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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observational constraints on entrainment and the entrainment interface layer in stratocumulus" *by* J. K. Carman et al.

Anonymous Referee #1

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Review of "Observational constraints on entrainment and the entrainment interface in stratocumulus" by Carman, Rossiter, Khelif, Jonsson, Faloona and Chuang

Overview

This paper describes measurements from 16 aircraft flights in stratocumulus clouds specifically designed to sample the entrainment interfacial layer (EIL). The authors build up composite vertical profiles of the EIL from numerous sawtooth profiles on each flight, by compositing each profile based upon the cloud top height in that profile. In addition to examining profiles of mean variables, the authors also derive small scale vertical TKE and vertical buoyancy flux estimates and composite these. Potential temperature

and TKE are consistent estimators of the EIL vertical extent. The EIL defined using moisture is not particularly useful because the jumps in moisture are too small in many cases to serve as a useful separator of FT and MBL air.

The first part of the study documents the vertical structure and extent of the EIL across the different flights using the different estimators of EIL extent. The results are very interesting and provide a wealth of new cases in which the EIL can be investigated in detail. The EIL is typically 40-60 m deep, and the cloud top does not serve as its base but generally occurs in the lowest one third to one half of the EIL. The authors use their observations to confirm previous large eddy simulations finding that the maximum thermodynamic gradients occur some distance above the cloud top. The turbulent fluxes also display interesting vertical gradients confirming the importance of negative buoyancy fluxes in the EIL, transitioning to positive fluxes below this.

The second part of the study takes the buoyancy flux estimates and attempts to use them to evaluate a metric of entrainment efficiency, i.e. the ratio of the integral over those layers where the flux is negative to the integral over the layers where it is positive. Only layers down to 100 m below cloud top are used. It is unclear to me whether this metric is a good predictor of the true entrainment efficiency because it misses the important contributions to buoyancy flux from layers lower down in the PBL, and because of the filtering out of eddies larger than about 50-100 m in the observational dataset. This is where having model simulations of one or more of the cases would greatly strengthen the paper, because the authors could then examine the degree to which their efficiency metric is a good predictor of the true metric used in Kraus and Schaller (Eqn. 4 in the current paper). I understand that the authors are presumably working with the modelers in POST to move in this direction, but are there no simulations of one of the cases that the authors could use to examine the utility of their efficiency metric?

The authors report some interesting correlations between their efficiency metric and the stability (greater stability leads to less efficient entrainment). This would be a very interesting finding if I could convince myself that their metric does not systematically miss a major part (both in spatial scale and in location within the PBL) of the positive buoyancy flux contributions. I hope that the authors can work on convincing the reader that their efficiency metric makes sense physically. Red lights start flashing when I see entrainment efficiencies of close to 1 (three cases exceed unity), whereas LES model runs show values several times lower than this and lower than 0.1 in all cases (in a model known to be too efficient at entraining). The authors do spend quite a bit of time discussing this in the paper, but I kept muttering "use a model" to myself when reading these sections. Given that there appears to be missing positive buoyancy flux in the observations, what is to say that there couldn't be a systematic correlation of this missing flux with e.g. stability, that would significantly change the correlations seen in Figs. 7-15?

Overall, I have no doubt that the results presented here serve as a useful and important contribution, and will be very interesting to the community. The paper is well-written and should be published subject to some revision. I believe it would be a much better manuscript if the modelers in POST could be brought on board, but I also understand that this would incur a lot more work, so could be left for later study.

Additional comments:

Entrainment closures: most modern closures do not use the flux partitioning type closure like that in Equation 4, but instead focus on convective velocity scale closures like that in Equation 1. At the least, the authors should include a discussion of how Equation 4 would actually be used to parameterize entrainment, because that is lacking in the current manuscript.

The typical horizontal scale of eddies in the EIL can be estimated as U/N where U is the wind speed and N is the BV frequency. For a thetav jump of 5 K in 20 m vertical, N=0.1 /s, so if U=5 m/s, L=50 m. Many of the relevant energy-containing eddies will probably be smaller than this, but it does suggest that a filter set to remove eddies

larger than 100 m will miss some of the EIL eddies.

Since the entrainment efficiency appears not to depend upon the TKE near the cloud top, this suggests perhaps that weighting TKE near the top more strongly in closures (e.g. Lilly 2002) might not be appropriate? Perhaps the authors can comment on this. P821, line 10/11: This is incorrect. The growth of the PBL top only equals entrainment if there is no large scale subsidence.

The authors should evaluate the sensitivity of their EIL structure, especially its relation to the cloud top, to their choice of threshold LWC (0.05 g/m3) used to define cloud top. Is this much different if 0.02 g/m3 is used instead? Why were turbulent fluxes of moisture not included in this study? I would think they could be quite interesting in some cases.

Fig 6. Looks discretized to 5m whereas the dataset used to derive this has a vertical resolution of 10m only. How is this?

P836: Factors controlling the EIL. Isn't the strength of the mixing a strong control on the EIL thickness, since mixing serves to sharpen up the inversion and then reduce the vertical EIL extent? Am I missing something? What nonlocal factors are likely to be relevant?

I'm not sure what to make of the lack of correlation between the negative and positive parts of the buoyancy flux integral. Suggests that consumption of TKE by entrainment has nothing to do with the creation of TKE. Doesn't this completely violate the Kraus and Schaller assumption?

P841: Why would one expect that deeper PBLs should have reduced above-cloud downwelling LW? If there is a correlation, I think it works in the opposite direction. Reduced downwelling LW drives stronger turbulence that deepens the PBL. Perhaps this is what the authors mean, but it's not too clear. Something similar might be happening in VOCALS (e.g. Bretherton et al. 2010, ACP). Can't this be tested with radiation observations from POST?

P843: Most people don't believe that the original Randall-Deardorff CTEI criterion is relevant to Sc. There are more physically-realistic criteria from e.g. Duynkerke. Table 2: If there is no correlation, why not just give the value of R2 rather than state "none"?

Typos:

Abstract: Line 10: "…properties tends to yield…."; line 17: "…analysis also demonstrates that the entrainment…." P819, line 12: air entrained air. Wyngaard citation incomplete. Should be Cambridge University Press. Equation 3 should not contain an h in the parentheses. Also, it is common to define this with a 2.5 inside the parentheses so that it seamlessly transitions to the clear convective PBL. P825: corrugated not corregated.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 817, 2012.