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Interactive comment on "Physics of Stratocumulus Top (POST): turbulence characteristics" *by* I. Jen-La Plante et al.

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We thank the anonymous referee for his/her review of our work. We will improve the manuscript according to most of the referee's suggestions, which we find inspiring, important and valuable. While we respectfully disagree with the general comment that there is not enough material in the paper to justify the publication, we agree that the additional analyses and discussion suggested shall improve the paper's quality.

Referee1, general comment:

"This study analyzes turbulence properties of the EIL by decomposing it into two sub-layers based on the POST observation data. Their analysis confirms existence of shear generated turbulence in the EIL, and suggests adjustment of the EIL so that the bulk Richardson number is maintained near critical value. Also, the authors show

anisotropic turbulence in the EIL due to damped vertical fluctuations by static stability. While their analysis is valid, two of these main results are not new, so I think that the authors should perform further analysis so that this study is considered to be published in ACP."

Our answer:

We agree with the referee that the present paper extends earlier study, but we believe that there are more new findings other than the ones noticed by the reviewer. From our point of view:

1) We extended former preliminary results concerning shear generated turbulence in stably stratified inversion, layer division and bulk Richardson number Ri based on two cases only to a wider range of stratocumulus top conditions, making these findings not hypothetical but more robust and well documented.

2) We provide important new information on the thicknesses of cloud top sublayers.

3) Most importantly, we characterize properties of turbulence in the sublayers by numbers, providing so far unknown information on anisotropy of turbulence, TKE and its components and estimates of TKE dissipation rates. We show that turbulence characterized by these numbers is DIFFERENT in sublayers, despite the fact that thicknesses of turbulent inversion sublayer (TISL) and cloud top mixing sublayer (CTMSL) result from near critical value of Ri.

4) We characterize anisotropy of turbulence in cloud top sublayers by means of Ozmidov and Corrsin scales, showing that these scales reach minimum of few tens of centimeters in TISL and of single meters in CTMSL. Such characterization of Sc top has not been documented so far.

We do not resist further analysis, but we disagree with the opinion that only a minority of the paper contains new findings.

Questions and suggestions:

1) *"...why the algorithm does not successfully divide the EIL into two sublayers for all cases, but only 8 cases?"*

This is a misunderstanding, and we will add explanations in the revised text. Our algorithm works well in all the cases we investigated. We limited ourselves to 8 cases due to practical reasons: workload to perform the analysis is enormous and resources are limited. Thus, from all flights we selected 8 cases covering the whole spectrum of physical conditions observed during the experiment.

2) *"How is the assumption for the characteristic horizontal size of large eddies of the order of approximately 100 m justified?"*

Some justification comes from the small influence of the averaging length on final results, which is written in the text. More justification can be found in power spectral densities and structure functions of vertical velocity fluctuation in TISL and CTMSL showing signatures of weak scale break at about 100 m. We will discuss this point in the revised text.

3) *"Why the classical cases show long tails in the CTMSL (figure 3). TO14 also has longer tail"*

This is most likely related to the accuracy of the estimate of the shear across thin layers. We will analyze this and discuss it in the revised text.

4) *"Why the theoretically equivalent method to estimate the TKE dissipation rate gives sometimes very different results?"*

In our opinion methods used to estimate the TKE dissipation rate are theoretically equivalent only in homogeneous, isotropic, stationary and neutrally stratified turbulence, which is not the case in our study. In the manuscript we write: *"Derivation of the TKE dissipation rate from moderate-resolution airborne measurements is always problematic. The assumptions of isotropy, homogeneity and stationarity of turbulence, used to calculate the mean TKE dissipation rate from power spectra and/or structure*

functions, are hardy, if ever, fulfilled. This is also the case in our investigation of highly variable thin sublayers of the STBL top and is enhanced by the porpoising flight pattern. Considering these problems, we estimated the TKE dissipation rate by two methods. Three spatial components of velocity fluctuations are treated separately, allowing for the study of possible anisotropy, which is expected due to the different stability and shear in the stratocumulus top sublayers." Nevertheless we will add additional discussion.

5) *"What is a better way to incorporate ... findings into entrainment parameterization?"*

This is a complex question, worthy of a new paper when answered. At this moment we may only state that we see a need to perform very high resolution (close to Corrsin and Ozmidov scales) numerical simulations of the cloud top region in order to understand how eddies that are anisotropic by shear and static stability transport mass, and how exactly the exchange between Sc top and free atmosphere looks. We will elaborate on this in the revised text.

6) *"Another concern is that, although I see some usefulness to study these two sublayers, I am not fully convinced if decomposing the EIL into two sublayers is absolutely necessary, since their main results seem to hold for the bulk of the EIL. In other words, their motivation to study two sublayers is rather weak and the significance of analyzing these two layers is not fully appreciated. This criticism partly comes from the lack of discussion for Tables 2, 3, and 4 and Fig. 6."*

We will provide more discussion to better document the importance of division into the sublayers.

We thank the reviewer for the specific comments and will account for them in the revised manuscript.

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