Responses to comments of the Referee #1.

Below we respond to the reviewer comments. The original comments are in the black color, our responses blue.

My understanding of the "scaled-DNS" presented in this manuscript is that one can simulate a large domain size by artificially increasing the kinetic viscosity of the airflow. The Reynolds number is kept unchanged in such simulations so that the computational cost is still feasible. Therefore, one can study how the supersaturation fluctuations can be affected by the large eddies. This is plausible as the small scales do not matter for the supersaturation fluctuations. The authors further tested the application of the superdroplet approach in such a setup to tackle the condensation process. I would recommend the publication of this manuscript after the authors carefully discuss the following comments.

The reviewer's summary of our methodology is correct.

The author addressed both in the abstract and in the conclusion that this is a "novel methodology". This statement should be treated carefully for the following reasons: 1. The method presented in this manuscript is DNS with large artificial kinetic viscosity, which is not a new method. 2. Mellado et al also used the same treatment (section 3 of Mellado et al).

We do not agree with this comment. Yes, Mellado et al. applied large domain in their simulations of the stratocumulus top. However, nowhere in Mellado et al. we find the formula showing how the viscosity needs to be increased to allow appropriate dissipation when the model grid length is far away from the dissipation length. This is not that important for the finite-difference model as long as proper dissipation is accomplished by the advection scheme as in the so-called implicit large eddy simulation. But the rescaling is critical for the spectral model that by design has no numerical dissipation. Our methodology targets traditional homogeneous isotropic turbulence simulation applying DNS that in its original form cannot consider droplet growth in large domains. We believe the methodology we propose and use is novel. That said, we added a reference to Mellado et al. and a few other papers (per comments by Rev.1 and 2) in the introduction.

Specific comments:

1. The paper by Mellado et al applied the same idea. Can the authors compare their work with the one of Mellado et al?

Mellado et al. applied similar technique to the cloud top simulation applying bulk condensation scheme. We are not sure what is there to compare.

2. Can the authors check Eq.3 again? If dR/dt=KrS/R, then R^2/2=KrS, together with Eq.2, you will get a pre-factor of 2 instead of 4/3 in Eq.3, right?

No. Eq. 3 does not come from Eq. 2. Eq. 2 is the condensation rate applying the analytic droplet growth rate equation that is shown in the text below Eq. 2. (We corrected an error in Eq. 2 pointed out by Rev. 2). Eq. 3 is the condensation rate derived from the change of the droplet volume as predicted applying a finite time step. Eq. 3 ensures that the mass of water (water vapor plus droplets) is conserved because condensation rate as given by Eq. (3) is applied in the water vapor equation.

3. L.125: Should the intensity of turbulence determined by the single parameter, Reynolds number? The energy dissipate rate is a small-scale quantity which describes how fast energy dissipates in the dissipation range of turbulence. In other words, it characterizes how vigorous the small eddies of turbulence are. It is calculated from the trace of the strain tensor. This aspect is also discussed by the authors in the paragraph just below Eq.10. Did you mean the energy transfer rate here, which is the rate energy transfers from large to small eddies in 3-D turbulence?

Reynolds number describes the range of scales between the scale of energy containing eddies (about the size of the domain in DNS) and the dissipation scale, see Eq. 5 in the paper. The intensity of turbulence is determined by the eddy dissipation rate. Yes, the eddy dissipation rate is a small-scale quantity, but this is how intensity of turbulence is expressed in models and in observations. TKE depends on the eddy dissipation rate and on the characteristic eddy scale, Eq. 4 in the paper. This is why TKE increases with the domain size as shown in Fig. 2 (upper row), and the TKE dissipation rate is the same (lower row).

Perhaps a discussion in Grabowski and Abade (JAS 2017, p. 1485) can also help.

4. Can the authors normalize the energy spectrum in the same way as the one of Fig.1 of Li et al. (2019)? If the Reynolds number is the same, the normalized spectrum should collapse on top of each other.

This can be done. However, we feel the figure in its current form is more informative for a cloud physics reader that we target in our manuscript. The figure clearly shows the shift of spectrum with the increase of L. Also, the figure in the current form illustrates the TKE increases with the domain size for the same TKE dissipation rate. We prefer to leave the figure in its current format.

5. Why is there an initial spike in Fig.6? I don’t understand why it is different for different domain sizes. The water mass loading is the same for all the simulations, right?

First, please note that the spike amplitude is very small compared to the standard deviation shown in Fig. 7. We mention that in the revised manuscript. Second, initial conditions are not in the equilibrium with droplets that are present locally, and establishing quasi-equilibrium takes some time. However, one may argue that this should not take as long as the figure shows. Another aspect is that “molecular diffusion” (in quotes as this is the scaled-up molecular diffusion), in addition to the local vertical velocity, impacts droplet growth. In fact, the duration of the spike (longer for larger domains) suggests that the diffusion is likely the key factor. This is where the absence of unresolved scales in the scaled-up DNS may play some role. Because those transient conditions are just an artifact of the modeling setup, we do not think exploring this any further is warranted.

6. As the integral time scale is different for simulations with different domain size, could it be an idea that the authors normalize the time axis by the integral time scale?

We included two horizontal axes on bottom panels of figure 2 per reviewer request.

Technical corrections:

1. Fig.4, caption: standard deviation of supersaturation fluctuations?

2. L248: When “the” multiplicity. . .

Corrected.