REPLY TO REVIEWER #3:

We thank the reviewer for the comments that helped us to improve the manuscript.

1. The starting point of the paper is the introduction of two collection kernels. Fig. 1 shows how the Reynolds number affects the change of kernel in each case. For most regions, the Ayala-Wang kernel seems to have a more stronger Reynolds number effect. Is there any region in Fig. 1 f) showing a stronger dependence compared to Fig. 1 c). If so, can the reason be provided?

For the details of the collection kernels we refer to the accompanying paper by Onishi and Seifert (2016, ACP). The Onishi kernel shows a strong Reynolds number dependency for small regions in Fig. 1f, mostly along the diagonal, i.e., for droplets of similar size. This is due to the Reynolds number dependency of the optimal Stokes number for the preferential concentration effect. It is postulated that the optimum value for the preferential concentration shifts from St = 1 to slightly higher Stokes numbers for high Taylor-microscale Reynolds numbers. Due to the fact that the flanks of the radial distribution function g_{11}, which quantifies the preferential concentration effect, are quite steep a shift in g_{11} leads to a strong increase (or decrease) in a narrow range of drop sizes (Stokes numbers). This is basically what we see in Figure 1f.

2. The paper relies heavily on the contents in other papers including basic definitions. For example, the precise definitions of autoconversion, accretion, and selfcollection are not given. It would be useful to provide definitions of such.

Yes, this paper is intended for scientists who are familiar with the basic concepts of cloud physics, bulk microphysical parameterizations and turbulence effects on collision rates. It is hardly possible to review all those topics in a scientific paper. Nevertheless, we provide a short introduction of essential definitions and relations for particle-laden turbulence in section 2. For the basic ideas and definition of warm rain bulk microphysics we would like to refer to Klaus Beheng's review paper

Beheng, K. D.: The evolution of raindrop spectra: A review of basic microphysical essentials, Rainfall: State of the Science, Geophys. Monogr., 191, 2948, 2010.

but following the request of the reviewer we have extended the introduction of the bulk microphysics scheme at the beginning of section 3, which now provides a short introduction to bulk microphysics parameterizations.

3. Furthermore, regarding the enhancement of accretion and self-collision krr, I assume this factor is used in determining the mean size of rain drops. Can an equation like Eq. (8) be provided to show how krr is actually in- corporated in the moment methods.

In the revised version of the manuscript these equations are explicitly given in section 4. This actually helped to fix some minor inconsistencies in the presentation.

4. One of the observations is that the Ayala-Wang kernel lead to faster autoconversion and Onishis leads to faster accretion. The faster autoconver- sion is due to stronger Re dependence. Can the reason for faster accretion for the Onishis kernel be provided? This could be discussed in terms of aspects related to the point 1 above.

This is discussed in detail in the accompanying paper by Onishi and Seifert (2016, ACP). As already explained in the answer to question 1, the main effect is the shift of the preferential concentration optimum to higher Stokes numbers in case of high Taylor-microscale Reynolds numbers. The larger Stokes numbers correspond to larger drops which belong to the raindrop category of the bulk scheme.

5. The study uses a single mass (2.6×10^{-10} kg or about 40 μm in radius) as the dividing size between cloud droplets and rain drops. I wonder how this choice affects the conclusions of the paper. Can the authors study other di- viding size such as 25 μm or 35 μm as the dividing size? This is important since a very rough moment method is used in the LES.

The threshold size is not arbitrarily chosen, but corresponds to the minimum of the bi-modal mass distribution function during the evolution of the drop size distribution (see e.g. Fig. 4 of Beheng's review paper). A small change like using 35 μm instead of 40 μm will not affect the results as this will only change the autoconversion rate by about 10 %. A reduction to 25 μm is inconsistent with the assumptions made in SB2001 and simply too small for the separating size of a two-category scheme. To explicitly predict the formation of such small drizzle drops the three-category scheme of Sant et al. (2013, J. Atmos. Sci) could be used instead. For shallow cumulus clouds this is not necessary, but it might be interesting for stratocumulus.

