Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-104-AC2, 2020

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

Dillon S. Dodson and Jennifer D. Small Griswold

ddodson@hawaii.edu

Received and published: 17 July 2020

COMMENT: I was surprised to see that other than the Zheng et al. study that looked at the CIRPAS Twin Otter data, no other Twin Otter studies are referenced, so perhaps these data have not been looked at before. I was extremely frustrated by its lack of clarity, rambling unfocused explanations, and grammatical and spelling errors throughout. The number of figures is also disproportionate to the information content of the manuscript.

REPLY:

* We hope that the modifications to the manuscript have addressed your concerns. In particular, we have made most of the recommended changes in regards to your

specific comments below, and have hopefully clarified some of the explanations given throughout the manuscript.

* The number of figures remains at 17 (one removed, one added through the revision process). Although yes, this is disproportionate, we also find it reasonable. The nature of this paper remains in characterizing boundary layer turbulence. Characterizing, or to describe something based on its main qualities, involves going over all those qualities in a broad sense. If the paper was focused on a single (or multiple) scientific questions to be answered, the paper would no longer be a characterization. Given the title of the paper, we assume that the reader will be aware of this fact, and understand what it is they are about to read.

* 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 UK BVAe146. Results here can therefore be related to those findings. A discussion has been added to the introduction (see lines 56-68 of the new manuscript) relating previous VOCALS publications to the datasets used.

COMMENT: 1) Abstract needs significant tightening and clarification of message. What is the role of radiative cooling? The last two lines could be written much more simply ("lower pressure allows the BL and entrainment zone thickness to increase" plus an explanation in a separate sentence of why turbulence decreases). Mentioning both in one sentence left me scratching my head.

REPLY:

* Hopefully you will find the structure more appropriate. We start the abstract by stating what it is we are going to do, the data we are using, where it was collected, and then provide various results which were found. It should be more focused.

* The role of radiative cooling seems to be out of place in the abstract, but is given more detail (in particular with regards to how radiative cooling is effected by the enhanced moist layer present on Nov. 1st and 2nd on lines 629-630 and/or 157-159 of the new manuscript.

* The last sentence from the old manuscript has been removed, but a similar statement has been made on lines 7-9 of the new manuscript. Pressure has been used in replacement of geopotential height, and the phrase has been divided into multiple sentences. Note we have replaced geopotential height with pressure when applicable throughout the manuscript.

COMMENT: 2) Introduction: Other than the message of "we're going to look at the turbulence data from this field campaign" I didn't get a sense of a focused science question. The authors should rewrite this with the benefits of hindsight to provide that focus.

REPLY:

* We would like to point out, that at no point in the original manuscript did we state "we're going to look at the turbulence data from this field campaign", we do outline our goals in two parts however (please see lines 51-54 in the new manuscript). While yes, these goals are broad, the point of the paper is to look at the in situ data collected to provide a general characterization of the turbulence within the boundary layer. This is a very reasonable objective, as no other paper from VOCALS has analyzed the turbulent data from the Twin-Otter aircraft to this extent. Given that the purpose of the Twin-Otter was to measure turbulent and microphysical properties, a paper which characterizes said turbulence seems more than reasonable (we also find it odd that one has not been published up to this point). We outline this in lines 65-68 of the new manuscript.

* 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 UK BVAe146. Results here can therefore be related to those findings.

COMMENT: 3) Section 1.1, there is a general lack of clarity and synthesis. Please be specific about mixing ratio (of water vapor!) and give it a symbol at the outset so its intuitive. Why is the Sc to Cu transition relevant here? "Buoyancy flux is the primary generator of TKE in the STBL" but what drives buoyancy flux? The reader has to read through to pick out the pieces and figure it out. The description of the Bowen ratio is straightforward and the text should be streamlined. Lines 104 through the end of this section ramble. On the other hand, you might explain why a larger latent het flux causes decoupling. Please simplify where necessary and expand where necessary to focus your key questions.

