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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud-droplet growth due to supersaturation fluctuations in stratiform clouds" *by* Xiang-Yu Li et al.

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> General comments: > This paper presents a DNS study on droplet growth by condensation in > turbulence. The purpose of this study is to explore the influence > of supersaturation fluctuation on the broadening of droplet size > distribution and to investigate the Reynolds number dependency of > the broadening. The microphysics is solved by using the Lagrangian > superdroplet method. By comparing the numerical results from the > condensation at different Reynolds numbers and dissipation rates, the > authors concluded that the supersaturation fluctuations produce broader > droplet size distributions. The manuscript represents a good contribution > to the

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development of new theories for the condensation process and is > of potential interest for Atmospheric Chemistry and Physics community. > However, by careful reading, some arguments in the context may seem > hand-waving and are not sufficiently robust to derive the main conclusion, > and the evidence that the authors have cited are not firmly supportive. I > would suggest that the authors provide more physical explanations and > plots for the arguments. I would support the publication of this paper > after the authors consider carefully the comments listed below.

We thank the reviewer for his/her constructive remarks. As explained below in detail, we have now tried to make our arguments more robust. All our changes are highlighted in blue.

Specific comments: > -Page 6/line 16: My main and critical points to the employed numerical > framework is the choice of the timestep. It is not true that the > Kolmogorov time scale is the smallest of the system. For 10 micrometres > droplet, the particle response time defined in equation (15) is > several order of magnitude lower. Unphysical droplet trajectories can be > generated used such a large time step. Which temporal integration scheme > is employed to solve equation (14)? Saito Gotoh used an implicit scheme > and nevertheless their time step is much smaller than the Kolmogorov > time scale. Can the authors comment on this issue? A validation case > must be provided (at least for one of the low-resolution cases) with a > much smaller time step. If the results differ, an entirely new dataset > must be generated for the paper.

The smallest physical time step in the system is indeed the particle response time. This was always handled correctly in the code, but we didn't describe this correctly. We have now corrected the text in the first paragraph of section 3.1 on page 7. A validation is provided in the response as supplementary material. Since our simulation time step is smaller than the smallest physical time step, a shorter time step gives identical results.

> -If the time step is the Kolmogorov time scale, why is the maximum > time of simulation limited to 80 s? The maximum number of iterations > will be 4000 that is not so

difficult to reach in a supercomputer with > few hours of computational time.

As explained above, the smallest time step in the system is indeed the particle response time. We have now corrected the text in the first paragraph of section 3.1 on page 7. A validation is provided in the response.

> -Why do you evolve superparticles? Can the authors not evolve the actual > number of particles inside the domain? The maximum number of droplets that > need to be evolved is about 30 million that again is not so prohibitive > in a modern supercomputer. State of the art of droplet-laden DNS has > reached much higher droplet numbers. -Connecting the previous points: > How long computational time is needed for the smaller and the larger > case? How many cores have you used?

We emphasized in lines 22-26 on page 3 that "For condensational growth, the superparticle approach (Li et al., 2017) is the same as the Lagrangian point-particle approach (Kumar et al., 2014) since there is no interactions among droplets. Nevertheless, we still use the superparticle approach so that we can include more processes like collection (Li et al., 2017, 2018) in future. Another reason to adopt superparticle approach is that it can be easily adapted to conduct Large-eddy simulations with appropriate sub-grid scale models (Grabowski and Abade, 2017)."

We have now added the description of the CPU cost at the end of section 3.2 on page 7.

> -Page 1/l. 7-8: the adverbs “strongly” and “weakly” (which also > appear in other parts of the manuscript) are not fully supported by the > results provided in the paper. I can see differences below one order > of magnitude smaller between the lines in the plots (e.g. Fig. 4). The > range of Reynolds number is quite limited to appreciate “strongly” > and “weakly” variations. The authors can modify the random forcing > term to achieve higher Reynolds.

We have now replaced "strongly" and "weakly" in the various places in the manuscript

by more precise statements by writing: that \sigma_A is proportional to Re_lambda to the 3/2 power, but only proportional to \bar\epsilon to the -1/5 power.

