheme (Eq. 3 of the manuscript).
To summarize, we show that the CIBU process increases the number concentrations of the small ice crystals while an excessive CIBU forcing can dramatically reduce the precipitation. As we believe that cloud models have now greatly improved to reach the level of quantitative precipitation forecast, the inclusion of new secondary ice formation processes like CIBU and raindrop shattering is not harmless. To the authors knowledge, all the recent studies concerning CIBU were done out of the context of three dimensional cloud simulation so ignoring side effects of CIBU, as the goal was to show an expected (explosive) increase of the number concentration of the small ice crystal concentration.
In the introduction and in the revised text, we insist more on the aspect that adding CIBU (and the raindrop shattering in a future work), must be carefully conducted to not alter a fundamental outcome of microphysics scheme that is the production of precipitation. However, we feel that the non-linearity of the microphysics schemes is such that the number concentration of the small ice crystals can be considerably increased through unsuspected cloud physics processes without significant changes in the production rate of the hydrometeors. This is what this manuscript would like to demonstrate.

The supporting case study STERAO is a standard case of isolated deep continental convection over the US Great Plains that lasted several hours. As recommended we provide more information about the model set up for STERAO. However we don't understand the remarks concerning the microphysics-dynamics feedbacks. The microphysics-dynamics interactions do exist naturally in the model, with and without CIBU. The results show that even "very dynamically forced", CIBU modifies the precipitation at the ground level if we compare with a reference simulation where CIBU is ignored. This does not mean that CIBU should act strongly and directly on the dynamics. On the contrary, our results show that CIBU is moderately but systematically reducing the production efficiency of the hydrometeors, snow-aggregate and graupel particles on the rebound, so that all simulations show qualitatively a similar evolution of the storm.

In the spirit of the preceding critique, there is very little discussion on how the collisional breakup mechanism can alter microphysics-dynamics interactions. The precipitation results for example are presented as if changes in precipitation do not alter the dynamics of the storm. The authors do calculate the tendencies for the ice budget, but very little discussion is carried out. The tendencies of vapor depositional growth, riming, sedimentation etc. are all *being altered but not enough detail is given as to how. Instead there is only a very brief overview (e.g. Sec. 3.3).*

We understand the critique but our goal was also to write a short paper. We don't discuss on the alteration of the storm dynamics but we show that increasing CIBU efficiency i.e., taking N_{sg}>10 is detrimental to the production of the precipitation. This is basically a major result of the STERAO case simulation of strong convection. We admit that we could have discussed this point more in depth. However it would be very strange that a process like CIBU which is ignored in most of the microphysics scheme, would change so much the dynamics of a storm when activated. We agree to extend the discussion around the ice budgets even if our primary goal was to shorten the manuscript.

Unfortunately, the manuscript in its current form is not suitable for publication in GMD. Despite the uniqueness of the study and its importance, the manuscript fails at placing the collisional breakup mechanism in the context of a cloud resolving model. My major concerns are further detailed in the specific comments that follow.

We hope that the amended version of the manuscript is more convincing. Our purpose was to draw attention to the overall integrity of a microphysics scheme. This means that adding "beneficial" processes, like collisional breakup or raindrop shattering, to increase the concentration of the small ice crystals, must be examined in the light of three dimensional simulations to evaluate all the consequences on the evolution of a precipitating system.

Specific comments

Abstract, L16-19: This statement is contradictory to the preceding one. If it is concluded that the CIBU scheme needs better observational constrains, then why is it ready to be used in its current form to simulated REAL deep tropical clouds?

We admit that the end of the abstract is awkward, so it has been revised.
We are convinced that CIBU does occur in clouds and that the strength of the process is high enough to be responsible for a secondary production of ice besides Hallett-Mossop mechanism and the rain drop shattering. But indeed, it is clear that very few lab data exist to elaborate a solid parameterization of CIBU (temperature dependence?). What is suggested here is to examine the consequences of a parameterization based only on a simplified form of the collection kernel between snow-aggregates and graupel particles times N_{sg}. So we agree without difficulty that our representation of CIBU could evolve with more data from lab experiments. In the meantime we urge the cloud physics

community to include a very simple representation of CIBU like the one proposed here in the microphysics schemes.

