Review of "*Stochastic coalescence in Lagrangian cloud microphysics?*" by Dziekan and Pawlowska, Review of the first revision

I appreciate very much the improvements made by the authors. The scientific content of the paper is novel and original. Nevertheless I believe that the presentation should be still improved in order to help future readers and to make sure it draws the attention it deserves. I still have the feeling that your presentation should be more explicit and that you should guide the readers better through the paper.

Main points

The newly added part towards the end of section 1 is a first step, but each section should start with a motivation and explain the goals behind the next tests. This is already nicely done in Section 4, 5, 6 and 7. On some occasions it may suffice to simply change the order of the presented material.

Some examples:

1. Whereas the intention of showing the size distribution in Fig.1 is clear to everyone and needs no further explanation, most readers probably do not know beforehand why you analyse of sigma(m_max)/E(m_max) and that this quantity is related to the gelation time. Your sentence on page 5, line 30 is essential in motivating what you do and hence should appear earlier.
2. You may still expand the description of the algorithm to make your paper self-contained such that readers are not forced to read Shima. Your whole paper is based on this algorithm, so I think it is worth investing a few more lines. It probably suffices to present a condensed version of their Eqs. 12-19, better explain multiple collections, and stress the point that only integer multiplicities are allowed. It may also help to highlight that the constant N_SD simulation with N_SD=N_0 is different to a one-to-one simulation with N_0.
   Still I think that some formulations are not explicit enough. See e.g. my comment from the last review "you should write that one SD is created"… Contrary to your response, I do not think you already say it. You just say that the radius is randomly selected. Better write that "one SIP per bin is created…".
3. The following comment nicely demonstrates your "implicit" style of writing: The number of used realisations must be mentioned in the text, not only in the figure caption. In section 3, even the fact you analyse a certain number of realisations is not really mentioned directly. It is only implicitly clear by saying "average" or because you analyse sigmas.
4. Could you expand the description of Table 1 in the text? I do not understand the meaning of the third column. Hence, I am not able to reconstruct all parameters of the individual cases. In particular I'm confused about values like 2, 3, 200 etc.

Discussion points

I was wrong in stating that all SD methods solve the KCE (see points 4 and 5 of original review) and you convinced me that the probabilistic nature of the all-or-nothing approach has the same source of variability as the master equation. However, this is not true for the Riechelmann and Andrejczuk algorithms. They are not probabilistic (no Monte-Carlo approach is used, instead they solve the average KCE) and behave differently in the limiting case (explanation follows in the next paragraph).

So I would put it the following way: Some SD methods are based on KCE, some are based on the master equation. Hence, to avoid confusion, your statements throughout the manuscript should be reformulated, as they do not hold for SD methods in general.

In your and Shima's application of the all or nothing algorithm the multiplicities are integer values. Due to the design of the algorithm multiplicities remain integer, if integer values are used at initialisation. This is different in the Riechelmann and Andrejczuk algorithms, they produce real numbers, even for "integer initialisations". So the limiting case of a "one-to-one" simulation does not reduce to the master equation. Hence, my impression is that the various SD methods are not equivalent in the limiting case. Note that the all-or-nothing algorithm can also be applied with real numbers (see Unterstrasser et al, 2017.

The finding in Sec 4 "N_SD >1/9 N_0" has strong implications on the feasibility of LES. May it be possible that with a full sampling of the SD pairs the constraint on N_SD is less strict? You may add at least a "full sampling" line for N_SD=32 in Fig.5 to get a rough tendency. I acknowledge the tests you show in Figs.1&2, but those may not suffice to "prove" the equivalence between the full and linear sampling for all applications.

Minor Points

You define SSA as the algorithm by Gillespie, but later on SSA refers to the algorithm by Alfonso, doesn't it?

p.2., l.24: You miss to cite the Lagrangian cloud model by Sölch & Kärcher,2010. By the way it uses also the "all-or-nothing" approach for particle collisions.

p.3, l.20: I would say that only the opposite direction is true, i.e. all droplets in a SD are identical, but not all identical droplets are necessarily represented by one SD. You can well have two or ten SDs that represent all those droplets with similar properties.

p.4, l.24: you may add "which can only happen if two SDs with identical eta collide".

p.6, l.7: you probably refer to the **exponential** distribution used in Section 3. Check also for other occurences.

To make the connection between Fig. 5 and 6 clearer, you may use the same colours for the squares in Fig 6 as in Fig.5

p.8, l.1: simulational = computational?

p.9, l.6: How can it be that DNS results agree well with KCE results, even though in Fig. 4 you argue that the autoconversion time of KCE (and SDM) is too short?

p.10, l.13: I thought the conclusion would be that rain production is overestimated. What do you mean with amount of rain, mass or number of rain drops? Maybe it is the case, that the mass is overestimated and the number is underestimated?

p.12, l.3: Do you want to say "..it takes until the first droplet grows to r=40um"? Your formulation could cause confusion, as it changes over time, which droplet is the largest.

p.13,l14 and l15: 1.) the ratios must be flipped to get numbers >1.

2.) Did Kostinski & Shaw results depend on system size? Your results do: For N_0=10^4 and 10^5, the ratio goes down from 3.7 to 2.9 (=3912/1336) and 2.3 (=2552/1090). Whats the interpretation of this?

Typos

p.5, l.15: of discrepancies

p.6, l.12: each simulation -> any Bott simulation

ensemble, not ensamble

The reference list contains several small errors. I guess this is mostly due to the fact that in your bib file the paper titles are not embraced by {{ title}}. Then all words appear in lower case, see Alfonso & Raga, Li, Malinowski, ..

Unterstrasser should be cited with the GMD, not the GMDD article.