Reply to Referee 3

We are deeply grateful for the referee's comments on our paper. Following your comments, we revised the manuscript. Our responses to your comments are as follows. Lines are those in the revised version. For convenience, we attach a supplemental material which is the same as the revised manuscript except that the changed parts are written in red color.

1. *The authors suggest that the current "eddy hopping model" has limitations in representing the correct scaling at small scales. A correction to the model is introduced (an additional drift term representing the turbulent mixing). A further modification is introduced by multiplying the timescales with constant coefficients that adjust the magnitude of the supersaturation variance. One of the corrections was already included in earlier papers (additional drift term from turbulent mixing), although the evaluation of the supersaturation variance scaling was not presented before.*

   Thanks for the comment. This point ["*One of the corrections was already included in earlier papers (additional drift term from turbulent mixing)*"] was also mentioned by the other referee. Additional drift term representing for turbulent mixing was first introduced into the eddy-hopping model by Abade et al. (2018), and therefore our contribution should be stated as validation of the model, rather than "correction" to the model. Following this comment, we made revisions as described below.

   In the revised manuscript, we first made the following revisions to correctly describe the contribution by Abade et al. (2018):

   - Line 21–23: A sentence "*Abade et al. (2018) extended the model ... due to turbulent mixing.*" has been inserted.
   - Line 125, section 4: The section title has been changed.
   - Line 126: A sentence "*We next consider ... as follows:*" has been inserted.
   - Line 131–132: A sentence "*The important change ... in Eq. (19).*" has been inserted.

   We made the following revision to distinguish two versions of the model:

   - Line 23–24: A sentence "*For clarity, we ... the second version.*" has been inserted.

   Accordingly, we removed the word and the phrase such as "corrected", "corrected model", and so on, and we refer to the eddy-hopping model developed by Grabowski & Abade (2017) as the "original version", and the extended model by Abade et al. (2018) as the "second version" in the revised manuscript. Also, to correctly describe our contribution, we made the following revisions:

   - Line 4–6: A sentence "*Two versions ... simulations (LESs).*" has been inserted.
   - Line 31–36: Three sentences "*These statistical properties ... leads to improvement.*" have been inserted.
   - Line 254–256: Two sentences "*we showed that ... the reference data.*" have been inserted.

2. *The new simplified model for the super-droplet method is good. But it might not provide much computational benefit if the subgrid-scale transport of super-droplets is also needed.*

Thanks for the comment. This was also pointed out by the other referee. Indeed, the simplified model does not necessarily lead to a reduction in computational cost when not only S' but also w' is used for the subgrid-scale parameterization in LES.

In the revised manuscript, we removed the phrase such as "reduction in computational cost" and instead used the phrase such as "reduces the number of model variables". We also removed the discussion on computational cost of the simplified model in section 5 and only discusses the convergence property of the model. Revisions are as follows:

- Line 7–8, Line 36: A phrase "*which may contribute to a reduction in computational cost*" has been replaced by "*which reduces the number of model variables*".
- Line 230: The item discussing possible reduction of computational cost ("*1. Reduction of computational cost. ... reduction in computational cost.*") has been removed.
- Line 260–262: The sentence "*Since the assumption ... after the simplification.*" has been replaced by "*This convergence property ... Lagrangian cloud model.*".

3. *L120-125 and Eq. 19: I agree the drift term due to turbulent mixing is necessary for correctly representing the supersaturation fluctuations. In fact, it was included in some of the past studies. However, a corresponding complementary diffusion term (the Wiener increment term) representing small-scale fluctuations/mixing would also be required in the corrected model.*

   Thanks for the suggestion. Yes. It would be possible to further extend the second version of the eddy-hopping model [by Abade et al. (2018, JAS)] by additionally introducing the Wiener process term which represents small-scale fluctuations/mixing. Such terms are actually included in the Langevin model of the supersaturation fluctuation considered in the previous studies: for example, Eq. (42) in Paoli & Shariff (2009, JAS) and Eq. (7) in Sardina et al. (2015, PRL). We made the following revision to note this point:

   - Line 139–142: A paragraph "*Note that it ... for future work.*" has been inserted.

   In the present study, however, we focus on statistical properties of the the second version with Eqs. (18) and (19) and leave this extension for future work.

4. *L132-136: Are the drift coefficients introduced just to scale the magnitude of the supersaturation fluctuations to a correct value, or are there any other physical reasons? More explanation would be helpful for the readers.*

   There are no other physical reasons. Two parameters c_1 and c_2 are introduced just to scale the magnitude of S' to that of the reference data. To emphasize this point, we made the following revision:

   - Line 181: A sentence "*Here, we do not ... as tuning parameters.*" has been inserted.

5. *A figure showing sample supersaturation trajectories from all three models (original, corrected, and simplified) could be informative (probably in the appendix section).*

   Thanks for the suggestion. We made the following revisions to show sample supersaturation trajectories from three models:

   - Page 12: Figure 5 has been inserted.

- Line 237–246: Two paragraphs "Figure 5 compares ... almost identical results." have been added.

6. *It would also be good to discuss the limitation of the current approach in representing the supersaturation fluctuation generation from scalar mixing (e.g., during the turbulent entrainment-mixing).*

   Thanks for the comment. As the referee pointed out, the turbulent entrainment-mixing is another important mechanism for the supersaturation fluctuation generation other than the stochastic condensation, and the effects of the turbulent entrainment-mixing are not included in the eddy-hopping model considered in the present study. We made the following revision to note this point:

   - Line 26–29: A paragraph "*It should be noted ... entraining parcel model.*" has been inserted.

   We also referred to the paper Abade et al. (2018, JAS), which investigated the effects of the turbulent entrainment-mixing and entrained CCN activation by using the entraining parcel model.

We again appreciate the referee's valuable comments which are very constructive to make the paper clearer and better.