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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.

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Thanks to both reviewers for helpful and stimulating comments, questions and suggestions. Below are our point-by-point replies.

Reviewer #1

R: 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?

We totally agree that it's unclear what this means for true efficiency, and we were (hopefully) very open about that. Obviously, one of the reasons we focus on relative measures (like the spread of our calculated efficiencies, and the dependence of these on other parameters) rather than on the absolute values if for exactly this reason: we aren't measuring the true efficiency. This also means we are not intending to test, say, the Kraus and Schaller parameterization; instead we're more interested in exploring efficiency (albeit in our filtered way) as an interesting parameter unto itself. We've edited the text to try to be clearer about this.

We have tried to clearly state that comparison with models will be an important next step. We do not (yet) have a simulation set up for any of the POST cases, but to really do this properly, one would need a whole series of them representing different conditions to really understand the limitations of the method, which is a large undertaking which we certainly hope to pursue in the future.

R: 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?

We do discuss extensively why the efficiency is greater than 1 (c.f. p. 14, line 15 (or 14.15) to 15.5, followed by 21.25 to 22.10). We know and admit that our "efficiency" isn't a reflection of the true value, and so we use these calculated efficiencies to understand their relationship to other parameters, and not to report absolute values. There is certainly the possibility that what we're missing varies in ways that can change the relationships that we observe. However, the premise of the paper is that we observe what we are able, and the role of the models is to adapt simulation results to the observations, not vice-versa. So our philosophy is that as long as our methodology is clear, a model can follow the same procedure and examine whether these observed relationships are consistent in simulation (even if these same relationships disappear or are reversed if the "true" efficiency is considered rather than our more limited filtered efficiency). If they are, one could further explore issues related to the true efficiency, etc.

We've edited the paper in a few spots to try to be clearer about this.

R: 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.

We agree that this is certainly not the end of this story, but simply one chapter that prepares the stage for more work.

Additional comments:

R: 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.

That's a good point. Our strategy of invoking old parameterizations is that the idea that this is relevant to entrainment is longstanding, even if it isn't necessarily used in current models. We've edited this to be clearer.

R: 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.

Thanks for pointing this out. We were not previously aware of this. Using this simple calculation is a bit tricky since the wind speed can change across the EIL. But the basic order of magnitude estimation is helpful since it is consistent with those studies that we cite in the manuscript to address this issue, as well as the results that we see (the negative buoyancy flux integral is much closer to expected values than the positive buoyancy flux integral).

R: 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.

While the entrainment efficiency doesn't correlate with TKE in our study, we do find, however, that TKE at cloud top correlates with the negative buoyancy flux integral (Rˆ2=0.46). And this correlation is weaker if TKE throughout the boundary layer is ocnsidered. So it's not clear to us that this finding really suggests anything definitive about cloud-top weighted-TKE type closures.

R: P821, line 10/11: This is incorrect. The growth of the PBL top only equals entrainment if there is no large scale subsidence.

Fair enough. Edited to:

"Typically, entrainment rate is represented by the entrainment velocity, w_{e} , which is defined as the speed at which the boundary layer top incorporates fluid from the non-turbulent free-troposphere above it."

R: 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?

We did do some tests of changing the threshold LWC, and didn't find much difference. We've added this to the text.

R: Why were turbulent fluxes of moisture not included in this study? I would think they could be quite interesting in some cases.

Two reasons: 1) the moisture measurements were made at lower time resolution (10 Hz rather than 40 Hz) so it would have been harder to construct turbulent fluxes and 2) there were artificial oscillations when crossing the cloud top boundary (mainly on ascent) which, although easily removed from the 1 Hz data set (by using multiple instruments) did not lend any confidence to our ability to compute these fluxes.

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

The two primary authors looked at the vertical profiles independently to estimate these boundaries. If we disagreed, it was always by 10 m (one bin) and we simply averaged the two values to get a "best" choice.

R: 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?

We agree that it doesn't seem intuitive: our initial analysis goal was to seek out those local factors that were predictive of the EIL thickness. But as we describe in this paragraph, nothing we tried made sense, including turbulence/mixing strength. So we are left to conclude that either our measurements are insufficient to tease out relationships that do exist, or that local factors do not dominate and instead non-local factors are most important.

But when considering this comment, we realized this was not properly expressed in the manuscript. While non-local factors such as subsidence rate or horizontal advection of moisture and/or energy might play important roles, it may also be that local factors integrated over a long (e.g. 24+ h) period are what matter. In this scenario, we speculate that the 2 to 2.5 hr snapshot using aircraft may not properly represent the longer time scale appropriate to the EIL.

We've edited the manuscript to reflect this change, and have also added examples of non-local factors:

"We speculate that this may occur because the EIL is not controlled locally, and instead by processes that take place over larger spatial scales (e.g. subsidence rate or horizontal advection) or by local properties whose effects must be integrated over a long time period rather than considering only instantaneous values of, e.g. Delta theta-v or Delta-qt."

R: 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?

One explanation could be that the relationship between buoyancy-generation and -

consumption of TKE, while expected to exist, is modulated by outside factors (interfacial stability being the most obvious candidate) so that this correlation didn't come out.

We don't think this is inconsistent with Kraus and Schaller. We interpret their paper as stating that the ratio of the negative and positive buoyancy flux integrals is variable, which is what we find as well. Regardless, our interest in this manuscript was more to illustrate the idea of this ratio being of interest rather than to directly address the accuracy of this closure scheme.

R: 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?

The Stephens 1978 paper referenced shows model calculations where the higher the BL, the less atmosphere there is above, and thus less downwelling LW. However, in light of this comment and thinking about it more, we weren't terribly happy with this explanation either and found a related but more reasonable and consistent explanation: the more stable the cloud top interface, the drier the air (Rˆ2 = 0.7), leading to less downwelling LW, which results in more net outgoing LW, increasing buoyancy production. While we don't show the relationship, there is a correlation with Rˆ2=0.6 between the moisture gradient and the buoyancy production which is consistent with this idea. Doing a complete radiation (shortwave and longwave) divergence profile seemed like too large a task to undertake to demonstrate this one point. We have edited the manuscript to change our explanation.

R: 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.

We agree with the sentiment, and suppose we're piling-on at this point, which perhaps

isn't that useful. However, we have still seen people discuss this criterion recently so we decided in the end to leave this part there.

R: Table 2: If there is no correlation, why not just give the value of R2 rather than state "none"?

More for clarity. It seems to us to be visually easier to locate the values which are substantially different from zero in the table when it isn't littered with 0.1 and 0.05 values.

Typos and corrections:

All the below were corrected. Thanks for catching these!

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.