Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-950-RC3, 2016


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

**Anonymous Referee #3**

Received and published: 3 March 2016

The manuscript discusses turbulence characteristics in the region of the cloud-top of stratocumulus clouds. The in-situ measurements from the POST campaign are used. The analysis is organized by considering the layer division classification of Malinowski et al. (2013), who carried out a closely related analysis of the POST dataset. The paper has two main goals/contributions: (a) derive small-scale and high-order turbulence statistics, such as the kinetic energy dissipation and velocity structure functions and (b) Using the POST dataset analysis, inform approaches for understanding and modeling the cloud-top entrainment process.

The study of stratocumulus clouds is of significant interest because of their large radiative forcing and the paper should be of interest to the broader atmospheric science community. The effort to process and analyze the in-situ measurements is also significant, because, as the authors point out, these measurements are difficult and valuable.



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Discussion paper

Overall, I find the paper interesting and a valuable addition to the stratocumulus literature. However, the paper has a significant limitation: it is mostly a presentation of processed results from the POST campaign. Most of the conclusions are expected and the investigation does not have significant depth in terms of links to theory. I believe that the current manuscript is not suitable for publication in Atmospheric Chemistry and Physics. A substantially revised version of the manuscript can be suitable for publication.

Major comments:

1. The analysis of the results is presented without any reference or relation to the broader meteorological conditions. The results, such as the dissipation rates of Figure 7, show large variability between flights. The authors seem to suggest that the bulk Richardson number and a second parameter based on neutrally stratified dynamics (they use the Corrsin scale) are sufficient to characterize the data. If this is the suggestion, it should by made clearer and explicit.

2. Further to the previous point, there is no information about the broader large-scale environment. For instance: the authors report zonal and meridional velocity statistics but there are meaningless without a reference direction. These should be presented with respect to the direction of shear.

3. Some information about the nature of convection and the radiative forcing of the cloud top should be included to make the presentation more self contained.

4. One of the conclusions is that "Turbulence in both sublayers is highly anisotropic, with Corrsin and Ozmidov scales...". I think it is well-established that the largest turbulent motions in the inversion are anisotropic. In fact, this is what the Ozmidoc and Corrsin scales characterize: the smallest scale where the effects of stable stratification and shear are important. The interesting question is if there is enough separation of scales from L_O or L_C to the Kologorov scale for the turbulence to approach isotropy at small scales. This is important for modeling, because many turbulence closures as-

sume small-scale isotropy and Kolmogorov scaling. It is perhaps beneficial to consider also the buoyancy Reynolds number (see eq. 1.3 and related discussion in Chung & Matheou, 2012, Journal of Fluid Mechanics), in addition to the length scales.

5. All the analysis is carried out under the assumption that stratification and shear are the dominant processes and that radiative cooling and buoyancy modification by latent heat exchange (e.g. buoyancy reversal) are neglected. This should be better justified. On line 60 these other processes are mentioned and it is argued that "These multiple sources are responsible for exchange across the inversion."

6. Further, assuming that radiative cooling and latent hear exchange does not play a significant role, why are the results not appropriately scaled? Most of the results are reported in dimensional quantities. Some of the scaling in Chung & Matheou (2012) and references therein can apply to the current data.

7. The definition of the Richardson number in eq. 1 should be based on the virtual potential temperature, rather than just the potential temperature.

Minor comments:

1. All the figures are very difficult to read.

2. On line 48: "Turbulent transport across the inversion is a mechanism that limits exchange between the cloud top and free atmosphere and should be considered". This sentence is not clear and it means that turbulent transport limits the transport of mass, momentum and energy across the inversion. Perhaps the intention is to say that stable stratification limits turbulent transport across the inversion.

3. On line 95: "These measurements indicated that wind shear across the EIL is a source of turbulence" Wind shear is a source of turbulence, the measurements are not needed to show this.

4. On line 100: I think a better term is "an empirically based division" rather than "an experimentally based division"

5. On line 165: perhaps is better to use the term “aircraft”

6. In Figure 1, in the caption, I am not sure what “The corresponding lines indicate segment-averaged” means. Which lines are the authors referring to?

7. In figure 6 the logarithm of distance and structure function are used as the x-y axis, rather than plots with logarithmic x-y scale. Currently, the x-axis has units of logarithm distance, which is strange.

8. In Table 3 and the corresponding text that refers to the table, it is not very clear what u, v and w mean.

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