Review number 2 of “A representation of the collisional ice A representation of the collisional ice break-up process in the two-moment microphysics scheme LIMA v1.0 of Meso-NH”

In their revised manuscript, Hoaraue et al. have addressed some of my concerns but not all of them. I appreciate that more details about the model setup and microphysics schemes have been added. However, I still see no discussion on the limitations of the experimental setup, no justification for why $0.1 < N_{sg} < 10$ is a physically plausible range, and no justification for the authors’ conclusions that more work needs to be done by the measurement community to further constrain this range. Additionally, I did not find that the authors made a real effort to improve the language and readability of the manuscripts. As it stands, I still can’t recommend publication in GMD.

Major concerns

The importance of realizing that results are specific to the experimental setup.

I appreciate the efforts of the authors to investigate a range of N_{sg} values. However, the results haven’t been shown to be robust (e.g. generalizable to some degree to more deep convective cases). That is because the experiments weren’t conducted for different cases, and perturbations to the initial conditions and other details of the microphysics scheme haven’t been carried out. I understand that the authors may not wish to add experiments at this point, but there at least needs to be an emphasis on this being a limitation. The authors did conduct sensitivity tests to the initial concentration of ice freezing nuclei, these may be enough to establish some trend in the impact of CIBU on LIMA, but the authors do not give that part of the study the attention needed to do so.

The authors’ conclusion that a range of plausible N_{sg} has been realized has not been justified.

This in part follows from the preceding critique. Only one case has been simulated, very few changes to said case have been carried out which makes it difficult to conclude that this range can be generalized. In addition, I am still not convinced that a conclusion can be drawn based on how small or large the induced perturbation to the storm dynamics and microphysics. The authors may have a good understanding of this, but they still haven’t communicated it well. Please revise this point. Write a very clear paragraph or even section explaining to the reader why a perturbation of a particular magnitude must not be exceeded when CIBU is introduced.

The conclusion that more measurements are needed to constrain the N_{sg} range.

As the authors note, it is extremely important for a study such as this to guide future measurements. There is some discussion of this in the conclusions, but it’s unclear. Please write a clear paragraph or section indicating the kinds of measurements needed based on the results.

The sensitivity studies are poorly discussed.

I understand the desire to write a short paper. We should always strive to write manuscripts in the least wordy way possible. However, this should not come at the expense of poor elaboration on the results of the experiments. It becomes especially frustrating when the reader reaches the interesting section of sensitivity to initial ice nucleating concentrations and is met with a very limited interpretation of what is happening.

The language remains a limitation.

Unfortunately, many statements made by the authors may struggle to be understood by a reader due to deficiencies in language. I had urged the authors to revise this aspect of the manuscript, but very little effort was made.

Line by line concerns

Sec .1. L54-55. “Huge” is not quantitative. Please replace with an actual enhancement factor.

Sec. 1. L56-57. This sentence is not clear. I’m struggling to understand what “The experiment setup used there was more appropriate to very big” means.

Sec. 1 L58-63. There is no need to clarify what the study is not. This series of sentences can be omitted, assuming the authors can clarify what the study entails in the sentences that follow.

Sec. 2.1 L88-89. Since the authors haven’t introduced what the categories are at this point, they should not expect the reader to understand what “small aggregates covering pristine ice” and “large graupel particles” are. Start by explaining what the categories are, then clearly state which categories are considered for collisional breakup and what size restrictions are applied.

Sec. 2.1 L95. “Symbolic” is not necessary here.

Sec. 2.1 L99. “Simplest” is not necessary here. The writers should say “an expression for alpha which *” where * would state what the assumptions behind the expression are.

Sec. 2.1 L125-140. I still don’t understand this explanation of N_{sg} based on previous work and how it ties to this study.

Sec. 3.1. This is the section where a better job can be done to explain to the reader why a plausible range of N_{sg} can be concluded.

Sec. 3.2 L267-268. “rain is mostly fed by melting of graupel particles”. The authors don’t show r_r production rates from autoconversion vs. melting. Thus, this statement isn’t justified. Consider rewording to something more suggestive.

Sec. 3.2 L266. Avoid using “clearly”.

Sec. 3.3 L276-279. This sentence is not clear. You are stating what the main processes are but simultaneously talking about how AGGS and CFRZ are changing? Please reword.

Sec. 3.4 L300-319. This is too dense. Please expand this explanation.

Sec. 3.4 L306-307. Please clarify that the N_i achieved when not considering CIBU is not the actual concentration of ice nucleating particles, but the resultant concentration of ice. This is an important distinction.

Sec. 3.4 L320-321. “Temporal integration” is too wordy. Consider using something simpler like “time integrated” if that’s what you mean here.

Sec. 3.4 L325-327. Reword this please. “In the case of water supercooling” is not clear.

Sec. 3.5 L351-353. Why is it difficult to interpret? The results seem clear here. I highly urge expanding this section in such a way to discuss the sensitivity to ice nucleating particle concentrations without CIBU first (beyond two brief sentences) then move on to the case with CIBU.

Concerns not addressed in the first round of revision

Below is a list of comments I wrote in the first round that I believe were not properly addressed.

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. 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. 3.4. 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?