Reviewer #1 (Formal Review for Author (shown to authors)):

This paper introduces a direct numerical simulation model for study of cloud entrainment. In general the authors have a rather small simulation with 256^3 gridpoints which can even run on a small home computer. They do the mixing diagram analysis which was done in the past in papers by Andrejczuk et al and Kumar et al, but I lack to find something completely new. The authors claim that they use a different method with finite differences instead of pseudospectral method with the argument that they define sharper boundaries than in Kumar et. al and more "realistic boundary conditions", but that is not pursued or analyzed in any detail, so is not relevant to this paper. In a recent paper by Gotzfried et al (J Fluid Mech. Vol 814, 452-483, 2017) the boundaries were even sharper without significant notice of Gibbs oscillations or errors. Overall the authors should clarify more the benefits of using their method to previous works, since it is not clearly visible in the draft.

The original aspect of the paper is found in sections 5.2-5.4, so this should be further developed. Most of what appears before that is easily cited as repetition of Andrejczuk et al and Kumar et al studies. The further development should be in several places. First, although Equations 18a-21 are introduced in other papers, it is more self-contained if they are given some interpretation here. For example, \beta is an angle in the mixing diagram, etc. Second, more investigation of the alternate Damkoehler number based on phase relaxation time should be given. On lines 434-436 it is stated that similar conclusions are reached, but this is likely because the simulations only include large droplets where the two time scales are similar. A wider range of conditions, including cases with smaller droplets where complete evaporation occurs, should be included for completeness. The power laws identified in section 5.4 are intriguing, and I suggest that expressions be provided.

**l137 What are the boundary conditions in contrast to periodic boundary conditions?**

A periodic boundary condition is used for the current work. However, the finite difference method gives a flexibility and possibility of change to other boundary conditions. We would like postpone this topic to our future research.

**l153: kinetic viscosity -> kinematic viscosity**

The author appreciates the reviewer’s comment. It is a typo and has been revised.

**l158-l160: The authors use finite difference method, how do they excite forcing in Fourier space?**

Similar as the velocity field, the force field still has a one-to-one mapping between the physical space and the Fourier space. The velocity field in physical space is firstly transformed to Fourier space, and then the Fourier space forcing can be computed using formula (3). This force is then transformed back to physical space and added to the incompressible Naiver-Stokes equation (Eq. 1a, 1b) as a forcing term. The Fourier transformation and its inverse is implemented in an external library FFTW3.

**l204: why bilinear interpolation? Trilinear interpolation is always needed.**

The author appreciates the reviewer’s comments. It is a typo and has been revised.

**l212: What is the typical Kolmogorov scale? Is the grid resolution enough to resolve the Kolmogorov scale ( in pseudospectral it was k\_max \* eta\_k >= 1.5)**

The initial Kolmogorov scale is around 1mm and the current resolution is 0.512m/256 = 2mm. However, due to a quick decaying of the turbulence in the initial period and the cloud droplets are released after this period, the typical Kolmogorov length scale is much larger than 2mm, which can be resolved in the current computational grid. This is true for both forced and decaying cases.

**l236: 'discrepancies' should probably be 'differences'**

The author appreciates the reviewer’s comments. This word has been replaced.

**l265: This transient enhancement results "likely" from the buoyancy effect -> be more specific**

According to Eq. 2, the buoyancy plays as a role of forcing term to the Navier-Stokes equation, and the magnitude of this force is determined by the difference between the spot value (e.g. temperature and vapor mixing ratio) and the volume mean value. During the simulation, the difference is large in the early stage and then decreases due to the diffusion process.

**l350: I believe the mixing diagram was first introduced by Jensen et al (J Atmos Sci vol 42, 173-192, 1985), so it should be cited here.**

The corresponding reference has been added.