6. The formulation involves a shape parameter (Eq. 7). I assume A and B are related to Lc and xc . It is not clear if is kept as a constant during the LES simulation and how is determined. Can this be clarified?

Yes, the gamma shape parameter of the cloud droplet distribution ν is constant during an LES simulation. We mention this explicitly in the revised manuscript in section 5.1. The meaning of this constant ν in the SB2001 scheme is often misunderstood as it is actually only the shape parameter before coagulation kicks in. It would be possible to estimate a local time-dependent ν which is consistent with the assumption of the SB2001 model from the universal function $\Phi_{au}(\tau)$ or simply as a function of τ. Here τ is the non-dimensional internal time variable of the system, which describes the evolution of the cloud droplet distribution due to coagulation. The autoconversion rate, Eq. (8), is not simply the solution of the collision integral for a fixed ν, but includes the change in the drop size distribution and, hence, an evolving ν. On the other hand, the cloud droplet distribution is not strictly a gamma distribution during coagulation, and therefore estimating ν would provide only limited information (especially the tail of the distribution is much more important than the shape described by ν).

7. In the model equation (10), a single exponent p is used for the whole range of Re. In reality, the collection kernel (specifically the RDF) first increases with Re, then saturates or decreases slowly with Re. The question is then how valid a single exponent in representing the effect of flow Re.

Given the various approximations and uncertainties in the kernel, the bulk scheme and the LES model, and the sensitivity to grid resolution of the LES, we would argue that the use of a single exponent p to describe the Re-dependency is a minor problem. For a true reference simulation we would need an LES model that can predict ϵ and Re_λ independently. Having such a model we could then think about using a super-droplet approach to simulate the coagulation explicitly with the full Onishi kernel.

8. Another observation is that the Ayala-Wang kernel leads to shallow inversion height. However, in Wyszogrodzki et al. (2013) and Grabowski et al. (2015, Atmos. Chem. Phys., 15: 913-926) based on the spectral bin method, it is shown the dynamic effect of faster droplet growth is a deep cloud top. I wonder if these two are contradictory, and if the reason for this contradiction is due to their use of the moment method. Clearly, the strong sensitivity of the collision kernel with droplet size and shape of droplet size distribution requires a more accurate representation than the two-moment method. The authors should clarify the various errors associated with the moment method, and potential effect on the conclusions of the paper.

This is maybe related to the simulation of the BOMEX case (by Wyszogrodzki et al. 2013 and Grabowski et al. 2015) vs the RICO case that is used here and has been used in Seifert et al. (2010). In addition the domain used by Wyszogrodzki et al. (2013) is quite small with only 6.4 km in the horizontal compared to 51.2 km in the current study, and the simulated time period is only 6 h in Wyszogrodzki et al. (2013) compared to at least 30 h in the current study. For example, the small domain may be dominated by individual clouds which can lead to a different interpretation of the results. Maybe more important, the BOMEX case was initially set up as a non-precipitating case and the system is in equilibrium without the formation of precipitation (Siebesma et al. 2003, JAS). Hence, the formation rain leads to a perturbation of this quasi-equilibirium state and pushes the system into an instationary transient state. In contrast, RICO is not in equilibrium without rain as it was designed based on data from a rainy period. The RICO case approaches a quasi-equilibrium only due to the formation of rain late in the simulations. These differences in the model setup and the case design can lead to quite different behavior and different interpretations. It would be very interesting to do an intercomparison with both cases using spectral bin and bulk methods. Unfortunately, it is very expensive and time consuming to do large sensitivity studies with bin microphysics models. Comparing just a few simulations is very questionable due to the strong sensitivity and randomness of precipitating shallow convection, especially in the RICO case.

We have tried our best to convince the reader that the moments method provides a reasonable parameterization of the collision-coalescence process, e.g., with help of Figs. 3 and 4. A full quantification of the errors is beyond the scope of the paper. Nevertheless, we are confident that the results are qualitiatively meaningful and provide valuable insights in the behavior of precipitating shallow convection and the turbulence effect on rain formation. As suggested by the the analysis presented in section 5.4 the largest uncertainty of the model might actually be associated with the still too coarse resolution of the LES model.