Consequently we add L12
"… an upper bound of the CIBU effect by examining the rainfall rates"
We modified the last sentence of the abstract to remove any ambiguity.
"However the proposed parameterization which is easy to implement in any two-moment microphysics schemes, could be used in this primary form to simulate deep tropical cloud systems where anomalously high concentrations of small ice crystals are preferentially suspected to occur."

Introduction: A discussion, with the relevant references, is needed to motivate the collisional break up process. Specifically, the writers should cite cases in which excessive ice crystal numbers cannot be explained by the Hallet-Mossop mechanism. The authors should also refer to other possible secondary ice formation mechanisms like drop shattering. In its current form, the introduction does not motivate the need to carry out numerical experiments of the collisional break up process.

We agree but the focus here is on the representation of the collisional ice breakup as a candidate for secondary ice production in clouds. The whole story of airborne observations of high ice crystal concentrations was redrawn in Field et al. (2017). We have no contribution here on observational evidence of collisional ice breakup so we would like to be brief on this kind of discussion. We have already a footnote (number 2) that refers to Table 1 of Field et al. (2017) where the secondary ice production mechanisms are listed with references.
However we added a new sentence at the end of the first paragraph (L39)
"… are predicted. At first, our wish to introduce CIBU in a microphysics scheme is essentially motivated by the detection of unexplained high ice water contents that sometimes largely exceed the concentration of ice nucleating particles (Leroy et al., 2015; Field et al., 2017; Ladino et al., 2017)

Sec. 2.1: Please justify the choice of a temperature independent Nsg here. For example, Sullivan et al. (2017) use an Nsg that is temperature dependent.

Sullivan et al. (2017) who didn't justify their choice either, introduced a temperature enhancement factor based on Fig. 4 of Takahashi et al. (1995). Compared to Vardiman (1977), we feel that Takahashi's experimental setup is not adapted for CIBU because the splinters are produced by cm size ice spheres to simulate graupel particles rubbing against each other (and not breaking up as expected for fragile aggregates). Furthermore we feel intuitively that the production of up to 400 ice fragments per collision at ~254 K is exaggerated.

We concluded that a limit of 10 fragments per collision is enough to increase by one magnitude the ice number concentration of the STERAO (continental) case. Yano and Phillips (2011) retained a production of 50 fragments per collision with no temperature dependence.

Sec. 2.2: A better description of the two moment scheme is needed. The equations can go into an appendix and more qualitative discussion of how the scheme defines the ice categories and how those would relate to the CIBA would be very beneficial here.

We would like to keep Sec. 2.2 as it is, but becoming Sec. 2.3 with title "**2.3 Representation of CIBU in the LIMA scheme**". We suggest reorienting the content of Sec. 2.2
"2.2 Characteristics of the LIMA microphysics scheme

The microphysics LIMA scheme (Vié et al., 2016) includes a representation of the aerosols as a mixture of Cloud Condensation Nuclei (CCN) and Ice Freezing Nuclei (IFN) with an accurate budget equation (transport, activation or nucleation, scavenging by rain) for each aerosol type. The CCN are selectively activated to produce the cloud droplets which grow by condensation and coalescence to produce the rain drops (Cohard and Pinty, 2000). The ice phase is more complex as we consider the nucleation by deposition on the IFN and the nucleation by immersion (glaciation of tagged droplets formed on partially soluble CCN). The homogeneous freezing of the droplets is possible when the temperature drops below -35° C. The Hallett-Mossop mechanism generates ice crystals during the riming of the graupel and the snow-aggregates. The H-M efficiency depends sharply on the temperature and on the size distribution of the droplets (Beheng, 1987). The initiation of the snow-aggregates category is the result of the depositional growth of large pristine crystals beyond a critical size (Harrington et al., 1995). Aggregation and riming are computed explicitly. Heavily rimed particles (graupel) can experience a dry or wet growth mode. The freezing of the raindrops by contact with the small ice crystals is leading to the frozen drops merged with the graupel category. The melting of the snow-aggregates leads to graupel and shedded raindrops while the graupel particles directly melt into rain. The sedimentation of all particle types is considered. The snow-aggregates and graupel particles are characterized by their mixing ratios only.
The LIMA scheme assumes a strict saturation of the water vapour over the cloud droplets while the small ice crystals are subject to super or undersaturated conditions (no instantaneous equilibrium)."

