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Atmospheric Chemistry and Physics Discussions



# *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

**Anonymous Referee #1**

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This paper uses LES data to assess the distributions of entrainment and detrainment rates in shallow cumulus, on a cloud by cloud basis, and as a function several quantities that are potentially determinining the entrainment and/or detrainment. I find this a very strong paper, with as only possible issue that it sometimes can be rather tough and technical to read. On the other hand, the approach of the authors is a rigorous one, grounded in statistics, and that is precisely what makes this a solid paper. I frequently found myself writing down a request for the authors to do a bit more on a certain topic, only to find out that they already did it in the next section. I can recommend this paper to be accepted, but I would enjoy it if the authors could answer the following points/questions.

1. The authors report that the fractional entrainment rate is no function of the cloud area, but that means that the absolute entrainment E probably is. I would suspect that in designing a bulk parameterization based on this paper, the way through would be to find a prediction for E and D, integrate that over all the clouds, and derive a fractional bulk entrainment rate from that. Could the authors comment on their reason to focus on $\epsilon$ and $\delta$ anyway?

2. p5372, l22: If I understand correctly, the filter is on samples consisting of 16 grid cells in space and time, making it an 'area' that is 10 000 $m^2 min$, right?

3. Fig 2a: Are these profiles over all of the clouds, or only the ones with cores? Or: How many of these small, filtered clouds contain a core and would have contributed to the cloud core area in the end?

4. I find the number of 100 independent samples somewhat worrying. It certainly does not seem enough to be able to distinguish a power law from a log-normal distribution. Also, could the authors clarify a bit more why it is immediately clear from fig 3a that it is a power law?

5. Fig 3, and other figures: Given the extensive discussion on skewness, a demarcation of the mean in the lower graphs would help.

6. p5380, l1: I don't think that the Turner assumption is commonly believed. At best, we simply have nothing better, and this sounds like a plausible first guess while we were busy with some more pressing uncertainties.

7. p5375,l9: While I applaud the emphasis on the fact that a statistical link does not imply causality, the authors do actually a good job in the discussion relating the relationships they find to existing theories and parameterizations, some of which are physically based. It would not hurt to explicitly refer here to the discussion for some of the story telling.



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8. p5376, l8: $d\theta_\rho/dz$) has a larger MI than z.

9. How do your results on the fractional dimension of the clouds relate to Siebesma and Jonker (PRL, 2001)?

10. What are the margins of error in the E/D vs a/C relationships? This is especially necessary for the claim that D goes with C, and not with a.

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