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Interactive comment on "Stochastic coalescence in Lagrangian cloud microphysics" *by* Piotr Dziekan and Hanna Pawlowska

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General comments:

The present paper describes the use of the super-droplet method (SDM) to study the collisional growth of cloud droplets. The technique is compared with DNS and the master equation. The results are new and original. The manuscript represents a good contribution to the development of new cloud microphysics models and is of potential interest for Atmospheric Chemistry and Physics community. Nevertheless, I have several comments/questions/suggestions that I hope will make this paper even more useful for the community.

Specific comments:

-The methodological section (2) should be expanded. From the current text, it is not possible to detect the equation of motions for the single droplets. Are the droplet trac-

ers or inertial? Or are they just subjected to the gravitational force? A complete set of kinematic, dynamics and radius equation evolution should be given for a more general configuration, and then the system can be simplified depending on the hypothesis introduced by the authors.

-The English level of the manuscript needs to be improved. The most frequent error is the lack of articles in front of many substantive in all the manuscript (in collisional growth→ in the collisional growth, growth rate of lucky droplets → The growth rate of lucky droplets, just to give few examples). The origin derives from the lack of articles in Slavic languages, so the manuscript English level should be more carefully addressed in the next revision.

-The authors compare the DNS case by Onishi et al. (2015). It is not clear in the paper how the comparison has been done. Again, it is not clear if the super-droplets are influenced or not by (and if they move driven by) turbulent fluctuations or if the comparison is done just considering gravitational settling. The latter case would imply that the turbulent fluctuations have a weak effect on particle-particle collisions that it does not seem the case in reality.

-Many important references are missing regarding the methodology: Unterstrasser et al. 2016 doi:10.5194/gmd-2016-271, Li et al. 2017 doi:10.1002/2017MS000930. The latter, in particular, has many analogies and supports the results of the current manuscript.

-It would be interesting to include the effects of condensational growth as stated in the last paragraph of the conclusion. A new Lagrangian stochastic model has been proposed by Sardina et al. 2015 doi:10.1103/PhysRevLett.115.184501. The model could be easily implemented in the super-droplet framework.

-The LES sentence in the introduction can be obscure for non-specialist researchers in the field. The implication of the current approach for LES can be fundamental. The paragraph should be expanded to explain better the concept of LES and why collisions

should be accurately modelled in the absence of small turbulence scales.

-Section 6-Lucky droplets: The values of Kostinski and Shaw (2005) are “estimation”. The sentence: “their theoretical analysis overestimates the luckiness in droplet growth” is too strong, the order of magnitude of their analysis is the same of the one detected with the super-droplet method.

Technical corrections:

-It is hard to distinguish the different lines in most of the plots if printed in black and white.

-For the reader point of view, it is easier if the comparison with the results of Alfonso and Raga (2016) are embedded directly in figure 1 and figure 2 (as the authors already did for figure 5).

-Figure 4: Is it possible to include in the plot the DNS results for a better comparison?

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