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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The effects of turbulent collision-coalescence on precipitation formation and precipitation-dynamical feedbacks in simulations of stratocumulus and shallow cumulus convection" *by* C. N. Franklin

Anonymous Referee #1

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Review of "The effects of turbulent collision-coalescence on precipitation formation and precipitation-dynamical feedbacks in simulations of stratocumulus and shallow cumulus convection" by C. N. Franklin.

Recommendation: accept after major revisions

This paper reports numerical simulations documenting the impact of the turbulent collision kernel on fields of warm shallow convective and stratiform clouds. This paper provides a useful contribution to a growing field of studies concerning the impact of

cloud turbulence of precipitation processes, but I feel there are aspects of the presentation that need to be revised before the paper is accepted.

Major issues.

1. Limitations of the double-moment scheme used need to be better exposed. First, the scheme assumes that the concentration of cloud droplets is constant in time and space. There are other schemes that do not make this assumption (e.g., the Morrison/Grabowski scheme, JAS 2007, 2008). In fact, simulations with such a scheme show that droplet concentration varies significantly within shallow convective clouds and activation above the cloud base is critical (Slawinska et al. JAS 2012). The approximately-constant with height mean droplet concentration is a good assumption, but locally concentration does vary significantly and this likely significantly affects warm-rain development. Such effects are included in the bin microphysics model, so a word of caution with the bin model in mind would be appropriate.

2. Simulations reported in this paper apply gravitational collision efficiencies. There are some studies (theoretical from Khain and Pinsky, and DNS from Wang's group in Delaware) that provide some rationale to include turbulent enhancement of the collision efficiency. I am curious why such effects were not included into formulation of the collision kernel. This is a significant limitation of the current study and this needs to be better exposed in the manuscript.

3. I have significant problems with interpretation of model results. In a few places, the author suggests that turbulent collisions lead to the increased latent heating and higher cloud water, and suggest an explanation through the effects on the subgrid-scale TKE. I am not comfortable with such an explanation because it relies on a very uncertain part of the model physics. Wyszogrodzki et al. (ACP, 2013) argued that the simulated effects can be understood through a combination of microphysical and dynamical effects, both resolved by the model physics. The microphysical effect relates to enhanced conversion of cloud water into drizzle and rain. The dynamical effect is

related to the increased cloud buoyancy when drizzle/rain falls out from the cloudy volume. Simultaneous increase of the cloud water and the rain water can only come from the dynamic considerations because microphysical effect can only lead to the increase of the drizzle/rain at the expense of the cloud water. But I feel the enhancements (of both cloud and rain) come from resolved model dynamics, not from the subgrid-scale model. I would like more analysis to document such effects.

4. Are results shown in the paper statistically significant? The RICO case is difficult because the cloud field deepens and it is impossible to select conditions close to the quasi-equilibrium (this is shown in Fig. 4). Most of the figures should include some measure of the statistical significance, for instance, the standard deviation of the temporal variability. I expect that many differences shown in the paper are not statistically significant.

Specific points (not related to those above).

1. P. 2280, L.23: "liquid water equivalent potential temperature" does not make sense. I think the model uses liquid water potential temperature as a prognostic variable. However, this variable is not strictly conserved when precipitation processes are included (the equivalent potential temperature is), so a better explanation is needed here.

2. Fig 1 and 2. I suggest splitting these figures into 4 to enlarge panels and make them more readable. At the moment, some features discussed in the text are barely visible. I also suggest increasing labels on axes of multi-panel figures as the labels are impossible to read on a printed version of the paper.

3. P. 2283, L. 11: "6.6 km2" does not make sense.

4. I am a little concerned with the reluctance of the author to discuss the impact of applying different double-moment parameterizations in both RICO and DYCOMS cases. The fact that changing the parameterization has as large effect as moving from gravitational to turbulence-enhanced representation of warm-rain processes is disturbing and

arguably makes results presented in this paper questionable. This needs to be openly stated in the manuscript.

5. Figures 4 and 5. First, I found the way various simulations are labeled confusing. It is a minor point, but lower droplet concentration results in larger droplets, so I would prefer to see larger symbols corresponding to smaller droplet concentrations, and not the vice versa as it is now. Second, since the results come from time evolving simulations, some measure of robustness of these results should be included (for instance, by marking the temporal variability around each symbol, i.e., each dot should have a cross representing some measure of the spread of results). A very minor detail (but perhaps important to understand the results): is the liquid water the sum of cloud and drizzle/rain, or just the cloud water? If the latter, then "liquid" needs to be changed to "cloud" to avoid confusion.

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