Sec. 3. L172-176: As mentioned above, many details of the test case are missing. "Several hours" is too general of a timeframe, please specify the actual time of *simulation. There are no details of the boundary conditions. What is the spacing between the vertical levels?*

We added the information in the revised text.
"The test case is illustrated by idealized numerical simulations of the 10 July 1996 thunderstorm in the Stratospheric-Tropospheric Experiment: Radiation, Aerosols, and Ozone (STERAO) experiment (Dye et al., 2000). This case is characterized by a multicellular storm that becomes supercellular after 2 hours. The simulations were initialized with the sounding given in Skamarock et al. (2000) and convection was triggered by three 3K-buoyant bubbles aligned along the main diagonal of the X,Y plan in the wind axis. Meso-NH was run for 5 hours over a domain of 320 x 320 gridpoints with 1 km-horizontal grid spacing. There were 50 unevenly spaced vertical levels up to 23 km height. With the exception of the wind component, all the fields including microphysics, were transported by an accurate, conservative, positive-definite PPM scheme (Colella and Woodward, 1984). There are no surface fluxes but the 3D turbulence scheme of MesoNH is activated. Open lateral boundary conditions are imposed. The upper level damping layer of the upward moving gravity waves starts above 12500 m."

Sec.3 L179-183: What about sensitivity to CCN? Please be clear here. Do you mean to say that you do not change the CCN concentration? As it is written, it sounds like you are saying that there is no sensitivity to the CCN concentration.

We are running the simulations with the same CCN set up. Of course the LIMA microphysics scheme is sensitive to different CCN characteristics (CCN size distribution or mode and chemistry) and the scheme is able to deal with an external mixture of several CCN modes. Here CCN activation is not the purpose of the study but we need to give CCN characteristics to run the LIMA scheme. We just mention that the focus is not on the sensitivity to the CCN. The same for the IFN except for the last study in Sec 3.5 where we are seeking for a minimal IFN concentration to enable the CIBU process.
We modify the last sentence of the paragraph:
"… chemical composition of the CCN and of the IFN, the characteristics of the five aerosol modes are standard for the simulations shown here except for …"

Sec. 3.1. L191-192: Why do the results suggest this empirically? Are the precipitation profiles being compared to some expectation which is satisfied in the specified range of Nsg? What is "unrealistic" about the simulation results for Nsg > 10.0?

The numerical simulations can only provide an indication of the plausible range of values for N_{sg} and so the interpretation of model results in terms of threshold

is by essence “empirical”. We clearly claim that N_{sg} must be larger that 0.1 to perturb a standard simulation run without CIBU while taking N_{sg}>10 is leading to an excessive perturbation of the precipitation field. A simulation performed with N_{sg} =50 reduced the accumulated precipitation by a factor 2.
Our strong argument is that it is not acceptable that the CIBU process which is ignored in the great majority of microphysics schemes, should take so much importance to accurately predict the precipitation. The central question of the study is therefore to find a compromise for N_{sg} in order to increase the cloud ice concentration but with the less possible disturbance to the amount of precipitation at ground level.
We revised the text:
“A value lower than 0.1 leads to a negligible effect of CIBU in the simulation, while taking N_{sg} >10.0 has an excessive impact on the storm precipitation (the N_{sg} =50 case is not shown).”
and
L199: “… considering the strong adverse effect …”
L200: “… satisfactory approach. Admittedly, the limit N_{sg} ~10.0 is more an order of magnitude but our conclusion is to recommend …”

Sec. 3.1. L199-202: This statement is unjustified. As emphasized in the preceding comment, realism of a specific Nsg range has not been established, therefore the writers’ conclusion on the choice of N0 by Yano and Phillips (2011) being unrealistic is not justified. Also there aren’t enough details about the cited study to make a meaningful comparison here.

