

Review of

Turbulent collision-coalescence in maritime shallow convection

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General comments:

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-coalescence 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 understanding of the rain formation in warm shallow clouds. I recommend publication in ACP after a few major and some minor issues have been resolved.

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 $\mathcal{R} = \langle K \rangle / K$ 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 $\mathcal{R} = \langle K \rangle / K$ 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.
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.

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'.
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.
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.
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.
5. pages 9225 and 9230: What is the difference between u' on page 9225 and u_{rms} on page 9230?
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 $\epsilon^{1/4}$.
7. page 9230, line 12: Where does the assumption $\epsilon \sim u_{rms}^3$ come from? What are the limitations of this assumption?
8. page 9230, line 13: How did you select the value of 2.02 m s^{-1} for u_{rms} ?
9. page 9230, Eq. (12): This equation is a mix of mks and cgs units. Please write this as $u_{rms} = u_{rms,ref}(\epsilon/\epsilon_{ref})^{1/3}$ with reference values $u_{rms,ref} = 2.02 \text{ m s}^{-1}$ and $\epsilon_{ref} = 400 \text{ cm}^2/\text{s}^{-3}$.
10. page 9230, Eq. (12): This equation is similar to - or consistent with - the assumption of Seifert et al. (2010) who use $Re_\lambda \sim \epsilon^{1/6}$. Please say so.
11. page 9232, line 15: 'the thermal with turbulent kernel rises ...'. Does a thermal have a kernel? Is 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.

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.
13. page 9232, line 17: maybe 'which is more effective' instead of just 'more effective'.
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?
15. page 9233, line 18: 'see their Fig. 6' instead of 'see Fig. 6'.
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?
17. page 9234, line 11: 'the CFAD' instead of 'CFAD'
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.
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 $\text{kg m}^{-2} \text{s}^{-1}$. 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).
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.
21. page 9236, line 17: Without further analysis the dynamical enhancement is only a hypothesis and may not be the only possible explanation.
22. page 9239, line 6: maybe 'in-cloud turbulence' instead of 'cloud turbulence'
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.
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?
25. Caption of Fig. 13: typo 'clod base'