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Interactive comment on "Collection/Aggregation in a Lagrangian cloud microphysical model: Insights from column model applications using LCM1D (v0.9)" *by* Simon Unterstrasser et al.

Anonymous Referee #1

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Authors of this paper test the behaviour of a numerical algorithm representing collisions between cloud and rain drops in Lagrangian microphysics schemes. They describe 3 versions of the algorithm (one of the versions is new) and test the convergence of the 3 versions in 3 different settings.

The presented work is based on a similar suite of tests carried out by the same group of authors and published in GMD in 2017 (https://www.geosci-model-dev.net/10/1521/2017/gmd-10-1521-2017.html). In 2017 the tests were done in a 0-dimensional box setup. Now the tests are extended to a 1-dimensional column.

I think that the presented tests are useful and the topic is interesting to the GMD com-

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munity. However, the overall presentation of the convergence of the algorithm should be improved before publication.

I couldn't find what is the criterion for reaching convergence in the tests. Despite being very thorough in testing different parameters and cases, the authors then use vague terms like “equally bad”, “tend to approach the reference” or “seem to converge”. The only way in which the authors show convergence is by plotting many lines on top of each other. This is not satisfying and results in a paper that is overflowing with figures that look the same. I suggest introducing a definition and quantitative measure of convergence. This would allow to change some of the repetitive concentration plots into plots showing convergence as a function of a tested parameter.

I think that the paper should be more concise. The authors carried out a lot of tests but summarising some of them would improve the paper. For example Fig 10 shows results for slightly different initial condition and Fig 12 for a different kernel. Neither of these figures show anything new about the collision algorithm behaviour. Similarly, the authors study the a bin collision algorithm with upstream advection scheme and observe that the results are “slightly smeared out”. The upstream advection scheme is known to be very diffusive and there were many papers published on that. This detracts from the main theme of the paper and the interesting parts of the study.

Additional comments:

Table 1: German language in the caption

Page 3, line 13-14: “Moreover, we will use the term cloud droplets interchangeably with ice crystals to increase clarity in writing.” - I don't think that this increases the clarity. The paper nowhere actually discusses issues related to ice crystals. I think that keeping the language focused on cloud and rain droplets is sufficient.

Figure 1: in SIP (Simulation particle) p should also be capitalised

Page 7 line 15: Smoluchowski equation

Page 8: Maybe it would make more sense to first describe the collision algorithms and then talk about the column model setup?

Figure 2,3 and later in the text: The abbreviations like AON, WM3D, etc were already defined and should not be defined again.

Page 9 line 5: I don't think it's legally allowed and generally acceptable to copy verbatim paragraphs from different papers (?). I would suggest to just refer to the relevant paragraph or to paraphrase.

Algorithm 1 and 2 caption: The style convention of the code block should be repeated in its caption.

Algorithm 2, line 16: iff

Page 14 line 15: "For more sophisticated kernels, including, e.g., turbulence enhancement, the present approach may not be adopted easily as the driving mechanism for collisions to occur in the current model is differential sedimentation (...)." - This is very important for real applications of WM2D algorithm. Could you expand on this? How would you implement the WM2D ideas in a full 3D LES simulation focusing on turbulence effects on precipitation formation?

Paragraph 2.5: Please provide a table which summarises the combinations of different algorithm options and the labels used to distinguish them.

Paragraph 2.5: What is the added benefit of comparing the BIN Bott and Wang collection algorithms in a paper about Lagrangian collection algorithms?

Page 19 line 8: "We found that convergence is usually more easily reached for higher moments than for lambda_0" – Why is that? Would different method of initialising the SIPs make a difference?

Figure 7 and others: Could you show results for t> 30min only? The first half of all those plots shows nothing.

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Figure 7 and others: Why are the Lagrangian schemes always predicting higher concentrations than the BIN scheme?

Figure 14: This figure is very interesting! It's staggering to see how very few of the tested combinations of SIPs lead to any collisions. A strong argument in favour of WM2D approach.

Figure 16: The figure is not clear, especially the yellow dash-dotted lines are hard to see.

Page 34 lines 15-19: If the results are "identical" and "basically identical" then they don't have to be shown again.

Page 35 line 4: This is not a new finding and is out of scope of the paper.

Figure 21: It's very hard to distinguish between the two blue lines.

Page 40 line 14: necessary is repeated

Code availability: It would help to provide a Docker or a Singularity image in which the column model scripts could be run. It would eliminate the need to change any file paths or to install compilers and packages with specific versions. This would be helpful, especially because the provided code is not just pure Python code. I know that it is not a policy of GMD, so it's just a suggestion.

In my case I tried compiling the code with gcc 8.3.0 and gcc/9.2.0 on a CentOS system with Python 3.7.0 and got an error:

AON_Alg.fpp:90:0: error: operator '*' has no right operand #if (KERNEL_INTPOL <= 1) /* logarithmic mass bin*/

I suspect it is some preprocessor issue but I didn't debug further. I then tried compiling with gcc/9.2.0 on an OSX system with Python 3.7.6 and got an error:

sed: illegal option – -

I guess it’s an issue with OSX default sed, but I didn’t debug further.

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