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Atmospheric Chemistry and Physics
Discussions

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

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COMMENT: The referencing in this article looks little outdated. Most of the references in the introduction section are from the 80s, 90s, and 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 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 has been a plethora of stratocumulus-turbulence interaction studies in the last 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.

REPLY:

* Referencing throughout the article has been updated. Although you make it sound as if we should only refer papers from the last 5-10 years, we have kept most of the original references and added newer (post 2010) references. Previous reviewers of previous articles have been picky about referencing the original papers. However, we do understand the need for having more balance, which we think the current manuscript achieves.

* A total of 42 references have been added (we won't list them out here) throughout the article which are dated 2009 or after, providing a more balanced approach to the references.

COMMENT: 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 a good idea if you can put your results in the context of other studies. Thanks.

REPLY:

* Originally, the lack of other VOCALS papers stemmed from the fact that most of the papers which have been published used datasets other than the Twin Otter at point Alpha. Papers which do use Twin Otter data tend to focus on aerosol and cloud microphysical properties, and not turbulence.

* The Twin Otter data is not the primary focus of analysis for other papers that have been published from VOCALS-REx. For example, Jones et al. (2011) and Bretherton et al. (2010), although they analyze the boundary layer structure and decoupling, the data being used is from the NSF C130 and/or UK BVAe146. However, you are correct in saying that results here can therefore be related to those findings.

* We have added a section to discuss previous VOCALS papers on lines 56-68 of the

new manuscript. In particular, results have been related to Jones et al. (2011) and Bretherton et al. (2010), including adding new measures of boundary layer decoupling in Figure 7 that are presented in Jones et al. (2011), as to better relate findings here to their results.

* We also found several instances where we mention findings from Zheng et al. (2011), but fail to circle back around and compare our finding with theirs (A specific example of this is the statement on lines 407-408 of the new manuscript, and the follow up statement on lines 653-655).

COMMENT: 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.”. 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.

REPLY:

* The sentence in question here is no longer directly in the abstract. We do discuss the precipitation and synoptic system on lines 13-18 in the new manuscript, and it should be worded more properly. We have also added an extensive discussion on how the moisture above the boundary layer can affect cloud top cooling and other cloud processes. Please see lines 629-634 of the new manuscript.

COMMENT: 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.

REPLY:

* The deepening of the boundary layer. . ..do you mean in regards to after the synoptic system passage? In the sentence in question, we state that the cloud layer deepens

(becomes more thick). As for the boundary layer height, it remains relatively unchanged between Nov 1st and 2nd (decreases roughly 50-m), but the cloud thickness becomes 100-m thicker. This is due to reduced cloud top cooling limiting the deepening of the boundary layer, while entrainment that is occurring will result in a lower LCL due to the higher moisture content. Please see lines 657-660 of the new manuscript.

* If you were referring to the deepening boundary layer after synoptic system passage, we discuss that on lines 699-704 of the new manuscript.

* It should also be noted, in particular when we are discussing the correlation coefficients, that we do our best to word the phrases properly as to not imply causation. For example, on lines 7-12 in the new manuscript, we state that “As the latent heat flux (LHF) and sensible heat flux (SHF) increases, zi increases, along with the cloud thickness decreasing with increasing LHF.” This makes more sense than stating “as zi increases, the LHF and SHF increases.” We know that stronger surface fluxes will increase zi, but the correlation coefficients only tell us that they are correlated, not which causes the other. Everything should be phrased properly throughout.

COMMENT: 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.

REPLY:

* We have added several paragraphs at the end of Section 2.2 (See lines 227-265 in the new manuscript) addressing these concerns. You are correct that they should be documented. Figures 16 and 17 have also had the raw calculations added to the profiles of w’N’ and w’w’w’, which clearly shows that the mean values that were being displayed are NOT statistically significant (as you assumed).

COMMENT: 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.

REPLY:

* Please see lines 162-167 in the new manuscript, which addresses the concerns laid out above. Although we have a limited capacity to the detail and length of explanation which can be given within the manuscript, we think the information added should address the concerns. Also, if you are curious, you can see the links provided for more information on the total set up of the Twin Otter, which has taken great care to make the most accurate measurements possible.

https://archive.eol.ucar.edu/projects/post/meetings/200902/documents/khelif_POST_SLC_F

https://www.researchgate.net/figure/UCI-Turbulence-instrumentation-on-the-CIRPAS-Twin-Otter-in-POST-and-VOCALS-REx-field_fig1_228968823

COMMENT: 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 a 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 changed throughout the document. Please see papers by Juan-Pedro Mellado. Thanks.

