Responses to the Reviewer 2.
(Comments in italics, our responses in regular font).

The manuscript presents results of large-eddy simulations of shallow convective clouds. The main aim of the study is to investigate the effect of enhanced collision rates in a turbulent flow on the rain formation in shallow cumulus clouds. Based on previous work of the authors the turbulence effect on the collision kernel is specified as a function of the dissipation rate and the root-mean-square velocity. A bin microphysics scheme is used to solve the kinetic equation for the collision-coalescence of the droplets. As their main result they show a strong increase in surface precipitation when including turbulence effects in the collision-coalescene process. Taking into account the turbulence effects does also modify the properties of the cloud field. The paper confirms the findings of Seifert et al. (2010) who applied a bulk microphysics scheme.

The paper is well written and makes an important and long-awaited contribution to our under- standing of the rain formation in warm shallow clouds. I recommend publication in ACP after a few major and some minor issues have been resolved.

We appreciate these nice comments, thank you.

Major comments:

1. *In section 3 the effects of the intermittency of the dissipation rate is discussed, i.e., the Reynolds number dependency of the turbulent enhancement of the collision kernel. This is important to bridge the gap between HDNS and LES. After integrating the collision kernel (which they found from HDNS at lower Re) over an assumed PDF of dissipation rate, the authors find that this effect is small. It is of course not surprising that the ratio R = <K>/K as defined in Eq. (8) is close to 1, because the kernel is approximately linear in ε and therefore the integration over a Gaussian PDF will result in a very small net effect. I am sorry, but I don't find this argument very convincing, because the ratio R = <K>/K is not necessarily the correct quantity to look at. By averaging over the kernel K instead of taking an average of the evolution of the drop size distribution itself, the nonlinear behavior of droplet growth is ignored. For example, some 'lucky' droplets may spend more time at locations with high dissipation rates where they grow to bigger sizes which later on makes them grow more efficiently because K is nonlinear in drop size. As the authors know, it is this interaction of Lagrangian particle dynamics with the turbulent flow which makes the whole problem so challenging. By averaging over K instead of using the full kinetic equation in the analysis the history of the particles is ignored and this may lead to a biased or even wrong result.*

We strongly disagree with the above comments. The enhancement is not linear in the TKE dissipation rate – this is evident from (now dropped) appendix and from our previous publications, e,g, Ayala et al. (2008a,b, NJP), Xue et al (2008, JAS), Wang & Grabowski (2009, ASL) , Grabowski et al (2009, ACP). This is also evident from the impact for high dissipation rate cases where the impact is appreciable.

Having said that, one needs to keep in mind that the turbulence (in addition to droplet sedimentation) mixes droplets from different sub-volumes of the LES gridbox and this strongly reduces the effect the reviewer refers to. In particular, the mixing happens rapidly for high LES-mean dissipation rates (where the intermittency effects seem to be significant according to our analysis), so we do not think the effect the reviewer has in mind is that important. There is also yet another effect that needs to be considered, the non-steadiness of the turbulence field. All these are now discussed in the revised text.

2. *In the abstract and in more detail in section 5.1 the 'dynamical enhancement' is introduced, i.e., the fact that clouds get more buoyant when liquid water is removed by precipitation. The simple simulations of section 5.1 are very helpful to understand the importance of this argument. Unfortunately, this is later only used in a very qualitative sense to explain the behavior of the LES (page 9236, lines 15-20). Although this hypothesis is quite plausible, a quantitative analysis would be necessary. In my opinion this should not be left for a future investigation, but has to be discussed in the present paper. Even more so, because this is maybe the only result of the study which differs from the earlier paper of Seifert et al. (2010). In that study it was found that the inversion height decreased with increasing precipitation, i.e., when taking into account turbulence effects on the collision rate. I would ask the authors to calculate cloud top height and inversion height from their simulations and discuss this in detail. Having the 3d snapshots available with 5 min interval (page 9228, line 29) it should not be too difficult to do this analysis.*

This comment follows the comment 2 from the Reviewer 1. However, the reference to Seifert et al. is not appropriate in this context. First, Seifert et al. used the RICO case in which the cloud field deepens as simulations progress. This complicates the analysis because the different rate of deepening occurs in gravitational and turbulent cases (most likely because of the differences between removal of condensed water near the cloud top depending on the precipitation rate as argued in their other papers). We purposely used the BOMEX case where the cloud field depth is similar in various simulations, so are the mean thermodynamic profiles (the latter is now mentioned in the revised paper). As stated in responses to the Reviewer 1, we added a figure showing the pdfs of the cloud top height and its discussion.

