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Interactive comment

Interactive comment on "Turbulent and Boundary Layer Characteristics during VOCALS-REx" *by* Dillon S. Dodson and Jennifer D. Small Griswold

Anonymous Referee #2

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Review of the article titled "Turbulent and Boundary Layer Characteristics during VOCALS-Rex" by Dodson and Griswold for publication in the Atmos. Chem. Phys.

The authors have used data collected by the Twin Otter aircraft during the VOCALS field campaign to characterize the boundary layer turbulence and microphysical properties. The article describes the synoptic, cloud, turbulence and boundary layer properties at the point-alpha, and then goes on to do correlation statistics between them. The article is overall well-written, and easy to read. However, the analysis falls short in many aspects. The principle issue stems from a lot of research already been done on the collected data. Please find below some suggestions for improving the manuscript. I recommend this article for publication after major revisions. The amount and nature of comments are rather severe, sorry about that, but they are meant for improving the

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Discussion paper

paper.

Major Comments: The referencing in this article looks little outdated. Most of the references in the introduction section are from the 80s, 90s, 00s, and the latest paper is Wood 2012. It will be good if the authors can do a thorough literature review and only refer papers from the last 5-10 years. I completely agree with the authors that the old papers are still valuable and are relevant. However, some of the conclusions/speculations reached by the authors have already been made by the subsequent article. It will be good if the authors can improve the referencing. There have been plethora of stratocumulus-turbulence interaction studies done in the past 5-10 years, using the cloud radars and large domain LES models. Line 94-99 document the turbulence structure of stratocumulus topped boundary layers, and it seems that the authors are not aware of recent findings.

In a similar vein, it is unclear to me why the authors have not considered other papers from the VOCALS campaign. Especially as they are all in the VOCALS special issue in ACP. The conclusions similar to this article have been reached by Jones et al., and Bretherton et al. papers in the special issue. It will be good if you can put your results in the context of other studies. Thanks.

Abstract line 10: the main conclusion of the article is “Findings show that the influence of a synoptic system on Nov 1st and 2nd brings in a moist layer above the boundary layer, leading to a deepening cloud layer and precipitation during passage, and a large increase in boundary layer height and cloud thinning after passage”. This is contradictory to the notion that moisture above the boundary layer reduces the cloud top cooling, thereby inhibiting turbulence and thinning the clouds. Please see Eastman and Wood (2018 JAS) and other papers. Do you think that the deepening of the boundary layer might be due to decrease in subsidence or increase in the surface fluxes? In any case, correlation does not imply causation, so maybe you can rephrase this sentence. Thanks.

Section 2.2 documents the way turbulence statistics have been calculated. It will be good if you can also include some sort of error analysis in it. I suspect the differences in you see are not statistically significant. This is often the case, however you should at least document these. Your results still should be relevant. The w'N' and the skewness of vertical velocity are the prime suspects in my opinion. Please see papers by David Turner and Wulfmeyer on the calculation of higher order moments. Also, how good are the temperature and humidity measurements within the cloud layer. The sensors suffer from significant drop shattering and cooling. Can you please discuss if the measurements are sufficient for calculating buoyancy fluxes. Thanks.

You are confusing the inversion layer and the entrainment zone. These are two different things. The entrainment zone is within (plus-minus 25 m) of the cloud top, while the inversion layer can span 100s of meters at times. There is no known mechanism that can bring air from above the top of the inversion into the cloud layer. This needs to be changes throughout the document. Please see the papers by Juan-Pedro Mellado. Thanks.

One of the main conclusions is that "A maximum in TKE on Nov. 1st (both overall average and largest single value measured) is due to precipitation acting to destabilize the sub-cloud layer, while acting to stabilize the cloud layer." This contradicts your earlier statement in the introduction about evaporating drizzle stabilizing the sub-cloud layer. There have been LES modeling studies and some observational studies showing drizzle to stabilize the sub-cloud layer, directly contradicting your conclusions.

Minor Comments:

Line 236-237: This has been already stated in the introduction section, so please remove. Thanks.

Line 268: The modulus of a number does not read well. I think you mean the absolute change. Maybe you can just mention (absolute change > 0.1)? thanks

Equation 9 seems out of place. I am not sure if it conveys anything meaningful.

Figure 1: Convert Omega to Pa/day and put latitude and longitude in regular (-ve for southern hemisphere) units.

Figure 2: Panel (b) is surface air temperature?

Figure 3: please convert Omega to Pa/day. The figure also doesn't tell much, so maybe you can move it to supplemental material.

Figure 4: Instead or in addition to the wind roses, it will be good if you also show the profile of wind speed. Thanks.

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