REPLY:

* You are correct. We (multiple times) exchanged the terms inversion layer and entrainment zone. This has been corrected throughout the manuscript and a more accurate explanation has been added. In particular, see the discussion added in the introduction on lines 89-96:

"The boundary layer top is characterized by several strong gradients, including the

cloud boundary (gradient in LWC), the entrainment zone (gradient in vorticity, where the entrainment zone separates regions of weak and strong mixing between laminar flow above and turbulent flow below), and the capping inversion (gradient in potential temperature). The cloud boundary typically lies in the entrainment zone (Albrecht et al. 1985, Malinowski et al. 2013), which in turn lies in the capping inversion, although these layers do not necessarily coincide (Mellado, 2017). Turbulent analysis of these layers in Jen La Plant et al. (2016) found that turbulence (both TKE and TKE dissipation) decreases moving from cloud top into the free atmosphere above, where mixing of the laminar and turbulent flows occurs within the entrainment layer."

* All subsequent discussions of entrainment have been modified within the manuscript. Although there are multiple examples of this throughout the manuscript, please see lines 485-486 within the new manuscript for a specific example. All original explanations which made it sound like air was being entrained from above the inversion layer has been corrected.

COMMENT: 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.

REPLY:

* An updated discussion relating to precipitation and its effects on boundary layer stability has been provided. Originally the explanation of precipitation within the boundary layer and how it may change the turbulent profiles was lacking. In particular, lines 635-644 in the new manuscript provide an updated discussion on how precipitation can influence the boundary layer. Feingold et al. 1996 is the original (as far as we know)

study to demonstrate how evaporation from precipitation acts to change boundary layer turbulence. If evaporation is occurring in select regions away from the surface (say just below cloud base), the sub-cloud layer will become unstable (i.e., light precipitation is occurring). If evaporation is occurring throughout the vertical sub-cloud layer, and in particular near the surface (i.e., heavy precipitation is occurring), the sub-cloud layer will become stable. The most recent paper that we could find to report findings of this nature is Ghate and Cadeddu (2019), who found that for a similar amount of radiative cooling at the cloud top, the average vertical velocity variance in the sub-cloud layer was about 16% lower during strongly precipitating hours than during weakly precipitating hours.

* The earlier statement in the introduction was referring to the explanation provided in Zheng et al. (2011). It is stated that "Zheng et al. (2011) suggest drizzle processes act to stabilize the boundary layer, leading to decoupling on Nov. 1st." I have circled back around to this statement on lines 653-655 of the new manuscript, stating that Zheng et al. is correct in stating that drizzle acts to decouple the boundary layer, but wrong in suggesting that it acts to stabilize the boundary layer as well.

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

REPLY:

* We have removed the statement in question from the new manuscript.

COMMENT: 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.

REPLY:

* You are correct in that we mean the absolute change. We have taken your advice and made the necessary corrections. Please see lines 341-343 in the new manuscript.

COMMENT: Equation 9 seems out of place. I am not sure if it conveys anything mean-

ingful.

REPLY:

*This equation has been removed, although what the equation conveys has been kept. Please see lines 491-495 in the new manuscript. We were just trying to relate that the boundary layer height changes based on entrainment and large scale subsidence. This can easily be described, as opposed to showing the equation however.

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

REPLY:

* The units have been converted to hPa/day, which is much more relatable than the original Pa/second. We are also unsure what you mean by –ve for the latitude units. However, we have changed the latitude and longitude labeling to match what has been published in previous VOCALS-REx publications. (see Zhang et al. 2011, Toniazzo et al. 2011, Rahn and Garreaud 2010). If you would prefer a different unit or way of labeling, we would be more than happy to change it.

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

REPLY:

* Yes, it is surface air temperature. This has been added to both the figure description and figure label. See Figure 3 in the new manuscript.

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

REPLY:

* This figure has been removed, especially since we already have a large number of figures presented.

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

REPLY:

* We have kept to wind roses, but have also added a vertical profile for wind speed. Please see Figure 4 Panel (e) in the new manuscript.

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