Minor comments:

1. *page 9219, line 5: Please give some references for the fact that 'rapid onset of rain [..] is often hard to explain applying classical droplet growth theory'.*

We added a reference to a recent JAS paper by Cooper et al. that mentions this in the introduction.

2. *page 9219, line 7-9: Please give some references for the statements regarding giant CCN and the broadening of the drop size distribution by entrainment.*

See the previous point.

3. *page 9228: I did not find any information about the domain size or number of grid points of the simulations. In Fig. 4 this information is missing, too.*

We added this information to the text and figure.

4. *page 9229: Please mention explicitly that the model carries an additional prognostic variable, N_{act}, to properly represent drop activation. Without this additional prognostic equation Eq. (9) would provide an unlimited source of CCN.*

We added this information to the text.

5. *pages 9225 and 9230: What is the difference between u' on page 9225 and u_{rms} on page 9230?*

We replaced u_{rms} with u' throughout the text.

6. *page 9230, line 12: For readers who are not so familiar with turbulence theory, it should be mentioned that the Kolmogorov velocity is proportional to $\varepsilon^{1/4}$.*

Added.

7. *page 9230, line 12: Where does the assumption $\varepsilon \sim u^3_{rms}$ come from? What are the limitations of this assumption?*

This comes from Kolmogorov' equilibrium assumption for turbulence at high flow Reynolds number, namely, the energy input at large scale is balanced by the TKE dissipation at the small scales. Dimensional analysis then leads to $\varepsilon \sim u^3_{rms} /L_f$ where L_f is the integral length scale. We added a reference to Pope (2000) book.

8. *page 9230, line 13: How did you select the value of 2.02 ms^{-1} for u_{rms}?*

   This is based on published observations at different flow dissipation rates given in Table 1 of MacPherson and Isaac (1977, J Appl Meteor, 16, 81-90), as discussed in Wang et al. (2006a), referenced in the text.

9. *page 9230, Eq. (12): This equation is a mix of mks and cgs units. Please write this as*

   $$u_{rms} = u_{rms,ref} (\varepsilon/\varepsilon_{ref})^{1/3} \text{ with reference values } u_{rms,ref} = 2.02 \text{ m s}^{-1}$$
   $$\text{and } \varepsilon_{ref} = 400 \text{ cm}^2/s^{-3}.$$

   We do not think this creates a problem. Units of u and epsilon are clearly stated in the text.

10. *page 9230, Eq. (12): This equation is similar to - or consistent with - the assumption of Seifert et al. (2010) who use $Re_\lambda \sim \varepsilon^{1/6}$. Please say so.*

    These formulas leading to $Re_\lambda \sim \varepsilon^{1/6}$ come from fundamental turbulence theory and perhaps a reference to Pope (2000) would be more appropriate. Since this book is referred to just prior to (11), we added a brief sentence emphasizing that $Re_\lambda \sim \varepsilon^{1/6}$ without a reference.

11. *page 9232, line 15: 'the thermal with turbulent kernel rises ...'. Does a thermal have a kernel? It the kernel actually turbulent or is it maybe only the flow which is turbulent? Here and elsewhere, the authors should be a bit more careful with their terminology, sometimes they use a jargon which makes little physical sense. A 'turbulent flow' is correct and we are used to 'turbulent fluxes', but I am not sure that 'turbulent kernel' is good and precise terminology, 'turbulent profiles' (page 9235, line 26) is definitely not.*

    We made some changes to the text. We believe "turbulent kernel" is an appropriate term and we used it before. We modified "turbulent profiles" to "profiles for the turbulent kernel" or similar in various places throughout the text.

