

Interactive comment on “Physics of Stratocumulus Top (POST): turbulent mixing across capping inversion” by S. P. Malinowski et al.

Anonymous Referee #1

General:

The paper provides a conceptual explanation of the entrainment process at top of stratocumulus clouds based on two case studies under different dynamic and thermodynamic conditions. The scientific topic is of great interest because stratocumulus is the prevailing cloud type over comparable cold oceans and covers huge areas. The entrainment mainly controls cloud lifetime and the structure of Sc-fields but is still not sufficiently understood.

I strongly suggest this manuscript for publication after considering a few suggestions that are presented below. Although I have no major scientific concerns I suggest that at least a few parts should be carefully edited and maybe re-organized (in particular for Sec 3 – see comments below).

I have to mention that I am not a native speaker and will not make any comments about the language and style; several co-authors are native speakers and can do this job much better than I can.

Thank you for your good evaluation of our work. The present version of the text was checked by native speakers. The new one is re-organized according to your suggestions.

Specific comments

The introduction provides a nice overview of the problem but maybe it could be at some place more precise instead of listing previous experiments and numerical investigations of the problem. What are the specific findings of these previous investigations and what are the specific problems/open questions in describing entrainment?

We expanded the introduction adding discussion of different entrainment values obtained for the same experimental data.

Page 15235, line 23, what does "TO" means before the flight numbers?\

This is an abbreviation of the aircraft name: TO-Twin Otter. Explained in the text.

A discussion of a recently published manuscript by Katzwinkel et al 2011 is missing in the introduction - in particular in that part of the introduction where the presence of an EIL and the need of high-resolution measurements are presented.

We added the following sentence:

“Such a shear-generated turbulence was observed in a great detail in the course of several penetrations of Sc top performed with a helicopter-borne measurement platform ACTOS (Siebert et al., 2006), reported in Katzwinkel et al. (2012).”

There results and data interpretation are quite similar compared to the present manuscript and the differences have to be clearly clarified and discussed. I realized that this paper is cited in Sec 4.3 but not really discussed and compared to the findings of the manuscript.

We added appropriate discussion in sections 3 and 4.

Quite often the vague term “high-resolution” (or similar) is used, what do you mean exactly?

At least ~1m. It is now explained in the introduction.

What is needed to observe the different details of the EIL and what resolution was actually achieved during POST for the different parameters? (I realized that technical information comes later but I think when mentioning this problem of resolution requirements here it should be clarified what was achieved during POST).

We prefer to stay with the present version of the text, the second reviewer had no objections.

Page 15236, line 20ff: How is the resolution of the PDI defined when it measures individual droplets? Do you mean the integration time for estimating a full droplet size distribution? Should be clarified. You mention "two sets of microphysical probes" – what are the others?

The reviewer seems to suggest editing "*producing 1 Hz (55m spatial resolution) outputs.*" We have edited this phrase to read "*producing 1 Hz (55 m spatial resolution) drop size distributions.*" He also suggests removing the reference to "*two sets of sensors*" since it's not really relevant to this paper.

We changed this fragment of the text to:

"Microphysical data discussed here were collected by the Phase Doppler Interferometer PDI (Chuang et al. 2008) located in a pod under the left wing, ~10m from the fast sensors. It produces 1Hz (55m spatial resolution) drop size distributions."

PDI droplet size resolution is addressed in the reference (Chuang et al. 2008). The resolution for any single droplet is estimated using laboratory measurements of drops produced using a vibrating monodisperse droplet generator. One standard deviation of droplet size is approx. 1 micron, with weak dependence on droplet size.

At the end of Sec 2 on page 15237 one could consider a somewhat more conclusive description of the two cases (classical and non-classical), for example does both cases include shear? A more detailed description follows in Sec 3, which is fine with me but if you already mention the two cases at the end of Sec 2 I suggest to give some more details at this place.

We prefer to stay with the present version of the text, the second reviewer had no objections.

Page 15239, line 12: " It is interesting..." I would avoid such sentences or explain why such an investigation is interesting. I think this sentence is not enough to justify Fig 5. The motivation to show this plot here is not clear and should become stronger because the presented data is interesting.

OK, corrected.

Figure 6.: It looks like that below 95m below cloud top, the drop size distribution indicates significant smaller droplets compared with the layers above – any idea?

Poor statistics of deep penetrations...