REPLY:

* The mixing ratio has been given a symbol in Section 2.1, where after that point it becomes much more common throughout the text. See line 161-162 of the new manuscript. The latent heat flux and sensible heat flux have also been simplified by using LHF and SHF, respectively.

* The original sentence on the Sc to Cu transition has been removed. Although not relevant, we were simply just pointing out that the process of entrainment and boundary layer deepening plays a key role in the Sc to Cu transition.

* Lines 100-101 in the new manuscript provides a very brief statement on what drives the buoyancy flux. Given that buoyancy arises from differences in density, which depends on temperature and moisture content, having to explain what the buoyancy flux depends on seems rudimentary. The statement in the original manuscript (and in the new manuscript) of "According to Shaw (2003), one of the main sources of TKE in clouds is evaporative cooling and condensational heating, implying the buoyancy flux is the primary generator of TKE in the STBL" on lines 107-110 of the new manuscript and lines 85-88 of the original manuscript also provides the fact that the buoyancy flux

is dependent on evaporative cooling and condensational heating.

* The explanation of the Bowen ratio has been simplified. Please see lines 118-119 of the new manuscript.

* The original statements from lines 104 through the end of the section have mostly been removed, even though they describe why the latent heat flux caused decoupling (perhaps you misread this portion). The discussion has been presented in a different format to hopefully make it more straightforward. Please see lines 120-123 of the new manuscript.

COMMENT: 4) Section 2.1: Shortwave absorption doesn't only occur at cloud top. Line 142: Don't you mean "on any given day"?

REPLY:

* You are correct, solar absorption does not occur only at cloud top (although this is where it is primarily confined). We have clarified this statement in lines 157-160 of the new manuscript. * "on any given day" and "from day to day" mean the same thing. However, we have reworded this to "on any given day" on line 173 of the new manuscript.

COMMENT: Given the focus on velocities, surely the instrumentation should be described rather than completely differed to Zhang et. al.?

REPLY:

* A more detailed explanation of the instrumentation, in particular that of the velocity measurements and moisture measurements, has been provided on lines 162-167 of the new manuscript.

COMMENT: 5) Section 2.2: You talk about 300-point averaging windows before discussing the sampling time. This is upside-down. Why is linear regression required to get the mean?

REPLY:

* The sampling time is initially discussed in the previous section (2.1) on line 134 of the original manuscript. However, we have corrected this and mentioned that the data used is 40-Hz before discussing the averaging technique on line 184 in the new manuscript.

* Linear regression is not required to get the mean, but it is one of multiple options. Other options include applying just a single mean value, or applying low pass filtering. Please see the figure 1 for examples of the three methods, all which are based on 320-pt. averaging.

COMMENT: 6) "Thetav is commonly used as a proxy for density". Please give a concise theoretical reasoning for this. You mention structure function method but provide no explanation of what it is. Please give a brief one. Simplify line 188. What are "interactions with the plane"?

REPLY:

* Virtual potential temperature is given by: theta_v = theta(1+0.61q-ql), where theta is the actual potential temperature and q is the mixing ratio of water vapor, and ql is the mixing ratio of liquid water in the air. Because water vapor is less dense than dry air, humid air has a warmer theta_v than dry air, while liquid water drops, if falling at terminal velocity, make the air heavier and therefore is associated with colder theta_v . Therefore, theta_v can be used for buoyancy. To be honest, this seems rudimentary to be included in a journal, but has been added nonetheless. Please see lines 200-203 in the new manuscript.

* We are quite confused on "you mention structure function method but provide no explanation of what it is". Please see Equations (5) and (6), which provide the structure function in mathematical form followed by an explanation. Please see lines 113-114 in the new manuscript. We have added a very brief explanation of what a structure function does (i.e., it is just a statistic to analyze common variation in a time series).

* Line 188 in the original manuscript has been simplified. See lines 221-223 in the new manuscript.

