Responses to comments of the Referee #2.

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

Overall, we feel the reviewer missed the key aspect of the paper: the largest turbulent eddies have the most significant impact on the diffusional droplet growth. Our desire to simulate the large-eddy end of the spectrum, and not scales close to the molecular dissipation, is the main motivation for the scaled-up DNS approach. The motivation has been discussed in the two paragraphs starting at line 64 of the introduction that sets the stage for the entire manuscript.

This study uses a modified DNS to investigate the effect of turbulence on the diffusional growth of cloud droplets in much larger domains (up to 64*64*64 mˆ3) compared with traditional DNS. The method is the combination of increasing air viscosity to allow DNS in a larger domain and using Lagrangian particle-based microphysics to lower the number of particles needed to be tracked in a larger domain. Results show that the variance of droplet radius increases with the square root of time, which is consistent with previous studies and the result from a stochastic model. Different domain sizes and multiplicities are also tested to check the convergence of the scaled-up DNS. The paper is interesting. However, I have some comments needed to be addressed before this manuscript can be accepted.

This is approximately correct summary of the paper. A small clarification: results from the stochastic model show that the supersaturation fluctuations in the domain are consistent with scaled-up DNS. The figure does not show the droplet radius variance.

Major comments:

1. Running DNS in a larger domain is not new, for example, Rotunno and Bryan (2018) recently applied direct numerical simulation to study lee vortices in a larger domain by setting viscosity as 1 m2/s. Although the combination of this technique and Lagrangian cloud model is interesting, it is not clear to me whether it is suitable to do that, or at least what we can learn from it, for the following reasons:

The lack of novelty comment echoes comments from the Reviewer 1. In general, running a fluid flow model with constant mixing coefficients, such as in Rotunno and Bryan (2018), goes long way back. See, for instance, Grabowski and Clark (JAS 1993, p. 555) for an earlier example. Another example is the so-called implicit large eddy simulation (ILES) approach where there is no explicit mixing and the computational stability is ensured by the numerical algorithm (i.e., the monotone finite-difference advection scheme). Such an approach is being used by many (see, for instance references at the beginning of section 3 in Grabowski JAS 2014, p. 4493). However, ILES would not work for a spectral model because proper dissipation at small scales is missing. In our study, we utilize a spectral DNS code with scaled-up viscosity and diffusivity, and use the code in simulations for domains that are impossible to consider with the traditional DNS. The basis of such an increased dissipation and its application for large domains is the key novelty of our study.

Because of this comment and a comment from the Reviewer 1, we added a couple sentences in the introduction of the revised manuscript.

1.1 When air viscosity is changed, it is unclear to me whether thermal diffusivity is also changed accordingly in this study. If not, the Prandtl number will be different compared with air. It means that this is a fluid that does not behave as air. If yes, this will be consistent with R&B (2018).

Thermal diffusivity is also changed in the same way as the viscosity. This is clearly stated in the abstract and we added a comment on that in section 3 below Eq. (8). The R&B study is irrelevant in this context, see above.

1.2 Changing air viscosity and thermal diffusivity will also slow down the condensational growth of cloud droplets following physical rules. But I guess this effect is ignored in this study. Therefore, the scaled-up DNS in this study simulates a turbulent cloud system that is not similar dimensionless to natural clouds. Therefore, it is unclear to me what we can learn from it. I hope the authors can comment on this.

This comment is incorrect. The molecular coefficients applied in the droplet growth equation are not scaled up and the typical droplet growth equation is used, see below Eq. (2) in the manuscript. The key for the condensational growth are the supersaturation fluctuations. Their magnitude increases with the turbulent eddy size and we aim at producing fluctuations typical for large eddies. The supersaturation fluctuations obtained from scaled-up DNS are consistent with the stochastic model as shown in figure 10. In short, the comment that we simulate a "cloud system that is not similar dimensionless to natural clouds" is simply incorrect.