The paper of Yano and Phillips (2011) was trying to demonstrate that CIBU could lead to an explosive ice multiplication regime most of the time (see the discussion in their section 3 about the normalized ice multiplication efficiency). Clearly speaking the concern there was to reproduce very high concentrations of ice in idealized simulations and so ignoring any possible side effects such as perturbations brought to the production of ice hydrometeors. In addition, looking at Fig. 6 of Vardiman (1978) results, one can see that the plausible values of the fragment numbers are well below 50. Only Takahashi et al (1995) could detect hundreds of splinters in their lab experiment but we believe that the experimental set up was not truly representative of ice crystal collisions in cloud conditions (rubbing of cm size ice spheres against mechanical fragmentation of aggregates and dendrites by shocks as in Vardiman (1978)). We admit however that an upper boundary of N_{sg} =10 is more an order of magnitude than a true threshold so we revise our wording in the text (see above).
The arguments to randomize N_{sg} were given in L128-131. Besides, the idea was also to check if rare events with N_{sg} >>1 could be sufficient to enhance the concentration of the small ice crystals but without adverse effect that is a significant decrease of the accumulated rain at ground level.

Sec. 3.2. L206-208: This would not be counteracting effect. There is a reduction in the snow category as well as a reduction in the graupel category.

This is true. Sorry for the coarse mistake. The sentence is "However a further effect is …"

Sec. 3.2. L229-230: This statement needs justification. There should have been more analysis of why the precipitation changes in the different simulations in Sec. 3.1.

In deep convective storms like the STERAO case, rain comes from the melting of the graupel particles and of the snow-aggregates, but with less importance. We showed in Fig. 8 that the extent of the mean profiles of the r_g and r_s mixing ratios is reduced when N_{sg} is increased. Consequently the same occurs for the r_r mean profile peaking at 0.05 gkg^{-1} for N_{sg}=0 compared to 0.04 gkg^{-1} for N_{sg}=10. We admit that the difference is not easy to detect but anyway less graupel implies less rain below the freezing level.
Modifications in the text:
"This change is accompanied by a reduction of r_s (more visible between cases b) and c)) and by a reduction of r_g which clearly stands out at z=8,000 m. The final result is a decrease of the rain mixing ratio r_r, because rain is mostly fed by the melting of the graupel particles."

Sec. 3.3: This section is struggling to properly describe what is going as a result of the lack of explanation of the two moment microphysics scheme. Please define AGGS, CFRZ, and SEDI. These are physical processes, why not just use their names (e.g. deposition-sublimation)?
Overall, its ok that this section is descriptive but it needs to be expanded to properly discuss the impact on each ice microphysical process.

We apologize to keep the acronyms. Reference to Table 3 is made earlier. This is corrected in the revised version.

Sec. 3.4. L274-276: An increase of 135% to 913% when Nsg increases from 2 to 5 deserves a lot greater attention. The authors should conduct more analysis here to find out why this is the case. Saying its "exponential" is not enough. The result is also not tied to what is happening to the ice mass. There needs to be a more comprehensive analysis of what is happening to the ice budget as a whole.

We drew attention to the transition of N_{sg} from 2.0 to 5.0 (log scale) because the peak value of N_i is growing fast in this narrow range, no more. We don't think this corresponds to an underlying physical process in this range of N_{sg}. However,

a saturation effect is slightly noticeable in the evolution of $CIBU_{ef}$ for N_{sg} >10. We agree to smooth the text in order to avoid words like "exponential" and "unrealistic":
"The results clearly show that the growth of N_i is fast when N_{sg} reaches …" and "… leads to a tremendously high N_i peak value."

