Reply to the Referee #1

July 14, 2017

We would like to thank the reviewers for their questions and comments. Before we answer them, we need to point out that we found an inconsistency in the way the collision efficiency tables were interpolated in the SDM and in the Bott method. It only affected simulations that use efficiencies from Hall (1980) for large droplets and from Davis (1972) for small droplets. This kind of collision kernel was used in Sections 5 and 6. The SDM simulations presented in Figs. 7, 8 and 9 were repeated with the problem fixed. The main difference is that now we see convergence of the "one-to-one" results to the Smoluchowski equation in the slow-coalescence case. What did not change is the fact that using larger coalescence cells can decrease the rate of conversion of cloud to rain drops due to additional collisions between rain drops. In consequence, using the Smoluchowski equation can underestimate the amount of rain produced. The problem affected only large drops, with radius greater than ca. 90 microns. Therefore the lucky droplet analysis from Sec. 6, in which droplets grow only up to 40 microns, remains valid.

Answer to the Anonymous Referee #1.

- 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 tracers 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.

We use box model simulations, which are convenient for studying coalescence. Droplet motion is not modelled, therefore we do not give their equations of motion. However, we use gravitational coalescence kernels, so droplets collide as if they settled due to gravitation. To clarify it, Section (2) was rewritten and says:

“ Consider coalescence of water droplets in a well-mixed volume V. Other processes, like water condensation and evaporation, are not included. Thanks to the assumption that the volume is well-mixed, all droplets within the same well-mixed volume can collide with each other, independently of their positions (Gillespie, 1972). Therefore droplet motion does not have to be explicitly modeled and droplet coalescence can be calculated in a stochastic manner, as it is done in the master equation. Consider two randomly selected droplets i and j. Probability that they collide during timestep Δt is $P(r_i, r_j) = K(r_i, r_j)\Delta t/V$, where r_i and r_j are their radii, K is the coalescence kernel and V is volume of the box. We use gravitational coalescence kernels, so the effect of turbulence on coalescence is not studied. ”

- 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.

We made an effort to improve the text. If the manuscript is accepted, remaining errors will be fixed during the copy-editing that is included in the processing charges.

- 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.

We studied coalescence only due to gravitational settling. It is now written explicitly in Section 2:

“We use gravitational coalescence kernels, so the effect of turbulence on coalescence is not studied.”

Onishi et al. (2015) performed DNS both for stagnant and turbulent air. They found that the mean autoconversion time is significantly decreased by turbulence, so turbulent fluctuations do have strong influence on collisions. However, we compare with them not the mean autoconversion time, but the relative standard deviation of autoconversion time and how it scales with the system size. Onishi et al. (2015) show that turbulence can change the relative standard deviation of autoconversion time by about 25%. While this is a significant change, it is barely visible on the logarithmic scale in Fig. 5.

-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.

Both papers are now cited in Section 1:

" A thorough comparison of coalescence algorithms from Lagrangian methods was done by Unterstrasser et al. (2016). It lead to the conclusion that the method of Shima et al. (2009) "yields the best results and is the only algorithm that can cope with all tested kernels". It was also found to be optimal in DNS tests (Li et al., 2017)."

Moreover, Li et al. (2017) is now cited in Section 8:

" Li et al. (2017) have shown that condensation can regulate differences between Eulerian and Lagrangian coalescence schemes. Discrepancies between these schemes that they observed in simulations with condensation and coalescence were smaller than in pure coalescence simulations. "

-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.

We have the option to include condensation in our model. We did not do it, because we believe that it is important to first understand the simpler problem of pure coalescence before dealing with more complex problems.

-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.

We do not include turbulence in our coalescence scheme, so we think that there is no need to explain the concept of LES. We agree that the sentence could be obscure. Moreover, use of the super-droplet microphysics is not limited to LES. For these reasons we changed the sentence so that it does not mention LES anymore. Implications for cloud modeling, including LES, are discussed in Sec. 8.

-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.

We changed that to:

“their theoretical analysis slightly overestimates the luckiness in droplet growth.”

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

In addition to different colors, lines now also have different dashing.

-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).

We have obtained the data from the authors of Alfonso and Raga (2016) and plotted it in the Figures 1 and 2.

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

Onishi et al. (2015) give a DNS result for stagnant air only for one system size. We have added it to the Figure 4. The DNS result is significantly different from the SDM and the Smoluchowski equation results. Following Onishi et al. (2015), we conclude that this is due to the inaccuracy of the Hall coalescence kernel that was used in the latter two. Part of Section 4 that discusses Figure 4 now says:

“ The SDM results are also compared with the results of DNS, in which air turbulence was not modelled, but hydrodynamic interactions between droplets were accounted for. We choose this kind of DNS, because it should be well described by the Hall kernel that is used in the SDM and in the Smoluchowski equation. It turns out that the Hall kernel gives too short autoconversion times. The same issue was observed by Onishi et al. (2015) (cf. Fig. 1(b) therein). ”