12. *page 9233, line 5 and Figure 6: I am not sure that most readers do readily understand what a CFAD is. In my opinion it is most easily explained as the one-dimensional PDF of some quantity as a function of height z or, alternatively, the conditional probability function P(x|z) of some quantity x.*

Personally I prefer the first formulation, because in the model and also for most measurements the height z is not a random variable.

We added an explanation at the first time CFAD is used in the text.

13. *page 9232, line 17: maybe 'which is more effective' instead of just 'more effective'.*

Changed as suggested.

14. *page 9233, line 7: To calculate the adiabatic fraction you calculate an adiabatic ascent from cloud base in the same column of the LES? Or do you use a Lagrangian trajectory?*

Adiabatic cloud water is estimated assuming the mean cloud base height from the simulation and the initial sounding. We think this introduces insignificant errors for mostly qualitative discussion of this figure. We added this information to the text.

15. *page 9233, line 18: 'see their Fig. 6' instead of 'see Fig. 6'.*

Added.

16. *page 9234, line 4 and Fig. 8: The effective radius is calculated over the whole drop size distribution, i.e., it does include the drizzle and rain drops?*

The effective radius is calculated using the entire spectrum. Otherwise the value of the effective radius would depend on the specific threshold used to separate cloud droplets from raindrops. This is now stated explicitly in the text.

17. *page 9234, line 11: 'the CFAD' instead of 'CFAD'*

We are not sure. We leave it for the technical editor to decide if "the" is needed.

18. *page 9234, line 16-18: Does the sentence starting with 'For simulations with lower CCN..' refer to a Figure which is not shown in the paper? I do not understand this sentence.*

This sentence indeed refers to the figures not shown in the paper. The sentence was revised to point this out.

19. *page 9235, line 16: It may be confusing for some readers that the precipitation rate is given units of m/s instead of mm/h or kg m^{-2} s^{-1}. The threshold value of van Zanten et al. (2010) is actually the flux of the rain water mixing ratio. I would recommend to either state this more explicitly or to give the corresponding value of the rain rate in mm/h (i.e. the mass flux of*

raindrops).

The units for "precipitation flux" in van Zanten et al. are kg kg^{-1} m s^{-1} (see table 4 therein). This is exactly m s^{-1} that we had. To avoid the confusion, we changed the units to have it exactly as in van Zanten et al. We also changed "rate" to "flux".

20. *page 9236, line 6: Why do you call this precipitation water path (PWP) instead of rain water path (RWP)? Even when it includes drizzle I would prefer RWP. The authors also use 'rain rate' and I assume that includes drizzle, too.*

    This is to distinguish rain from drizzle plus rain. We left PWP, but change "rain rate" into "drizzle/rain rate" throughout the text.

21. *page 9236, line 17: Without further analysis the dynamical enhancement is only a hypothesis and may not be the only possible explanation.*

    The added figure 14 clearly demonstrates the dynamical enhancement. Additional analysis will be shown in a separate paper.

22. *page 9239, line 6: maybe 'in-cloud turbulence' instead of 'cloud turbulence'*

    We do not agree. We use "cloud turbulence" throughout the paper and never use "in-cloud turbulence". Why use it here?

23. *pages 9241-9244: An appendix summarizing the turbulence effects on the collection kernel is useful, but I would recommend to structure it more like a fortran subroutine, i.e., start with the known input quantities (e.g., ε and u_{rms}) and give step by step the necessary equations which in the end lead to the collection kernel.*

    The appendix has been removed per suggestion from the Reviewer 1.

24. *Figs. 13 and 14: Units should be given at the y-axis, not only in the caption. What is actually the difference between the accumulated precipitation and the cumulative precipitation flux?*

    These are now Figs. 14 and 15. Can you have accumulated precipitation at the cloud base? Since this is an imaginary horizontal plane (as opposed to the surface), we think using the term cloud-base accumulated precipitation is not appropriate.

25. *Caption of Fig. 13: typo 'clod base'*

    Corrected.