Sec. 3.4. L276: Another reference to realism without justification.

This is true, see above for the correction.

Sec. 3.4. L280: Why is HIND more efficient here? Is it because the air becomes sub-saturated with respect to liquid water? Why about homogenous ice nucleation? What are HMG and HMS?

Without CIBU (N_{sg}=0), the heterogeneous nucleation process, HIND, is essential to feed the ice crystal concentration. However we can't say that HIND is less efficient when CIBU is activated (the horizontal scale is changing from 13a to 13c). The peaks of the HIND profiles are due to the different nucleating properties of the IFN (Ice Freezing Nuclei), dust black carbon and organics, as compiled by Phillips et al. (2008).
The homogeneous nucleation of the ice starts at a height level colder than -36°C. This process HONH is not very active in this case study. The definition of the acronyms is given in Table 3. HMS(HMG) is the Hallett-Mossop process attached to the riming of the droplets on the "snow-aggregates" ("graupel particles"). Both are treated the same way.

Sec. 3.4. L289: "Equilibrated" is not the right word here. I think you mean "balanced".

Yes, "balanced" is much better.

Sec. 3.4. L290: I gather here that there is that the authors have some understanding of why Ni grows exponentially. This can address my earlier comment if the authors can clarify what they mean here. Why do all of these process rates grow in this fashion?

Well if CIBU is run with N_{sg}=10, then the most important processes to shape N_i are CIBU (source), AGGS and CEDS (both sinks). The sedimentation of N_i (SEDI profile) is also noticeable on the plots of Fig. 13. In passing, it is important to stress that the CEDS process acting on N_i corresponds to the full sublimation of the cloud ice crystals (a local loss of N_i concentration) when these are detrained in unsaturated areas of the cloud surroundings. In a 2-moment scheme mixing ratios and number concentrations must be consistent so

N_i is set to zero whenever the mixing ratio r_i becomes negative (in LIMA, we check for a strict conservation of the mass of condensate and water vapour). However it is clear also that ice sublimation is marginal for the ice mixing ratio in the low levels of the clouds because, as expected, the water vapour deposition dominates the growth of the ice inside the convective cells (see the profiles of mixing ratio r_i in Fig. 9).
So increasing N_i through the CIBU source of ice crystal is compensated by an increase of AGGS (more available crystals are captured by the snow-aggregates) and by an increase of CEDS (more ice crystals are detrained). The other processes revealed in Fig 13a (N_{sg}=0) are not changing very much but their importance is reduced because of changing the plotting scale when moving from Fig. 13a to Fig. 13c.
Modifications in the text:
"The CIBU source of ice crystals is balanced by an increase of AGGS and, above all, of CEDS (here CEDS represents the sublimation of the ice crystal concentration when detrained in the low level of the cloud vicinity, below the anvil for instance). Finally, the "N_{sg}=10" case demonstrates the reality of the exponential-like growth of N_i because the three main driving terms CIBU, CEDS and AGGS are growing at a similar rate that is multiplied by a factor 5, approximately."

Sec. 3.5. L304-305: "Difficult to interpret" is not a satisfactory conclusion here. If the reader is going to be convinced of the very important argument being made in this section, a better effort needs to be made at understanding how the baseline simulations' Ni change with different ice nucleating particle concentrations. I'm also quite concerned that homogenous ice nucleation hasn't been addressed at all.

The reviewer is right. What we meant here is that the mean N_i profiles are not growing with N_{IFN} as for instance we end up with very similar N_i profiles but for N_{IFN} =10 and 0.01 dm^{-3} in Fig 14b. The many terms involved in the ice phase budget (see the profiles in Figs 9a and 13a) explain the difficulty of a bulk analysis. However the important result here is that the number concentration of nucleated IFN follows the initial IFN concentrations as shown in Fig. 14 so the IFN nucleation efficiency is independent of the initial N_{IFN}

Sec. 3.5. L308-310: This statement is unclear. The authors say the nucleated IFN evolve in close proportion to initially available IFN but then the authors are also saying that the IFN do not depend on the IFN concentrations as expected?