* Interactions with the plane (at higher frequencies) include aircraft vibrations, etc. Please see Figure 1 Panel (a) in the new manuscript, where the 0.3-5 Hz frequency range is shown as a light gray envelope. 0.3-5Hz covers the inertial subrange of the data, with a spike in energy located at roughly 10Hz, again, attributed to interactions with plane vibrations, etc. This spike in energy is also observed in Jen-La Plante et. al. (2016), where their explanation is “interactions with the plane”.

COMMENT: 7) Section 3.1: What is omega? Surely it should be defined and given a symbol?

REPLY:

* Omega is the vertical velocity in pressure coordinates (so positive omega is negative vertical velocity), having units of pressure per time. Since much of operational meteorology uses pressure surfaces, omega is a more common quantity to see, especially when quantifying larger temporal scale vertical motions. We have added a brief description of this on lines 275-276 of the new manuscript.

COMMENT: 8) Section 3.3: Although you don’t have flight data on consecutive days, you do have reanalysis that I expect would be helpful to address boundary layer height changes. (ECMWF?)

REPLY:

* This is a great point, and an excellent addition to the manuscript. Figure 5 in the new manuscript has added ECMWF-BLH data that was derived from the extrapolation of relative humidity (RH) data, where the BLH was determined in the vertical layer that had the largest gradient of RH. There is (relatively) good agreement between the in-situ and ECMWF data, and the ECMWF provides a look at what the BLH did during days where flights did not occur.

COMMENT: Top of page 10: why does enhanced moisture above the BL translate to higher aerosol? Here and elsewhere you would help the reader a great deal by using symbols like z’ for normalized altitude, theta, q, etc. – i.e., symbols that are in common use.

REPLY:

* Increased moisture can lead to aerosol swelling for aerosols that are hygroscopic. This means that aerosols that are smaller than the size range being measured by the PCASP (range 0.1 – 2 um) under dry conditions, may increase in size enough under more moist conditions to be measured. This has been discussed on lines 363-365 of the new manuscript. Also, symbols have been added for variables such as zi (inversion height or BLH), q (mixing ratio) and (potential temperature) throughout the new manuscript.

COMMENT: You mention a secondary cloud layer (line 329). Is this a layer of penetrating cumulus? Or something else?

REPLY:

* It is believed to be a layer of cumulus, but not penetrating the Sc deck. The profile of LWC in Figure 2 can give you a better idea of the structure of the profile, where the main Sc deck is in red, and the secondary cloud layer (cumulus layer) is in blue. See lines 412-414 in the new manuscript.

COMMENT: 9) Section 4: Line 332, don’t you mean horizontal layers?

REPLY:

* We did mean vertical layers, in reference to analyzing the boundary layer through distinct vertical bins or layers (i.e., between z/zi =0 to 0.25, or the bottom $\frac{1}{4}$ of the boundary layer, etc.). However, we think the paragraph reads better by just removing the sentence in question.

COMMENT: Line 347: This doesn't make sense. An increase in the Bowen ratio means an increase in SHF or decrease in LHF.

REPLY:

* This was a typo, and has been changed to sensible heat flux in the new manuscript (Figure 8 did display the correct information, the text just mixed up the sensible heat flux and latent heat flux). Please see line 438 in the new manuscript.

COMMENT: Line 378: How can Fig. 11 display the same information as Fig 10? Perhaps you mean it has the same format. There are similar instances.

REPLY:

* Yes, we mean that it has the same format as that of Fig. 11. This has been corrected throughout the manuscript. Please see line 468 in the new manuscript for an example.

COMMENT: The use of geopotential height is distracting, and for no good reason. You could make your points much more clearly by talking about pressure. I had to read the text starting from Eq. (9) through to near the end of the section a half dozen times and I still don't know what you are trying to say. Correlations are mentioned and causation is implied. And when it is not, one is left wondering why there is a correlation, and what confounders might be driving the correlation. The summary section might have helped, but it is poorly written, and sometimes repetitive, and circular. Why is geopotential height correlated with sensible heat flux? It may be simple, but at least provide a physical explanation. Stating "agreement with Palm (1996)" doesn't help. The last 3 lines of this section do make sense, and the 'could be' might not be necessary.

