Responses to comments of the Referee #3.

Below we respond to the reviewer's comments. The original comments are in black color and our responses are in blue. We do not agree with many suggestions as they seem to put our manuscript in a context that we feel is not appropriate for this work. Please see specific responses below.

This study applies the scaled-up DNS method to simulate supersaturation fluctuations and spectral broadening in an idealized framework of forced, isotropic turbulence. The supersaturation fluctuations are produced by the turbulent vertical motions. Scaled-up DNS is what I would consider to be the simplest possible form of large-eddy simulation. In scaled-up DNS, the molecular diffusivities are increased to maintain the same kinetic energy dissipation rate as an otherwise identical simulation with a smaller grid size. In the present application, the range of scales between the domain size and grid size was maintained. Droplet condensational growth was represented using the superdroplet method. The impact of the droplet multiplicity of the super-droplet method on the simulations was studied. The results (i.e., supersaturation standard deviations) of the scaled-up DNS were compared with those from a stochastic model, and exhibited very good agreement.

Specific Comments

1. lines 36-45: What is described here is not the only mechanism by which supersaturation fluctuations can be produced within a cloud, and probably not the most important. Entrainment and mixing also produce supersaturation fluctuations and are well-known to be an important source of spectral broadening, and should be mentioned here in order to place the focus of this study in proper perspective. As will be mentioned later, the set-up of the simulations actually implies an external forcing, which could be interpreted as a crude representation of entrainment.

Although we agree in general with the reviewer's comment, we do not think bringing detailed discussion of entrainment and mixing is needed. The paper discusses a very specific aspect of the homogeneous isotropic turbulence impact that was studied in the past applying DNS. Our approach extends those studies and targets DNS community. That said, we modified the introduction and brought several references to entrainment and mixing. We do not understand the last sentence – the simulations are forced in a way typical to traditional DNS.

2. lines 46-47: It would be appropriate to refer here to the even earlier study by Su et al. (1998) in which diffusional growth of cloud droplets in a turbulent environment was simulated using the Explicit Mixing Parcel Model, which is essentially a 1D kinematic DNS. lines 54-56: Droplet sedimentation was included in the EMPM simulations mentioned.
Su, C.-W., S. K. Krueger, P. A. McMurtry, and P. H. Austin, 1998: Linear eddy modeling of droplet spectral evolution during entrainment and mixing in cumulus clouds. Atmos. Res., 47–48, 41–58.

This part of the text has changed. We added several references to entrainment and mixing, including the one suggested. We do not want to single out EMPM as it is only marginally relevant to our study. Please see our response to 1 above.

3. line 57: Unclear. What has a “small impact on the droplet spectra"?

The supersaturation fluctuations. Text modified.

4. lines 56–64: State what mean supersaturation was used in these studies.

Vaillancourt et al. applied a rising adiabatic parcel setup. The mean supersaturation was never presented. Lanotte et al. and Li et al. applied no mean vertical motion (as in our study) and started with zero mean supersaturation. This is now mentioned in the modified text.

5. line 65: It is possible to do simulations with larger domains with the EMPM. It would appropriate to mention here that the EMPM simulations reported in Su et al. (1998) used a 20-m domain size, and EMPM domains up to 100-m domains were used in Tölle and Krueger (2014). Tölle, M. H., and S. K. Krueger, 2014: Effects of entrainment and mixing on the droplet size distributions in warm cumulus clouds. J. Adv. Model. Earth Syst., 6, 281–299, doi:10.1002/2012MS000209

As stated in our response to 1 and 2, we do not want to bring entrainment/mixing in this manuscript except in a brief comment in the final paragraph in the conclusion section.

6. lines 67–69: Be clear about the source of the supersaturation fluctuations that you are referring to, which vertical gradients of potential temperature, not entrainment and mixing. Noting this also makes the explanation of why large eddies produce larger fluctuations more obvious.

There are no mean temperature gradients in the traditional DNS and scaled-up DNS. In the framework we use, larger supersaturations come only from larger and longer-lasting fluctuations of the vertical velocity. We modified the discussion in this paragraph.

7. lines 69–72: It would also be appropriate to mention the EMPM approach here, because it certainly does perform “DNS-like simulations in computational domains comparable to the size of the LES grid box." Furthermore, the recent development of a linear-eddy based SGS model that is combined with the super droplet method by F. Hoffmann should be mentioned. Hoffmann, F. and G. Feingold, 2019: Entrainment and Mixing in Stratocumulus: Effects of a New Explicit Subgrid-Scale Scheme for Large-Eddy Simulations with Particle-Based Microphysics. J. Atmos. Sci., 76, 1955-1973, https://doi.org/10.1175/JAS-D-18-0318.1 Hoffmann, F., T. Yamaguchi, and G. Feingold, 2019: Inhomogeneous Mixing in Lagrangian Cloud Models: Effects on the Production of Precipitation Embryos. J. Atmos. Sci., 76, 113-133, https://doi.org/10.1175/JAS-D-18-0087.1

We do not agree that this aspect needs to be included in the manuscript. We see similarities between DNS and EMPM, but this is only tangentially related to the main thrust of our manuscript.

