

Responses to the Reviewer 1.

(Comments in italics, our responses in regular font).

*This paper uses a LES model with bin microphysics to investigate the effects of turbulent collision-coalescence under a range of CCN concentrations. A highly simplified experiment using a warm bubble is first used to illustrate that, as well as there being a microphysical enhancement to the formation of rain that occurs when turbulent collision kernels are used, there is also what appears to be a dynamical enhancement due to the suspected unloading of water from updrafts. The BOMEX case study of shallow convection is used to examine these enhancements and it is shown that in the high CCN experiments the effects of turbulent collision-coalescence is to increase drizzle within clouds. In the low CCN experiments there is a significant increase in surface rainfall in the turbulent coalescence cases and no impact on the cloud water contents. It is suggested that this is due to a dynamical enhancement. An interesting result shown in the paper is that the effect of subgrid scale TKE dissipation intermittency due to the different resolutions of DNS and LES is roughly negligible. This is an important result that means using mean TKE dissipation rates from LES grid boxes to calculate the turbulent collision kernels that are derived from much higher resolution DNS dissipation rates is an appropriate strategy, at least given our current knowledge and numerical capabilities. The paper is generally well written and contains new results that should be of interest to the general cloud modeling community. The discussion of results includes a critical assessment of the limitations of the methodology applied and future directions for numerically investigating turbulent collision-coalescence. I recommend publication of the paper after the comments below are addressed.*

We appreciate the Reviewer's positive comments on our manuscript. The section describing the scale separation between DNS and LES simulations is now expanded per comments from the Reviewer 2.

*Comments:*

*1. Have you investigated the sensitivity of your results to the cloud/rain threshold radius? The value you use of 25 microns is small, was this chosen to maximize the rain water since this case is a non-precipitating case? I wonder whether this could be a reason why you do not produce larger cloud water contents for the turbulent collision-coalescence cases.*

We think the Reviewer is confused here. The 25 microns threshold is only used to partition the droplet/drop spectrum into cloud water and drizzle/rain. It does not affect the physics of the problem (i.e., the autoconversion), only the interpretation of results. The revised manuscript mentions this explicitly.

*2. To support your argument for a dynamical enhancement that comes from using turbulent collision kernels, it would be useful to provide some evidence in the form of a figure that shows something even as simple as vertical velocity statistics from your simulations.*

We added a figure that shows pdfs of the cloud-top height and added its discussion.

*3. Some discussion is warranted on the reasons why you simulate deeper clouds through some suspected dynamical effects compared to the results of Seifert et al. (2010) who found small reductions in the height of the inversion, which as they discuss is consistent with the findings that more precipitation leads to a shallower boundary layer.*

There is a significant difference between the model setup used in Seifert et al. and in our study. Seifert et al. used the RICO setup in which the cloud field deepens during the course of the simulation, and differences in the precipitation lead to the differences in the evolution of the cloud field depth. This is a significant complication because additional feedbacks are involved. The BOMEX setup dictates the cloud field depth in the nonprecipitating case, and the mean depth does not change significantly between the simulations when precipitation is allowed. We explicitly mention this aspect in the revised paper.

*4. The abstract states that this paper is focused on a quantitative assessment of the effects of turbulence on rain formation and in the introduction on page 9221 it says that the analysis of results in this paper will “unambiguously” evaluate the effects of turbulence. However, the conclusions then state that the results presented “have to be considered as just an initial step” towards quantifying turbulence effects on rain development and describe reasons why a quantitative assessment is very challenging. I agree with the conclusions and suggest that you tone down the previous descriptions in the paper that claim this is a quantitative study.*

We agree with this inconsistency between our goals and conclusions. The text in the conclusion section was modified accordingly.

*5. The appendix should be removed as it does not contain anything that is not repeated in at least 2 other papers. The results presented in Appendix A appear exactly in the same form as the original paper by Ayala et al. (2008b), as well as other papers by this group such as Xue et al. (2008).*

The appendix was removed.

*6. Page 9228: What does “almost converged” mean?*

Changed to “close-to-converged”.

*7. Page 9228: Details of the domain size should be given here and included on Figure 4.*

Done.

*8. Page 9230, lines immediately after (12): Please provide numbers to define small and larger droplets.*

Added.

*9. Page 9240: Can you provide some ideas as to how one should go about using remote sensing observations to validate the modeled effects of turbulence on rain formation?*

We have some ideas on how this can be tried and already initiated a collaborative project with the CloudSat group at JPL. We decided not to dwell on this in the paper, however.

*10. Discussion of Figures 9 and 10 should be expanded to include some description of the cloud water content being similar between the simulations with and without turbulent collisions and the effect of turbulence on rain water appearing to be larger for low CCN as compared to high CCN.*

The discussion of the two figures has been expanded as suggested.

*11. There is no need to include both Figures 13 and 14, just show one of these.*

We do not agree. Including the log plot in addition to the linear plot shows enhancements for the low precipitation cases. From the log plots, on the other hand, it is difficult to deduce the precipitation enhancement when turbulent kernel is used. We point this out in the revised paper.