Atmospheric
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Discussions

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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.

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 ϵ and δ anyway?

The reviewer is correct in suspecting E and D are (near-) linear functions of the cloud area. Our primary reason for focusing on ϵ and δ is that these are the standard quantities used in the cloud entrainment literature, as they are independent of cloud fraction. The scheme proposed by the reviewer would likely work, but would be significantly more complex than current cloud parameterizations, requiring, for example, knowledge of the cloud property and size distributions for the parameterized cloud fields.

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 10000 m^2 min, right?

No, the filter is instantaneous, removing any cloud samples with areas less than 10000 m^2. Because of this, most clouds have some part of them removed from the data set. We have modified the text to make this clearer.

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?

These profiles are only over clouds with cores. We have altered the caption to make this clearer.

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?

This is a fair criticism. We base this conclusion on the results of previous literature which finds that cloud areas follow a power law distribution, and have not applied explicitly statistical tests to the datasets to determine which distribution best represents the data–our inferences are based on visual inspection of the data. We should instead say that the results are consistent with a power law distribution, as they decrease monotonically with increasing size. We have altered the text to make these points clear.

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

We have altered the figures to include the mean values in the lower graphs.

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.

Fair enough, but we do not think we have implied the Turner assumption is commonly believed, simply that it is used as the basis of many entrainment parameterizations. We do state that it appears incorrect, but this does not necessarily imply that it was commonly believed to be valid.

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.

We have added such a reference to the text.

p5376, l8: dθ/dz has a larger MI than z.

Oops. We have corrected the text.

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

We would like to thank the reviewer for highlighting this highly relevant reference. Siebesma and Jonker find that cloud perimeter scales with linear size as $C = l^{1.32}$. Our results find that $C = a^{0.73} = l^{1.46}$. However, Seibesma and Jonker perform their calculation on a two-dimensional projection of the three-dimensional cloud field, in order to compare results with satellite observations. For entrainment, the more relevant relationship is between cloud area and perimeter of a cloud cross-section at a single height. We have added this information to the text. Additionally, the reviewer's highlighting of Siebesma and Jonker's work makes us realize we should have used the term "perimeter" instead of "circumference" in the text. We have altered the text accordingly.

What are the margins of error in the E/D vs a/C relationships?

The standard error on all of the fit slopes is less than 0.01 (0.007–0.009). We have added this information to the text.

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