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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.

Anonymous Referee #1

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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 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.

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.

-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.

-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?

-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.

-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.

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

-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?

-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.

-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.

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

-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 $Re_\lambda = 40$. If you want to show slightly less dependence repeat the same simulation set up with three different dissipation for the higher resolution setup.

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

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

the main text

Technical corrections:

-Pag3 3/l.13: is→ are

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

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

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

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

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

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

-Page 7/l.7: fix KOlmogorov

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