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

Anonymous Referee #2

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Overall Evaluation

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.

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Interactive Discussion
Discussion Paper

Specific Comments:

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.

p. 15235, l. 22: . . .providing data for the analysis is in Sect. 2.

p. 15235, l. 24: Data analysis and discussion of the results are presented. . .

p. 15236, l. 13 UFT should be defined.

p. 15236, l. 20: . . .and absolute humidity, respectively.

p. 15236, l. 25: . . .The data is. 4 lines earlier "data" is considered a plural noun. Which will it be?

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.

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 unrealisti-

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cally 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.

p. 15240, l. 14: "θl and total water profiles are deflected from vertical across the âĹij 80 m thick region in the upper part of the cloud." I don't know what "deflected from vertical" means here.

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?

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?

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

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.

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?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 15233, 2013.

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Interactive Comment