2. For the same energy dissipation rate, if air viscosity increases, Kolmogorov's length scale increases and therefore a coarser resolution can be used. However, Komogrov's velocity scale also increases at the same time. Larger velocity fluctuation in a larger domain leads to larger supersaturation fluctuation (Figure 7), and thus faster broadening of droplet size distribution (Figure 9). The reason that the variances of droplet size distribution from DNS/scaled-up DNS are consistent with the stochastic model (Figure 10) is that they generate/use the same strength (e.g., PDF) of the velocity field. Therefore, their agreement and the consistency with the tˆ(1/2) scaling law is not a big surprise to me.

This comment provides an approximately correct summary of our results. However, the reviewer confuses increase of the Kolmogorov scales with the increase of the supersaturation fluctuations. Please see our response to the next comment.

The fact that the results are as expected supports validity of the proposed approach. Yes, it is expected that larger eddies feature larger and longer-lasting supersaturation fluctuations and thus have more impact on the spectral broadening. However, specific details (such as the spectral width increase with the domain size when following t^1/2 scaling, fig. 9) can only be obtained with the scaled-up DNS.

An important comment is that the approach developed in this paper is being used in ongoing studies with more realistic systems, such as a rising parcel with CCN activation. To be

relevant to a turbulent cloud, domains larger than a mere few meters are needed. This aspect is mentioned in the final paragraph of the paper.

However, in an idealized homogeneous isotropic turbulent cloud, the velocity (supersaturation) fluctuation should be independent of the volume we choose, meaning that in either a volume of 1 mˆ3 or 10 mˆ3, the energy dissipation rate, Komogrov's length scale and velocity scale should be the same. Therefore, it is unclear to me how the simulated cloud in a domain size associated with the increase of Komogrov's velocity scale, is related to the conceptual cloud with the same large domain with the original (smaller) Komogrov's velocity scale. Please comment on it.

The initial part of the comment is incorrect. The velocity and supersaturation fluctuations **increase** with the domain size when the eddy dissipation (and thus Kolmogorov scales) remain unchanged. This has been shown in past DNS studies that we refer to and is the key of the argument; see also Grabowski and Abade (2017). The largest supersaturation fluctuations come from the largest eddies, and these are related to the turbulent kinetic energy (TKE). This is because TKE represents energy of the largest eddies of a turbulent flow. TKE increases with the domain size and the correct scaling is given by Eq. 4 in the paper (line 129).

3. Figure 4 shows that results converge for different multiplications. As stated around line 257 "Note that for real DNS (Table 2 and Fig. 4), having a droplet in one of several dozens of grid volumes still results in supersaturation fluctuations in agreement with real droplets." However super droplet even with the multiplicity equals to 1 (real droplets) is one out of eight grid boxes, meaning that the density of droplet is low in the domain. Is it possible the evolution of the supersaturation fluctuation shown in Figure 4 is just the background even without droplet? I think a more careful test is to track more particles in real DNS, at least 10 per grid box, and then change the multiplicity but maintain the same number of particles in the domain.

The initial conditions for the simulations are as in Lanotte et al. DNS study, droplet concentration of 130 per cc and droplet radius of 13 microns (see line 119 of our ACPD paper). Indeed, clouds are fairly diluted systems. However, it is not true that the fluctuations for the case with droplets are the same as without droplets. The supersaturation variance in simulations without droplets for the two domain sizes shown in Fig. 7 are about 8×10^{-4} for the left panel and about 1.6×10^{-3} for the right panel, that is, larger than the mean of those shown in the figure. We do not think we need to discuss this in the paper.

The reviewer's suggestion at the end of the comment does not make sense. Droplet concentration (130 per cc) implies that it is impossible to have 10 droplets per grid box if the grid length is 1 mm. There are on average 0.13 droplets per grid box, or one droplet in about 8 grid boxes as the reviewer notices.

Minor comments:
1. Line 17: "mean droplet radius variance", should it be "droplet radius variance"?

2. Line 50: “however, see the comment on that paper by. . .” This sentence is not clear to me.
3. Line 101: “K is the molecular diffusion coefficient” should be “thermal diffusion coefficient”
4. Equation 2: I think \delta t should be removed to make sure the unit is correct. Please check.

These comments have been addressed in the revision.