8. line 100, Eq. (1): In general, this equation should include a term w dT /dz . I suspect that this term is missing because dT /dz ‾ = 0 is enforced due to the cyclic b.c. at the top and bottom boundaries. It this is the case, it should be mentioned. It should also be mentioned that forcing dT

/dz = 0 is equivalent to forcing a nonzero gradient of potential temperature, which acts as the source of temperature and supersaturation fluctuations.

The reviewer is correct. DNS by design cannot feature mean temperature gradients because of the triply-periodic boundary conditions. This is why Eq. (1) does not have the w dT/dz term. Eq. (1) is standard for the DNS of homogeneous isotropic turbulence (e.g., see Eq. 9 in Vaillancourt et al. JAS 2001). We prefer not to bring this aspect in the model description.

9. line 111. Eq. (3): State how changes in R3 are calculated if not by using (2).

We added a brief comment on that. The key is that droplet growth is calculated first, and then condensation rate follows from (3).

10. line 118: Please state the initial conditions, particular the initial temperature profile, as well as the initial supersaturation profile.

There are no profiles in the spectral DNS. The initial conditions are specified in the last paragraph of section 2 of the original submission. We added information about the assumed temperature and supersaturation.

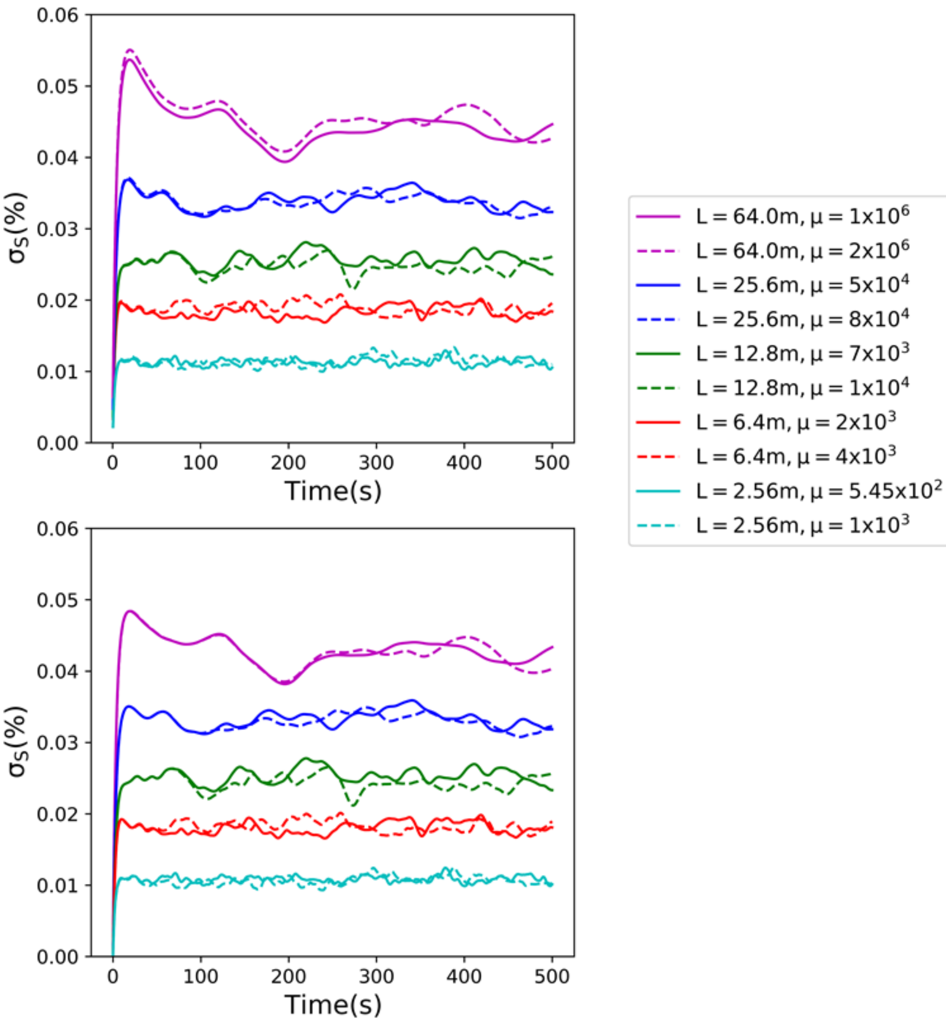
11. Figure 2: It would be enlightening to the readers to discuss how the TKE can be made non-dimensional in terms of flow parameters (a velocity scale specifically).

We are not sure what the reviewer has in mind here. The velocity scale comes from TKE, see section 5. Perhaps the discussion in Grabowski and Abade (JAS 2017) can help.

12. Figures 6–8: Please state whether the supersaturation statistics plotted are at the droplets or everywhere in the flow. They are most useful for understanding the DSD properties if they are at the droplets, as those for the stochastic model are.