**Section 5.2 In two places it is stated that extremely homogeneous mixing is denoted by a vertical line. Please explain why this is so. My understanding is that homogeneous limit should be the dashed line shown in Figure 6.**

The homogeneous mixing trajectory in Figure 6 records the instantaneous relationship between number density and volume mean radius. If the observation box is fixed in an extremely homogeneous mixing, the number density would not change while the mean radius would decrease, since all the droplets are exposed to the same environment. This corresponds to a vertical line in the figure. However, if we consider two adjacent states and use one state as the start point, the other state should lies on the dashed line in an extremely homogeneous mixing. This is because given the information of the cloudy air, clear air and cloud droplets in the observation box, and in the assumption of homogeneous mixing and water mass conservation, we are able to predict the new state in a short time period. This is also how the homogeneous mixing line calculated.

**l522: Effect of turbulent intensity influence already investigated (see D. Tordella et. al, Gerashenko 2012, Götzfried et. al 2017) affected by large scale properties of the flow.**

The author appreciates the reviewer’s comment. We didn’t notice this recent publish and the sentence about turbulent intensity in the conclusion part has been removed.

**Figure 1: Not relevant to main points of the paper, should be deleted.**

This author agrees with the reviewer’s comment and the figure has been removed.

**Figure 2: Main difference in the initial conditions is in the initial length scale of the inhomogeneity (trivial) and the existence of correlation or lack of correlation of the humidity and velocity fields (more interesting). The latter should be discussed in interpreting the differences. Note also that the caption should state that the top row if for RH and the bottom row is for temperature.**

Not understand?

Caption will be added

**Figure4: There is a jump in droplet radius distribution for the upper right figure. Can the authors explain it?**

Not understand?

**Figure 5: It looks like a and b have been switched?**

This has been revised.

**Figure 6: Label individual panels by corresponding simulation scenario (e.g., D1, D2, etc) to help with interpretation.**

Figure will be replaced

**Figure 10: It is unclear how this figure is significant. Suggest deleting or developing further to explain relevance to main conclusions.**

Remove?

Reviewer #2 (Formal Review for Author (shown to authors)):

This paper introduces a new DNS model for cloud mixing at small scales. The underlying equations are the same as in Kumar, Schumacher and Shaw (2014). Both models differ in the numerical implementation. While Kumar et al. use a spectral method for the dynamic equations; the new model employs a fraction step algorithm. This allows for resolving very sharp interfaces and for using non-periodic boundary conditions.

The authors present simulations for three different initial mixing scenarios for decaying and forced turbulence. It is really interesting to see how the mixing diagram N-R^3 changes for the different scenarios, even when the thermodynamic and turbulence properties are the same. The authors introduce then different measurements for how homogeneous/inhomogeneus is the mixing, and present different scalings as function of the Dammkohler and the transition scale number.

The new model and results introduced in this paper are interesting, but I think that there are still some major points that need clarification (see below). For this reason I recommend major revisions.

Major comments

**1) Since this paper introduces the new model, it would be useful to provide more detailed information about the numerics. For example, which is the overall accuracy of the model? How is the pressure/continuity equation solved? Is it easier to parallelize than the spectral model? (I guess so, since spectral models require continuous transformations between the spectral and physical spaces) It would be also very useful to know which discretization is roughly needed to reach the same accuracy of a parallel model for some statistics (like turbulent moments). This information could help readers to choose which model to use, and to evaluate the quality of the results.**

The numerical implementation basically follows “Brown, Cortez and Minion, Accurate projection methods for the incompressible Navier-Stokes Equation”. This method is second order accurate and uses projection algorithm to decouple the pressure and velocity field. The parallelization is designed using domain decomposition and buffer extension. This simplifies the parallelization since the subdomains are relatively independent and the exchange of buffers only needs after solving the equation. For the computation of statistics, a central difference discretization was used to keep the second order accuracy. Need to add to paper?

2) **The authors modify the measures psi2.. psi5 to consider adjacent states (temporally and locally), instead of using the reference values (as in Lu et al). I do not understand the reasoning behind this choice, which is crucial for most of the paper results. Could you better explain which mixing scenario do the authors have in mind? How far are the states j and j-1 in time? Maybe some example mixing diagrams like Fig. 1 in Lu et al. [2013] would be helpful.**

The idea is similar as in Lu et al. 2013, in which the reference state is fully cloudy. In current work, neither of the two adjacent states are fully cloudy and may contain environmental air. Therefore, the formula needs to be modified to represent the cloudy air, environmental air and cloud droplets in these two adjacent states. The interval for data gathering is preset to 0.5s, such that a total of 60 time frames are gathered during the simulation.