No what we wrote is that the nucleation efficiency of the IFN does not depend on the initial IFN concentration. We clarify this point in the revised text.

"In Fig. 14c, the IFN profiles are rescaled (multiplication by an appropriate numbers of powers of ten) to be comparable. Here this is equivalent to computing an IFN nucleation efficiency. The important result here is that the number of nucleated IFN evolves in close proportion to the initially available IFN concentrations, meaning that the nucleating properties of the IFN do not depend on the initial IFN concentration as expected."

Sec. 3.5. L314-322: The conclusions here are struggling to be properly understood and interpreted due to the fact that not enough information about LIMA or the baseline simulation have been give.

We rewrote this part more carefully:
"The last plot (Fig. 14d) reproduces the normalized differences of N_i profiles between twin simulations performed with CIBU and without CIBU. Even if simulations made with the same initial concentration N_{IFN},, diverge because of additional non-linear effects (vertical transport, enhanced or reduced cloud ice sink processes), the figure gives a flavour of the bulk sensitivity of CIBU to the IFN. The enhancement ratio due to CIBU remains low (less than 1 for N_{IFN} ~ 100 dm^{-3}) but it can reach a factor of 20 at 9,000 m height in the case of moderate IFN concentration i.e. for N_{IFN} ~ 1 dm^{-3}. The behaviour of LIMA can be explained in the sense that increasing N_{IFN} too much leads to smaller pristine crystals that need a longer time to grow because the conversion to the next category of snow-aggregates is size-dependent (see Harrington et al. 1995 and Vie et al., 2016). On the other hand, a low concentration of IFN initiates fewer snow-aggregate and thus less graupel particles, so the whole CIBU efficiency is also reduced. Consequently, this study confirms the essential role of CIBU to compensate for IFN deficit when cloud ice concentrations are building up."

Sec. 4. L329-330: I can't agree with this statement. As I've already noted, no justification to this has been given.

We reword the sentences at L328-330 as follows:
"The number of ice fragments that results from a single shock, N_{sg}, is a key parameter which is only estimated from very few past experiments (Vardiman, 1978). A merit of this study is to provide an upper bound to the value of N_{sg} because of the sensitivity of N_{sg} to the simulated precipitation. We found that taking N_{sg}>10 reduces significantly the precipitation at the ground. This is not acceptable since most of the cloud schemes (running without CIBU process) are tuned for quantitative precipitation forecasts."

Sec. 4. L359-360: A quantitative conclusion about the sensitivity of the simulations to different realizations of CIBU (due to changes in observationally constrained parameters) hasn't really been reached.

We reformulate our last conclusion that a complete set of secondary ice production including Hallett-Mossop, CIBU and the raindrop shattering, is of great interest to simulate very high crystal concentrations. To this end it is worth to check if such situations in real clouds are not the result of a cooperative action between the many secondary sources of ice crystals.
"So the next step in the LIMA scheme is to introduce the shattering of the raindrops during freezing as proposed by Lawson et al. (2015) and to compare with CIBU, because the basic ingredients, raindrops and small ice crystals, leading to a different ice multiplication processes are not the same. Then, the final task is to check that microphysics schemes with all known sources of small ice crystals, nucleation and secondary ice production, are able to cooperate and to reproduce observed ice concentrations which can reach very high values (units of cm^{-3}) in deep convective clouds but without convincing explanation yet. Quantitative cloud data gathered in the tropics during HAIC/HIWC (High Altitude Ice Crystals/ High Ice water Content) field project (Leroy et al., 2015; Ladino et al., 2017) could be a starting point to evaluate high resolution cloud simulations with high ice contents."

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