This is a good point. For DNS, the statistics are for the flow and we mention this in the revision. At some point we compared the statistics for the flow and at the droplet positions. The differences between the two methods are small as shown in the figure below, so we decided to use the flow statistics as a much simpler to calculate. We feel this is because gradients at the grid scale are small due to molecular (or scaled-up molecular) transport coefficients and droplets (or super-droplets) present only in the small fraction of grid volumes.

The upper panel in the figure below shows the statistics derived using the flow data. The figure includes some data shown in the manuscript's Fig. 7. The bottom panel shows statistics calculated by interpolating the supersaturation to the droplet position. There are some small differences, but the mean values are close. We mention that in the footnote of the revised section 4.



13. Figure 7; It would be enlightening to the readers to discuss how σS can be made non-dimensional in terms of flow parameters.

We do not think this is possible. There are several parameters that likely play the role, and only some (like the domain size) vary in our study. Perhaps Fig. 10 provides something along the lines suggested by the reviewer.

14. Figure 9: It would be enlightening to the readers to discuss how σR2 can be made non-dimensional in terms of flow parameters.

Please see our reply to 13 above.

A general comment to 13 and 14 above: the problem has several parameters. From the fluid flow, the eddy dissipation rate and the domain size are the key (see Grabowski and Abade JAS 2017). From the cloud droplet perspective, the droplet concentration and initial size are relevant. We doubt our limited set of simulations, used primarily to demonstrate the approach strength, allows to draw specific conclusions in terms of nondimensional parameters. This has to be left for future follow-up studies.

15. lines 272-73: Clarify this sentence: "The fluctuating in space supersaturation in the dynamic simulations (i.e., real DNS or scaled-up DNS) is modelled as independent realizations of the fluctuating in time supersaturation." I am not sure what you mean. Modelled by what? The stochastic model? Also clarify that the stochastic model predicts supersaturation fluctuations at the droplets. (See comment 12.)

The text has been modified to better explain how this is done in the stochastic model. The fact that the stochastic model predicts supersaturation in the droplet vicinity is mentioned in the footnote in section 4.

16. line 283 and Figure 10: Clarify that the stochastic model predicts supersaturation fluctuations at the droplets.

See response to 15.

17. line 317: Please explain why small-scale motions would reduce σS.

Just simply because of the heuristic argument that missing scales of motions would provide additional smoothing of supersaturation gradients. See also our response to 21 below.

18. Section 6: It would probably be helpful to many readers to make clear how scaledup DNS differs from LES. In my view, it is really LES but with a constant eddy viscosity, which is not a good SGS model. Explain why you chose this approach rather than using a better SGS model? I would also suggest that you discuss the advantages and the disadvantages of the scaled-up DNS approach. For the problem addressed, it seems that the stochastic model captures the important physics, and that the scaled-up DNS does not add any additional insights.

We do not agree with this comment. The paper shows how to expand DNS to larger domains at the same Reynolds number and thus to include larger eddies featuring larger supersaturation fluctuations. We think a better analogy is the so-called implicit LES, that is, an approach used in finite-difference models where no SGS scheme is used. Unfortunately, such a technique cannot be used with a spectral model and this is where the scaled-up DNS fits. We mention this in the revised introduction.

19. lines 332-33: I agree with this statement. You may want to mention again other possible methods that have been developed (such as those mentioned in comments 2, 5, and 7).

Again, we specifically target homogeneous isotropic turbulence simulation methodology. We do not want to bring other methods as not relevant to what we present.

20. lines 365-6: This might be too general of a statement. The large eddies dominate for this mode of supersaturation fluctuation because they span a larger potential temperature difference for the same mean vertical gradient. For other modes of supersaturation fluctuation generation

such as entrainment, large eddies also dominate, but for a different reason (their greater mixing time scale).

This comment is incorrect. Larger eddies feature larger and longer-lasting vertical velocity fluctuations because of the way TKE scales with L for the same eddy dissipation rate. As explained above, spectral DNS has no mean vertical gradients.

21. lines 381-82: "Finally, one can also consider applying scaled-up DNS in simulations of the turbulent entrainment and mixing similar to those discussed in Kumar et al. (2018)." There is a significant drawback for this application of scaledup DNS due to the importance of the small-scale supersaturation fluctuations in determining DSDs. Such near-Kolmogorov-scale variability is not present in scaled-up DNS.

It remains to be seen if the small-scale supersaturation fluctuations are important compared to large-scale fluctuations. The reviewer seems to believe so. We are not sure. Grabowski (JAS 2020) makes that point while discussing ILES simulations of the Pi chamber. He states that only true DNS can answer this question through the comparison with LES or ILES. We added a reference to Paoli and Shariff (2009) that is yet another method to include the impact of entrainment/mixing into DNS and scaled-up DNS in addition to Kumar et al. (2018).

Technical Comments

1. line 139: It would be helpful to define L1 and L2.

Text modified.