**I am often confused how to apply concepts from inhomogeneous-homogeneous mixing to DNS. The way I understand the homogeneous and inhomogeneous mixing lines, and the measures psi2,...,psi5 from Lu et al., is that they apply to thermodynamical equilibrium after mixing two reference parcels of cloud and dry air. They represent the final stage of cloud parcels after a mixing event (entrainment). However, DNS at the time and length scales affordable nowadays are rarely at equilibrium, and therefore the diagrams and measures cannot be interpreted in the same way. I would really like to know what the authors think of this difficult subject.**

The mixing of two parcels are still considered here, while the two parcels are neither completely dry nor cloudy in our case. Therefore, formula needs to be modified to represent these three parts in order to apply the thermodynamically equilibrium.

**3) Naively, the conclusions seem contradictory. On one hand, different mixing diagrams and DSD are obtained in simulations with the same turbulence and thermodynamical conditions, just by changing the initial cloud configuration. On the other hand, a universal scaling law that describes all simulations is found. What am I missing? How this law be of practical use (line 518) if mixing depends on the filament orientation?**

The main goal of this work is to find a universal law or parameterization for different mixing scenario. The thermodynamics, such as mean radius, vapor mixing ratio and temperature, would depends on the mixing settings, while the parameterization is expected to hold for all the cases.

Further comments

**1) I am curious. Do you think that solving very sharp interfaces can change the mixing behavior? Have you done some tests?**

We believe the sharp interface would not affect the mixing behavior. This is demonstrated by our consistent results with Kumar’s work. However, we would like to eliminate the influence of interface settings by taking advantage of the numerical method.

**2) I do not fully understand the initial droplet configuration described in page 11. Do you put droplets only in cloudy areas for scenario 1? This seems inconsistent with Fig. 6, where n never reaches 1. For the forced turbulence, do you initiate the thermodynamic fields when you release the droplets (at t=5 s) or at t=0s?**

The x-axis in Fig.6 is defined as the ratio of the number density in sample box and the number density in cloudy region. Due to our settings in scenario 1, the sample box is not fully cloudy, and therefore the ratio cannot reach 1. The thermodynamics fields are also released after the spin-up period.

**3) In all configurations the cloudy air is supersaturated by 2%. This leads to an initial condensation of the droplets (at least for the cases 2 and 3), with subsequent increase of ql and temperature. Also some droplets should be larger than 15 mu m, during the initial mixing stages. It is difficult to see, but the initial plateaus in Fig. 3 and 5 and the DSD in Fig. 4 seem to indicate otherwise.**

The author agrees with the reviewer.

**4) Related to the previous question. Can you explain the initial plateaus in Fig. 3 and 5 for the forced cases when compared with the non-forced ones? Why initial evaporation is faster in the decaying case?**

The evaporation speed roughly depends on the local supersaturation of the droplet. In decaying case, the mixing is slower and droplets have a high possibility to reach the low humidity region. However, in the forced case, the droplets are more likely to enter a well-mixed region, and hence has a slower evaporation.

**5) The reference to de Lozar and Mellado (2013) is wrong. You mean de Lozar, A. and Mellado, J. P.: Cloud droplets in a bulk formulation and its application to buoyancy reversal instability, Q. J. Roy. Metor. Soc., 140, 1493-1504, doi:10.1002/qj.2234, 2014.**

This has been revised.

**6) You could consider discussing the DNS results of Kumar, B., S. Bera, T. V. Prabha, and W. W. Grabowski (2017), Cloud-edge mixing: Direct numerical simulation and observations in Indian Monsoon clouds, J. Adv. Model. Earth Syst., 9.**

Need to add a discussion?