>- -Page 1/l. 11: the simulations have been done without updraft. The authors should > add a paragraph in the introduction of the effects and consequences of the updraft in > the broadening of droplet size distributions.

We have now added the following discussion at the end of the second paragraph of section 1 on page 2: "When the mean updraft velocity is not zero, there could be a competition between the mean updraft velocity and supersaturation fluctuations. This may diminish the role of supersaturation fluctuations (Sardina et al. 2018)."

> -Page 2/l. 17: Paoli Sharif results are strongly influenced by an > arbitrary forcing term for the temperature and water vapor equations

We have now addressed the "arbitrary forcing term" by saying "turbulence as well as stochastically forced temperature and vapor".

> -Page 2/l. 26-27 (and many other locations in the manuscript): Can the > authors comment on the sentence “solve the thermodynamics” when the > maximum temperature fluctuations of their system are 0.1 K?

The main difference between the present study and that of Sardina et al. is that we solve the supersaturation explicitly by solving the temperature and water vapor mixing ratio field. This is why we say that we "solve for the temperature field". Due to the limited Reynolds number in DNS, the temperature fluctuations are small. In this sense, it is indeed acceptable to treat the supersaturation as a passive scalar. Nevertheless, solving for the temperature and water vapor mixing ratio directly is beneficial when one wants to incorporate entrainment and so on, which is an ongoing project.

> -Page 3/l. 8: Can the authors provide a plot with the ratio between a rms and B rms > where a is the fluid acceleration (the material derivative of the velocity)? My feeling is > that at these small scales buoyancy effects can be neglected.

The forced turbulence becomes stationary. In this case, a_rms/B_rms=250. Therefore, the buoyancy force is indeed small. We have now discussed this in the last paragraph of page 8. The figure showing a_rms/B_rms is attached as supplementary material.

> -Page 3/l. 31: A theoretical issue: the velocity field within the > Boussinesq approximation is divergence free that is not the case. A short > paragraph should be added to justify this theoretical mismatch briefly.

We discussed that "The background air flow is almost incompressible and thus obeys the Boussinesq approximation." in the first paragraph of section 2.1.

> -Page 8/l. 5: How can you see that equation 18 follows a Brownian motion?

We have now changed it to the following, "It can be seen from \Eq{eq:A} that the evolution of the surface area is analogy to Brownian motion," at P.10/l.3.

> -Page 11/l.15: “Therefore, neglecting the smallest scales in the stochastic model is > indeed acceptable”, the stochastic models are derived under the hypothesis of large- > scale separation so that they cannot be applied at Re λ = 40. If you want to show slightly > less dependence repeat the same simulation set up with three different dissipation for > the higher resolution setup.

We have now updated all the figures for different \epsilon with Re_lambmda=130.

> -Page 12/l.3: I guess that the contradiction is due to the presence of updraft

We have now added the following discussion at P.14/l.1-2, "It could also be due to the mean updraft cooling included in the model of Vaillancourt et al. (2002), which was excluded in the present study and in the work of others."

> -The three appendices containing just one definition are not needed, please move in > main text

We have now moved all the appendix to the main text.

> Technical corrections: > -Pag3 3/l.13: is→ are

We have now corrected it.

> -Page 3/l.22: there is a 0 after the citation Li et al, 2017

We have now corrected it.

> -Page 3/l.29: provide a reference for the code

We provided it in the last sentence of the acknowledgement.

> -Page 4/l.11: index and vectorial notations should not be mixed

We have now only adopted the tensor notation.

> -Page 6/l.5: is the nonlinear correction needed? What is the range of droplet Reynolds > number?

It is between 3 to 5.5, which is almost negligible. Nevertheless, we always turn this on so that collision-coalescence can be investigated as a sequential work.

> -Page 6/l.25: I guess the factor 2 β is wrong, otherwise, it would be 2 64 for the larger > case!!!

We have now removed the statement because β is not used elsewhere in the manuscript.

> -Page 6: there is no need to create a new subsection 3.2

Section 3.2 is the DNS, which is to be distinguished from the initial configuration in 3.1.

