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Comment on acp-2021-207

Anonymous Referee #2

Referee comment on "Statistical properties of a stochastic model of eddy hopping" by Izumi Saito et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-207-RC2, 2021

The submitted manuscript presents a study of the statistical properties of the stochastic model of eddy hopping. The Authors validated the model (in the statistically stationary state) against reference data from direct numerical simulations (DNS) and Large-eddy simulation (LES). However, the Authors claim that they have "corrected" the stochastic model of eddy hopping. I do not agree with this statement.

The stochastic model of eddy hopping was designed to mimic supersaturation fluctuations at unresolved scales in LES Lagrangian cloud models. The model has been presented in two articles cited in the References: Grabowski and Abade (2017) (GA17 hereafter) and Abade
et al. (2018) (AGP18). GA17 presented the first version the model, which is valid in the regime of large Damkholer numbers (or fast microphysics). For fixed microphysical characteristics (such as the droplet integral radius), the Damkholer number depends on the resolution length scale "L" of the LES. Typical values for "L" used in cloud-scale flow simulations range from several meters to several tens
of meters, then yielding a Damkholer number of at least O(10). In this regime, the GA17 is a useful model.

AGP18 extends the eddy hopping model by introducing a term accounting for the relaxation of supersaturation fluctuations due to turbulent mixing. The extended model by AGP18 mimics supersaturation fluctuations in the whole range of Damkholer numbers, from fast to slow microphysics. However, the Authors of the submitted manuscript claim to have corrected the eddy hopping model of GA17 by including a relaxation term that have been already included and studied by AGP18.

The Authors claim to have introduced a second correction of the model by introducing tuning parameters (the factors c_1 and c_2
multiplying the two time-scales involved: the phase relaxation time and the turbulent integral time). In GA17 and AGP18 the constants c_1

and c_2 are assumed to be of order unity and the results reported in the submitted manuscript confirms this assumption. Thus, the introduction of tuning parameters to fit the model to reference data cannot be regarded as a "correction" to the model (particularly when those fitting parameters turn out to be O(1)), unless theoretical expressions for the parameters and a rationale are provided.

The phase relaxation time encapsulates intricate interactions among growing droplets by condensation. Also, the Lagrangian integral time-scale requires *a priori* knowledge of the Lagrangian correlation function, which we do not possess. Thus, a better theoretical parametrization of the two time-scales involved is a difficult task and this task has not been tackled in the submitted study.

Finally, the Authors suggest a simplification of the model that eliminates the vertical velocity fluctuations w' from the list of stochastic variables attached to computational particles (superdroplets) in LES Lagrangian cloud models. The discussion and procedure of model simplification is instructive and gives valuable insights into the model. However, this simplification does not necessarily imply in reduction of computational cost. This is because the velocity fluctuations of superdroplets are usually necessary to resolve the subgrid-scale transport of superdroplets. Also, the simplified model accurately reproduces time correlations in the Lagrangian supersaturation in the regime of large Damkholer numbers. This is exactly the regime of applicability of the GA17 model.

Overall, the submitted manuscript is instructive and gives valuable insights into the statistical properties of the eddy hopping model. However, the Authors should state differently their contribution to the field.