REPLY:

* Where applicable, geopotential height has been replaced with pressure. Please see lines 490-495 in the new manuscript for an example (although there are multiple instances where GPH has been replaced with pressure throughout the new manuscript).

* We believe we tried to convey to much information within this section (in regards to discussing the correlations), and as a result it seemed confusing and congested, with no clear start and finish (i.e., circular and repetitive, as you stated). This entire section has been re-written and simplified. Correlations which are not directly discussed in the text have been removed from Table 4. Each paragraph is arranged to discuss a correlation, to go along with a physical explanation for why said correlation exists.

* For example, the second to last paragraph in this section (lines 508-511 in the new manuscript), discuss the correlation between turbulence and pressure and boundary layer height. Correlation values are given, and a physical explanation for this correlation is provided. Again, the last paragraph in this section (lines 512-516 in the new manuscript), discuss the correlation between Na, Nd, and drop size with turbulence. Correlation values are given, and a physical explanation for this correlation is provided. The summary section has been removed (we feel it is no longer needed with how this section has been re-written), and the last three lines of the section from the original manuscript have been moved to lines 509-510 of the new manuscript, and the 'could be' has been removed.

COMMENT: 10) Section 4.2: Line 441, the variance peak at z'=0.99 might simply be because of the strong q gradient. Lines 480-418, you make it sound like the updrafts and downdrafts are meeting in the middle, but they must be spatially displaced.

REPLY:

* You are absolutely correct. The variance due to the strong q gradient is common in most boundary layer vertical profiles of q, and should have been mentioned. This has been added at two points within the new manuscript. Please see line 526 and line 575 in the new manuscript.

* The updrafts and downdrafts are not meeting in the middle. Here, we are simply implying that the peak in w'theta_v' at an in-cloud normalized height of 0.59 (near cloud middle) is due to the w' and theta_v' both being large and positive (i.e., warm

moist updrafts, a positive flux)) and due to the w' and theta_v' both being large and negative (i.e., cool dry downdrafts, still a positive flux). This height value of 0.59 just so happens to be where this is a maximum. The flux is still positive throughout the cloud layer however, meaning that warm moist updrafts and/or cool dry downdrafts are present throughout the cloud layer depth, they are just enhanced (or at a maximum) just above cloud middle. If the flux was negative, warm moist downdrafts and/or cool dry updrafts would be dominant.

COMMENT: 11) Section 4.3: This entire section should be tightened. I get contradictory messages on the role of precipitation. It can both stabilize the BL (cooling near the surface) or destabilize (cooling higher up). I don't have a clear picture of the precipitation/evaporative cooling profile. Line 558, why bring in the skewness with a single sentence? How does it tie into the text above? What do you mean by "the boundary layer has been turned over"? please be more precise.

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 precipi-

tating hours. Hopefully the updated discussion provides a clearer picture of what is occurring. Our results (based on profiles of LHF and SHF) demonstrate evaporation occurring away from cloud base, near z/zi = 0.40 and 0.60 (orange envelopes in Figure 16), leading to the increased turbulent values measured in the sub-cloud.

* We have changed the sentence mentioning the skewness to include the phrase "providing more evidence that" on line 665 of the new manuscript. This is done as to provide more meaning for the skewness in relation to providing further evidence that the boundary layer is decoupled. It is mentioned in previous sections that the skewness is negative for well-mixed boundary layers. We are simply just trying to connect back to this concept. If needed, we can remove this sentence completely and the reader can just refer to the figure.

* The sentence originally containing the phrase "the boundary layer has been turned over" has been removed from the new manuscript. However, we were simply just referring to the fact that turbulent mixing stabilizes the boundary layer (the mixing reduces the instability, or the boundary layer has been overturned meaning the warm air near the surface and cold air in evaporative regions were mixed together).

