

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

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.

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).

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.

#### Specific comments

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

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.

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

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.

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?

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.

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  $N_{sg}$ ? What is "unrealistic" about the simulation results for  $N_{sg} > 10.0$ ?

Sec. 3.1. L199-202: This statement is unjustified. As emphasized in the preceding comment, realism of a specific  $N_{sg}$  range has not been established, therefore the writers' conclusion on the choice of  $N_0$  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.

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.

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.

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.

Sec. 3.4. L274-276: An increase of 135% to 913% when  $N_{sg}$  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.

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

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?

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

Sec. 3.4. L290: I gather here that there is that the authors have some understanding of why  $N_i$  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?

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.

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?

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.

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

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.

## References

Sullivan, S. C., Hoose, C., and Nenes, A.: Investigating the contribution of secondary ice production to in-cloud ice crystal numbers, *J. Geophys. Res.*, 122, doi:10.1002/2017JD026546, 2017.

Yano, J.-I. and Phillips, V. T.: Ice–Ice Collisions: An Ice Multiplication Process in Atmospheric Clouds, *J. Atmos. Sci.*, 68, 322–333, doi:10.1175/2010JAS3607.1, 2011.