

Interactive comment on “Diffusional growth of cloud droplets in homogeneous isotropic turbulence: DNS, scaled-up DNS, and stochastic model” by Lois Thomas et al.

Anonymous Referee #2

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I'd like to thank the authors for their detailed response. I agree with most of their comments except the one below:

The author said that “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.” However, the growth equation does depend on the coefficients of diffusion and thermal conductivity, which is embedded in the condensational growth equation. For example, check eq. 7.17 in the textbook of Rogers and Yau. In this study, it is assumed to be constant. I am not to say it is forbidden to use a constant. However, justification, clarification, or comments should be added.

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In addition, I might not clearly represent my second comment. What I am considering is the following three cases:

A. a small domain (e.g., 1 m^3), energy dissipation rate, air viscosity

B. a large domain (e.g., 10 m^3), same energy dissipation rate (not TKE), same air viscosity

C. a large domain (e.g., 10 m^3), same energy dissipation rate (not TKE), larger air viscosity

In this study, A and C are compared. But I think A and B are more realistic. I know it is impractical to simulate a large domain using DNS without changing the air viscosity. I just curious what if DNS can simulate a large domain (without changing the air viscosity) in the future, is there any difference between the results from B and C the authors expect to see.

In the end, as I said in my comments, this study is interesting, and this technique (DNS in large domain + Lagrangian super droplet) can be applied for other research topics, e.g., as the author mentioned, a rising parcel with CCN activation. But some clarifications related to my comments can help the reader, at least me, to better understand the merits and limitations of this study. I am appreciated for this.

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