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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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> The focus of this paper is the influence of supersaturation fluctuations > on droplet condensation growth, which has become an active area of > research in recent years. To have the stratiform clouds as a motivation, > authors have studied this effect in the absence of the mean updraft > velocity. In this study, the conservation of momentum and scalar > (temperature and water vapor) equations are solved using the direct > numerical simulation (DNS) in a rectangular domain and the random > velocity forcing drives the turbulence. Here, the Eulerian scalar and > momentum field is coupled with the Lagrangian droplet dynamics using the > superparticle method. Additionally, the

physics of droplet activation > and droplet collision-coalescence process were ignored. All droplets were > considered at an initial size of 10 um and the starting supersaturation > in the domain was 2%. Authors have examined cases of different Taylor > Reynolds number (Re λ) and mean kinetic energy dissipation rate (). > In general, the approach here is very much similar to that of Sardina > et al. (2015), Siewert at al. (2017) and others. The only significant > difference is the treatment of supersaturation field; in the current > case, it is obtained by solving temperature and water vapor conservation > equations contrary to the assumption of supersaturation field as a > passive scalar in previous studies. Moreover, the authors compared the > results with the stochastic formulation of Sardina et al. (2015) and other > numerical-simulation studies. The results are consistent with the other > studies, the droplet size dispersion (σ A) growth is proportional to > tˆ1/2. Similarly, the broadening in droplet size distribution is shown > to be nearly independent of (a slight decrease), however, it increases > with increase in Re λ consistent with the conclusions of Sardina et > al. (2015).

We thank the reviewer for his/her constructive comments and have now emphasized the novelty of our work in various places. Our detailed response to the reviewer's comments are explained below, highlighted in blue.

> Review points: > - The authors should be clear about the novelty. The main significant > differences between current simulation and previous are the treatment of > supersaturation field and the feedback due to condensation. Although, > authors also acknowledge that the treatment of supersaturation as a > passive scalar is sufficient. Furthermore, they explicitly showed that > the results are independent to the dissipation rate () which was not > clearly presented in the other studies. Please update abstract, intro > and conclusions to make clear.

We have now added the following in: 1. abstract "The supersaturation field is calculated directly by simulating the temperature and water vapor fields instead of treating it as a passive scalar. Thermodynamic feedbacks to the fields due to condensation are also included."

"Also, for the first time, we explicitly demonstrate that the time evolution of the size distribution..."

2. introduction We addressed that P.2/l.29-30: "Neither Sardina et al. (2015) nor Siewert et al. (2017) solved the thermodynamics that determine the supersaturation field." P.3/l.11-12: "where turbulence, thermodynamics, feedback from droplets to the fields via the condensation rate and buoyancy force are all included." P.3/l.15-17: "For the first time, to our knowledge, the stochastic model and simulation results from the complete set of equations governing the supersaturation field is compared."

3. conclusion P13/l5-6: "For the first time, we explicitly demonstrate that the size distribution becomes wider with increasing Re_lambda, which is, however, insensitive to \bar\epsilon."

> - Claimed relevance is to stratocumulus clouds, but entrainment of unsaturated air and > possible secondary activation is know to strongly change droplet size distribution in that > system. How does absence of entrainment limit the results presented? What changes > can be expected when entrainment and activation are included? These limitations > should be discussed.

We have now added the following: P.14/10-13: "Entrainment of dry air is not considered here, which may lead to very rapid changes of supersaturation fluctuations and result in fast broadening of the size distribution Kumar et al. (2014). Activation of aerosols in a turbulent environment is omitted. This may provide a more physical and realistic initial distribution of cloud droplets. Incorporating all the cloud microphysical processes is computationally challenging, which will be explored further in the future studies."

> - Page-6, Line 16: It should be supersaturation instead of saturation.

We have now corrected it.

> - Page-7, Line 7: Fix the typo

We have now fixed it.

> - The assumption used to get the eq. 20 is not required to derive the equation for σA growth.

The scaling law \sigma_A~t^{1/2} does not require the assumption. However, to obtain eq.23, T_0»\tau_phase is needed. We have now added the explanation below eq 22 on page 10.

> - The phase relaxation time might be changing with time due to the mean radius growth > (specifically, at the starting since there is a starting supersaturation around 2%). It > might cause some deviation in the result (σ A vs t) from the t 1/2 relation. Authors should > discuss this effect along with the discussion of figure 4.

We have now added a discussion in the last paragraph at P.12/l.9-12.

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