Review of "Initiation of coalescence in a cumulus cloud: A beneficial influence of entrainment and mixing" by W. A. Cooper, S. G. Lasher-Trapp, and A. M. Blyth

I admit that I have struggled for many weeks thinking about the review for this manuscript, reading it several times, since I had thought that I was missing some aspects that the authors were considering, given that they are respected members of the cloud physics community and have contributed significantly to the field in the past. The very detailed description of the calculations presented made me think again about the topic of spectral broadening and I re-read several of the papers written 20 and even 30 years ago. I wanted to make sure to identify the aspects of the work in this manuscript that was new and different from those earlier studies. My conclusion is that conceptually not much is different, perhaps only the complexity of the tool used in this case, but the results seem to point to the very same conclusions.

The authors state that "*The central premise investigated in the present study is that entrainment and associated mixing in clouds broaden the droplet size distribution in ways that introduce some relatively large drops into regions with high liquid water content, and the resulting growth of those larger droplets accelerates the formation of raindrops.*" This same statement could have appeared in several of the earlier studies of droplet spectral broadening due to entrainment and mixing.

Figure 1c is supposed to show the range of initial locations for trajectories that end up at the same location within the cloud, and all it shows is that yes, they can originate at about 80m difference at cloud-base and end up at the same place. This variability in location (and associated perturbations in temperature and humidity and updraft) will lead to different integral super-saturations, as was originally postulated by the first author in 1989. I don't see the need for showing the 3 panels in Figure 1. On the other hand, Figures 2 and 3 are really nice and look very realistic, the software developed is outstanding, but that is not the point of this manuscript.

The very detailed calculations using hundreds of trajectories in this study seem to confirm what we already knew about the initiation of coalescence in small cumulus clouds from previous studies:

1. adiabatic ascent is not a good approximation for droplet growth
2. entrainment and mixing will lead to spectral broadening, the concept of inhomogeneous mixing introduced 30 years ago.
3. variability in cloud-base conditions (thermodynamic and updraft) will lead to perturbations in the integral super-saturations
4. activation of CCN at cloud-base is combined with CCN also activated during entrainment events.
5. total evaporation of droplets and residual CCN that can be re-activated after a new entrainment event, modifying the droplet spectrum.
6. in-cloud turbulence will lead to nearby droplets having experienced perturbations in thermodynamic conditions. (Note that turbulence was not included in these calculations, but the authors indicate in the last section that it will be considered in the future).

This list of conclusions from several previous papers contains the key conclusions listed in Section 5.5. I don't find a new contribution to our knowledge on initiation of coalescence from the results presented up to that point.

In Section 6 the authors proceed to test if the modeled droplet spectra can produce precipitation in the observed time (around 20 minutes). A continuous collection model is used rather than a stochastic approach. Given the effort shown in the previous sections to incorporate the effects of the different trajectories, it seems that neglecting stochastic collisions may lead to large errors in the estimation of time to precipitation.

Section 7 introduces a sensitivity study to several factors to determine their importance. The authors conclude that coalescence is important in producing precipitation embryos; I cannot think of anybody disputing this conclusion in warm cumuli, but is it a new finding? And then there is the issue of giant nuclei that has been around for many decades as one of the sources of precipitation embryos. Here again, the authors agree that giant nuclei are important for fast precipitation development and "*account for a majority of the rainfall that develops in the model*". The sensitivity to using ensembles of trajectories also seems to suggest a modest contribution compared to a single trajectory, particularly when giant nuclei are included. So it appears that perhaps the central premise of this study is not as important as suggested.

If this manuscript were to be accepted, I recommend a major revision to shorten it and concentrate only on the new aspects of the numerical model (currently listed in the Annex). This new tool can then be used to study some real clouds under more complex environmental conditions. A more in-depth comparison with several observed clouds may be attempted, because very little is presented in this study. Radar observations are mentioned from the SCMS project and perhaps radar pictures of the simulated clouds can be produced (using the software in Figs 2 and 3) and their evolution compared. This would help determine how much the evolution will depend on the inclusion of giant nuclei vs. the ensemble trajectory approach.