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Interactive comment on “Direct entrainment and detrainment rate distributions of individual shallow cumulus clouds in an LES” *by* J. T. Dawe and P. H. Austin

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Before we respond to the reviewer, we must inform him or her that during our revisions we discovered we had performed calculations on the ARM dataset using a grid spacing of 30 m (corresponding to an initial model run we had performed) instead of the actual grid spacing of 25 m. However, correcting this error does not substantially change our results; most of our correlations and relationships we find become stronger. The exception is the conclusion that detrainment D is proportional to cloud perimeter C in section 3.3, where the power law fit exponent has increased from 1.05 to 1.08.

In figs. 6 and 8, the authors show the mean values of log(epsilon) and log(d). What's the physical meaning of the mean of the logarithm of fractional entrainment and detrainment rates? In the text, these were referred to as the PDFs (or histograms) so I'm a little confused about what they are.

We plot the mean of the logarithm of the fractional entrainment and detrainment PDFs because the three dimensional PDFs we analyze in these sections cannot be properly represented in two dimensions. Since ϵ and δ have log-normal distributions $\log(\epsilon)$ and $\log(\delta)$ are normally distributed, and if we assume the PDFs of ϵ and δ remain log-normally distributed at all points in the variable space, half the $\log(\epsilon)$ values should be smaller than the mean of $\log(\epsilon)$ and half should be larger. By plotting the means of $\log(\epsilon)$ and $\log(\delta)$ we are attempting to show in an intuitive way the shifts in the ϵ and δ PDFs at different points in the variable space. Our text is indeed confusing on this point, and we have rewritten it in an attempt to make this clear.

In the discussion of the relationship between the circumference and area, are the differences between the correlation coefficients significant? You have got 0.928, 0.913 and 0.925. Can one use such small differences to reject one relationship in favor of the other?

Correction of our grid spacing error in the ARM LES increases the correlation coefficients to 0.950, 0.934, and 0.945, but the same question applies to the revised correlation values. The standard deviation of a correlation r calculated from n samples is approximately given by $\sigma = (1 - r^2)/\sqrt{n-1}$. Our dataset consists of approximately 10000 independent samples, making $\sigma \approx 0.001$ in all cases. 0.950 is thus significantly larger than 0.945. We have added the standard errors to the text.

In a couple places (beginning of section 4 and end of section 5), the authors stated that parameterizations should outperform the statistical results. It would be useful to elaborate on this, particularly because this analysis focuses on the mass exchange, which is not directly applicable to parameterizations as the authors have noted in the paper. Which aspects of the statistical results are the parameterizations supposed to outperform?

When we write of parameterizations outperforming our statistical results, we are primarily attempting to justify the lack of dimensional consistency in the relationships we find. Most parameterizations require dimensionally consistent results and by presenting fractional power law parameterizations we are not advocating this requirement be relaxed (though we are not particularly wedded to this requirement ourselves). Instead, we are arguing that parameterizations of mass entrainment and detrainment should at least show higher correlation and lower RMS error when compared with statistical power law fits to be considered valid. Because most parameterizations are tuned to calculate tracer exchange rates instead of mass fluxes we have avoided making these comparisons, as they might seem unfair. Ideally, we believe that future cloud parameterizations should calculate mass entrainment and detrainment rates and then modify these with parameterized cloud shell values to calculate entrainment rates for specific variables, under which conditions comparison with our statistical relationships would have more validity. We have added an explicit statement of this rationale to sections 4 and 5.

Pg 5385, line 7, “and” not “an”

Fixed.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 5365, 2013.