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Anonymous Referee #2

This is a very well-written, well-documented study of the entrainment process across the top of the marine stratocumulus-capped marine boundary layer. The authors apply recently-developed observational capabilities to make novel observations of the entrainment region and do a careful analysis of these observations to make new insights into the processes involved. In particular, they contrast two different regimes and carefully document the differences. This paper is a significant step forward in understanding in more detail how the entrainment process works in this regime, which is an important scientific issue that has relevance to e.g. global climate models.

Thank you for a good evaluation of our paper.

p. 15235, l. 14: *" . . .in order to allow for entrainment, turbulence in the inversion must be excited. This means that at least locally . ."* This statement implies that with no shear there is no entrainment. Yet, we know that buoyancy is also a source of turbulence and that buoyancy generated turbulence can also entrain. I think that this needs to be reworded to not exclude the possibility that buoyancy-generated turbulence can also result in entrainment. In fact, I find it puzzling that radiative cooling at cloud top is seemingly never mentioned as a source of turbulence generation by buoyancy.

Your understanding of this sentence is in agreement with our intentions. We want to underline, that in a quasi-stationary situation no local production of TKE by shear means no entrainment across a statically stable inversion layer. Entrainment can occur only in the presence of a mechanism which can break a barrier of potential energy (PE) imposed by the capping inversion, as Richardson number indicates.
We agree that the buoyancy generated turbulence can also entrain, but not directly and mechanism of entrainment involves shear across inversion. Let's explain our understanding of this process. Buoyant turbulence is produced in regions characterized by negative Ri, **not in the EIL**. The primary mechanism in cloud top layer **below EIL** is production of negative buoyancy by a negative heat flux due to radiative and evaporative cooling. This cannot break the barrier of PE. Updrafts are produced at the surface of stratocumulus topped boundary layer (STBL) by positive heat flux and are eventually accelerated by latent heat effects. These effects result in Rayleigh-Benard convection across STBL. Organized updrafts impinge upon inversion. They are relatively weak (we discuss STBL, not convective BL over land) and their kinetic energy does not allow to break the PE barrier. Thus updrafts diverge below/at the inversion which results in production of local shear. This shear can excite turbulence and entrainment locally, or add to the shear resulting from a large scale forcing. We referenced in the paper the LES simulations by Kurowski et al., 2009, (consult Figs 7 and 9 and corresponding text therein) that explain in detail this effect.
We explain it in a revised version in a following way:
... turbulence in the inversion must be excited. Inversion capping a typical marine stratocumulus is usually too strong to allow buoyancy-driven turbulence, produced elsewhere and advected into the EIL, to break the barrier of potential energy and mix across the stably stratified layer. This means, that at least locally...

p. 15235, l. 22: . . .providing data for the analysis is in Sect. 2.
This is corrected in the revised version of the paper.

p. 15235, l. 24: Data analysis and discussion of the results are presented. . .
This is corrected in the revised version of the paper.

p. 15236, l. 13 UFT should be defined.
This is corrected in the revised version of the paper.

p. 15236, l. 20: . . .and absolute humidity, respectively.
This is corrected in the revised version of the paper.

p. 15236, l. 25: . . .The data is. 4 lines earlier "data" is considered a plural noun. Which will it be?
This is corrected in the revised version of the paper .

p. 15239, l. 8: *". . .the amplitude of temperature fluctuations is an order of magnitude smaller and temperature variations are not always correlated with those of LWC."* I see little evidence of any correlation at all; but if anything, overall it seems to me that there might be a slight negative correlation.
"Always" is removed.

p. 15239, l.20: *". . .i.e. parcels of negative buoyancy, formed in the course of mixing and evaporative cooling at the cloud top, slowly descending across the cloud deck."* A question I have is how fast do they descend? These parcels have very little negative buoyancy so the descent must indeed be very slow. Is it negligible? Can you give any estimate as to what it might be? If you assume that this is a relevant process there should be some basis for the assumption. I made some very crude assumptions using the approach by J. S. Turner, 1962: The "starting plume" in neutral surroundings. J. Fluid Mech., 13, 356-368, and I conclude that it takes what seems to be an unrealistically large "buoyancy flux" (Q in Turner's terminology) to get a long enough time (eq. 13 in Turner's paper) for a significant vertical displacement. Admittedly, this is very crude; perhaps the authors can do better? Otherwise, I think they should be more cautious about assuming that parcel descent is a significant process in this region.

Conditional sampling performed on a negatively buoyant parcels in LES simulations of Kurowski et al., (2009) presented there in Fig.12 show a typical value of 0.2-0.3 m/s.

We reworded the sentence:
"Parcels with reduced LWC most often appear in the CTMSL, while in the CTL a depletion of LWC is less common and indicates the presence of ``cloud holes'' Gerber(2005), i.e. parcels of negative buoyancy, formed in the course of mixing and evaporative cooling at the cloud top, slowly descending across the cloud deck as shown in Fig.12 of Kurowski (2009)".

p. 15240, l. 14: *"θl and total water profiles are deflected from vertical across the ~80 m thick region in the upper part of the cloud."* I don't know what "deflected from vertical" means here.
Replaced by *"θl and total water profiles show dilution of the ~80-m thick region in the upper part* in the revised version of the text.

p. 15241, l. 28: *". . .rather than slowly increasing as in TO10."* It seems to me that the median droplet diameter is constant with altitude near the top for TO10. What don't I understand?
Green line in Fig.6, corresponding to the median droplet diameter shows a weak, but systematic increase in a 60m thick layer below the cloud top.

p. 15242, l. 6: *"High humidity of entrained air would be expected to decrease number concentration to a much larger degree than drop size. . ."* This is not obvious to me. Is there a simple explanation for this that eludes me?
Replaced by *"High humidity of entrained air suggests that dilution will affect the number concentration to a much greater degree than partial or total evaporation of drops will decrease either drop concentration or drop size"* in the revised version of the text.

p. 15242, l. 10: *". . .may be that collisional growth is also active in this cloud."* Is this because of the larger droplets?
Yes.

p. 15246, Eq. (2): Lenschow et al.: 2000: Measurements of fine-scale structure at the top of marine stratocumulus, BLM, 97, 331-357 also estimated Ri in the interface region by a somewhat different analysis technique and over a smaller height interval.
The results seem consistent with and complementary to the estimates obtained here in that a critical Ri occurs in the vicinity of cloud top.
Thank you for pointing this omission. We added this information in the revised version of the paper. We also added information on PDF's of Ri estimated from LES simulations in Wang et al., 2012. The last sentence of section 4.2. in the revised text is now:
"It has to be mentioned that similar experimental values of Ri at Sc top region are reported by Lenschow et al., (2000). Histograms in Figs. 14e and 15e closely resemble that in Fig.7 of Wang et al., (2012) obtained with use of LES simulations."

p. 15269, Fig. 16: I have to say that this schematic is a nice try, but it doesn't do much for me. It seems too busy. Also, I don't understand why the layer boundaries are nearly straight lines slanted to a peak near the middle. Perhaps it would help to have a separate panel for each case?
We improved the scheme according to the suggestion.

References:

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Wang, S., Zheng, X., and Jiang, Q.: Strongly sheared stratocumulus convection: an observationally based large-eddy simulation study, Atmos. Chem. Phys., 12, 5223–5235, doi:10.5194/acp-12-5223-2012, 2012.