Page 15240, line 14 & 15: I don't really understand this sentence, can you please clarify what exactly you mean?

Replaced by " *θ_i and total water profiles show dilution of the ~80-m thick region in the upper part*" in the revised version of the text.

I strongly suggest to quantify the shear in Fig 7 (and 2) and show a profile of $\sqrt{(du/dz)^2 + (dv/dz)^2}$ or so.

We would like to avoid such estimates on a single case. The inclined path of the aircraft samples both horizontal and vertical variability, which definitely influences vertical gradients. We believe that the strength of our results is in Figs 14 and 15 which show statistics of the results.

At several places you use phrases such like " this parameter is high above cloud" or so, I suggest being more precise and quantifying these values.

OK, corrected.

General comment about Sec 3: The two different cases are really interesting and worth to be shown in detail, in particular when it comes to the cloud response in terms of the microphysical properties. However, it was difficult for me to follow all the differences and details between the two cases and I suggest a slightly different presentation of the data. Why not using a non-dimensionalized vertical axis and showing both cases in one plot or at least close to each other. For example combining Fig 2 & 7, which would allow a direct comparison and the differences, would become clearer. With two different plots a few pages separated one has to switch all the time. This is only a suggestion but I feel that this would improve the manuscript significantly and it would become much easier for the readers to understand and realize all the details and difference of the two cases!

We rearranged the whole section according to the suggestion. The section 3 is entitled now: “*Two cases: classical TO10 and non classical TO13*”.

The subsections are now:

- 3.1. *Atmospheric soundings*
- 3.2. *Cloud tops*
- 3.3. *Fine scale structures*
- 3.4. *LWC profiles*
- 3.5. *Microphysics*

Introduction of Sec 4: Are there, by chance, photos of the different cloud decks available
– would be great to illustrate the differences!

Unfortunately we do not have appropriate illustrations of publication quality.

Please check the units for the squared wind shear in Fig 12 & 13,
Corrected, thanks!

I also suggest to label the different layers in the figure instead of only mark them by vertical lines. It will help the reader!

OK, done.

Why do you show the parameters as a function of time including a time series of the measurement height instead of presenting the material as a function of height in general?

We do want to underline that the measurements were NOT taken in vertical, but along an inclined slope.

It would be natural to show a profile as a function of height and the interpretation would be much easier, this comment is also valid for a few previous figures.

We know that this is a common practice, but in this way of presentation is misleading, since often variability of measured parameters along slightly inclined slope is treated as vertical variability.

Honestly, in Fig 12 I would place the vertical black lines slightly different and in Fig 13 I see no convincing arguments for distinct layer. The explanation in the manuscript seems to be somewhat arbitrary?! Please comment on this.

In a first approach to the data analysis we used subjective layer division. We found it unsatisfactory, too dependent on the personal opinion. Thus we spent a lot of work to produce, test and verify an algorithmic method of layer division, described in section 4.1.

Why did you use the “i” in the Richardson number as an index? R_i instead of R_i ? I

never saw this before.

Corrected.

Is it possible to provide mixing diagrams (e.g., cubed diameter over droplet concentration) for both cases? Are they different? If yes, one could think of including one figure with both cases.

Mixing diagrams would describe in more detail the microphysical response to entrainment. We agree that this kind of analysis would be useful and we plan to work on it in future. Nevertheless, we believe that the way we have presented the microphysical properties along with the accompanying discussion is sufficient for the purposes of supporting the differences between these two days. An increased level of detail probably isn't suitable for this paper where the focus is on the nature of the entrainment itself.

Figure 16: This conceptual figure is nice and illustrative but maybe it could be improved a little bit. The capture is at least in some parts difficult to read and the explanations should be re-ordered. It is difficult to follow all the lines and I suggest mentioning the meaning of dashed and solid lines earlier. In addition, the meaning of the red arrows is not really clear for me. Maybe (just an idea) it would be clearer if you show a straight line for the temperature and LWC instead of fluctuations. At some place in the cartoon there are just too much lines and "scatter".

We changed the cartoon, separating "classical" and "non classical" cases into two panels, according to the suggestion of the Referee 2.

A final question: Can this conceptual picture help to estimate the amount of entrained air or to estimate an entrainment velocity? How can modelers benefit from your findings?

The conceptual figure is not only for modelers. We hope, however, that the modelers will make use of our results. We know that efforts to quantify our findings with use of high-resolution numerical experiments are on the way.