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

Thank you for including a description of the LIMA scheme and the STERAO case study. These help the coherence of the manuscript. Many, but not all, of my questions have been addressed in the author response, and I have no objection to its publication in *GMD*. I am still not sure how the parameterization addresses the discrepancy between ice crystal and INP numbers found *at mixed-phase conditions*, given the highest simulated CIBU contributions at cirrus altitudes. Allowing the process to occur over a wider range of altitudes than in the real atmosphere will certainly affect the results through the vertical latent heating profile and the impact of that heating on dynamics. Some discussion of these considerations could be incorporated.

In line with this kind of discussion, a visualization of the "*upward transport in the convective cells*" of ice crystals formed by CIBU would also be appreciated, since from Figure 9, it seems rather that there is a sedimentation loss from these altitudes. The manuscript could still do with some proofreading because the wording is hard to understand in places.

Specific comments

Line 27 – "*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.*"
This is still poorly worded. Can you simply say: "In contrast to the Hallett-Mossop process, the majority of microphysics schemes do not include the CIBU process."

Line 29-30 – "*Yet, even without absolutely incontestable clues, still missing even in recently published cloud data records"*
I would remove this, as it is superfluous.

Line 41 – It does not make sense to motivate the work by a discrepancy between IWC and INP number. It is a discrepancy between *ice crystal number concentration* and INP number.

Lines 63-64 – It is not clear what an "*asymmetric collision*" is. I would still prefer "mass loss" to "*erosion*".

Line 72 – Remove one "*ice*" from ice number concentration.

Line 84 – "Collisions" is a preferable term to "*shocks*" that are generally electrostatic phenomena (and the latter happens due to ice during lightning formation so the potential for confusion is particularly high).

Lines 113-114 – I am still not clear from the author response how both $D_{s,max}$ and $D_{g,min}$ are chosen based on a single criterion for relative terminal velocity. If it is just a matter of choosing round numbers because there are no other constraints, this should be stated explicitly.

Line 138 – Unless I missed it, you do not mention which nucleation scheme is used. This should be included to know if the nucleation tendencies in Figure 13 should be on the high or low side.

Line 156 – I would still explicitly state "*In a 2-moment* bulk *scheme*."

Lines 196-198 – “*by a multicellular storm*” Please add “over land” here. The STERAO case be “*very classical*” but not all readers will necessarily be familiar with it. I would also say “three 3 K-buoyant bubbles along the horizontal wind direction” if this is what is meant in line 198.

Line 226 – I do not think “*disruptive process*” is a clear description. I would just say “From these simulations, inclusion of CIBU can strongly modify surface precipitation when N_{sg} > 10.0 fragments per aggregate-graupel collision.”

Lines 233-235 – Here again, a direct comparison of ice mass and number metrics does not make sense. Presumably you mean that higher ice crystal concentrations with larger N_{sg} deplete the supersaturation that would otherwise go to snow-aggregate growth. Please say this instead.
Line 242 – Why would one expect any change in the graupel mixing ratio at all since, from Lines 178 to 179, “*the mass of the graupel is unchanged*” in this CIBU parameterization?

Line 257 – Again can you make clear why there should be a reduction in r_g given that the graupel are acting as “passive colliders” in your parameterization?

Figure 9, author response – I understand that nucleation has a much more important impact on ice number than ice mixing ratio. But here and throughout, a motivation to explain ice mass seems misguided to me. Ice-ice collisional breakup was proposed to explain *discrepancies in measured ice number concentrations.*

Around Line 277, author response – I am still unclear about why ice mixing ratio and number concentration peak at different altitudes. In the author response, I am not sure what the “*limiting value dr_i/dt*” means. Can you clarify? There are no *min* functions in Equations 3 to 5.

Line 312-314, Figure 13 – I am curious why the Hallett-Mossop on Graupel process peaks around 5 km if the graupel mixing ratio peaks around 9 km. Is the droplet number large enough to compensate for such low graupel mixing ratios?

Line 305, author response and Lines 340-341 – If the INP number is high enough to deplete supersaturation, you have no homogeneous nucleation. I would imagine that is why you see a decrease in N_i concentration with increasing IFN in Figure 14b.