> -Page 7/l.7: fix Kolmogorov

We have now fixed it,

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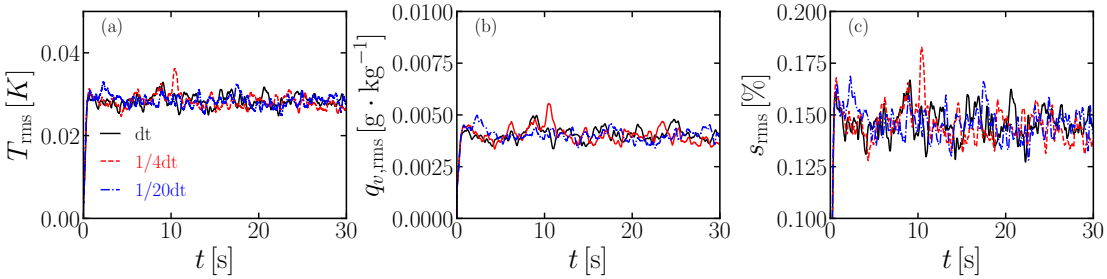

Fig. 1. A validation of the time step

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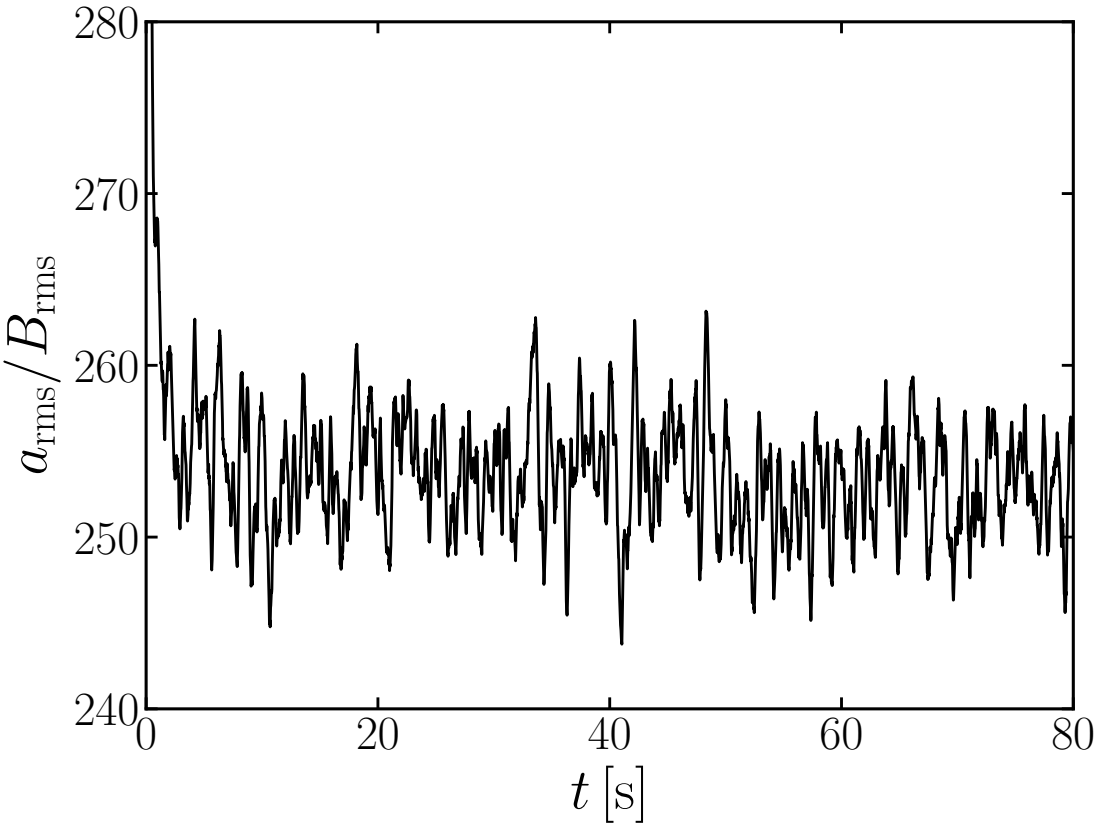

Fig. 2. Plot of a_rms/B_rms

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