COMMENT: 12) Conclusions if pressure increased after the passage of the front, why did the BL height increase? The bullets are helpful. The paper would benefit greatly if the conclusions contained more synthesis like this – particularly if focused science questions/hypothesis were addressed. I sincerely hope the authors will focus the revised manuscript around science questions. Lines 610-611: this isn't an interesting result. It's an artifact of the sampling. I don't know why it is in the conclusions. The last lines are so far from the theme of this paper that I wonder why the authors mention these topics.

REPLY:

* A small discussion has been added on lines 699-704 of the new manuscript to address why the BL height increased while pressure (i.e., subsidence) was increasing.

* We have discussed our reasoning in regards to focusing on specific questions/hypothesis in our previous reply towards the beginning of this document. This paper sets out to characterize boundary layer turbulence (there are many papers that simply characterize results from a field campaign, without focusing on specific questions or hypothesis). In regards to the VOCALS dataset, in particular that collected at point Alpha in the twin otter (with an objective to measure turbulence among other things), very little work has been published in regards to the turbulent structure. Most published work revolves around other aircraft and ship based measurements that were made at other sampling regions during VOCALS. For example, Jones et al. (2011) and Bretherton et al. (2010), both which focus on boundary layer structure, decoupling, and precipitation, use data collected from the NSF C-130. Having this characterization of the turbulence in our paper goes a long way toward not only relating to results found in other regions during the campaign, but leads to a better understanding of turbulence on a day to day basis, and what variables can influence it.

* Although not an interesting result, we do believe it is worth mentioning that how one sets out to measure turbulence will ultimately influence the results. Figure 15 is devoted to looking at this through differences when using vertical profiles or horizontal flight legs. Although this is an artifact of the sampling, and the reader can infer that you get vastly different results based on the measurement and averaging methods used, it is still central to the results that are presented and worth mentioning. * The last three lines pertaining to future work have been removed.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-104, 2020.

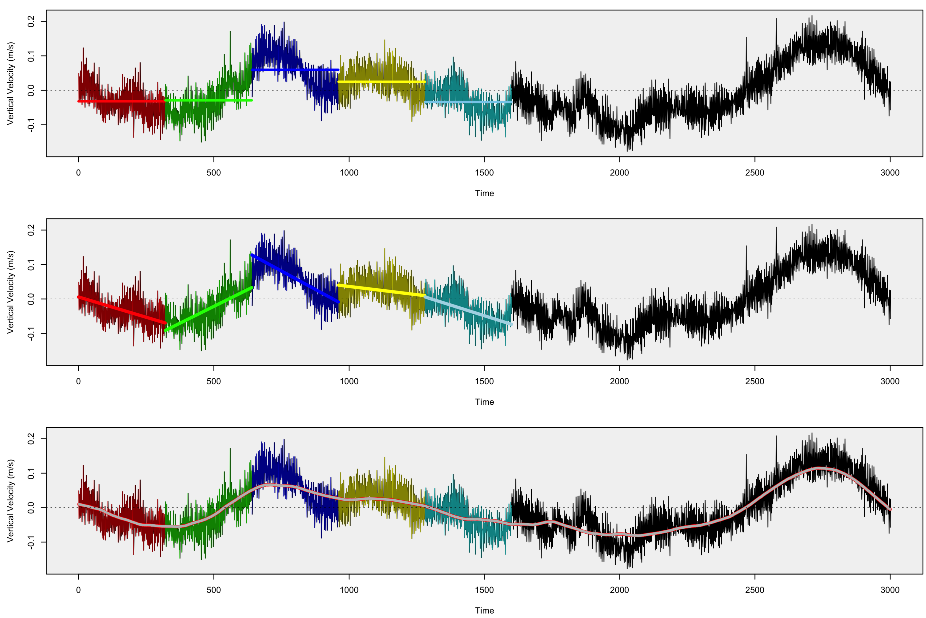
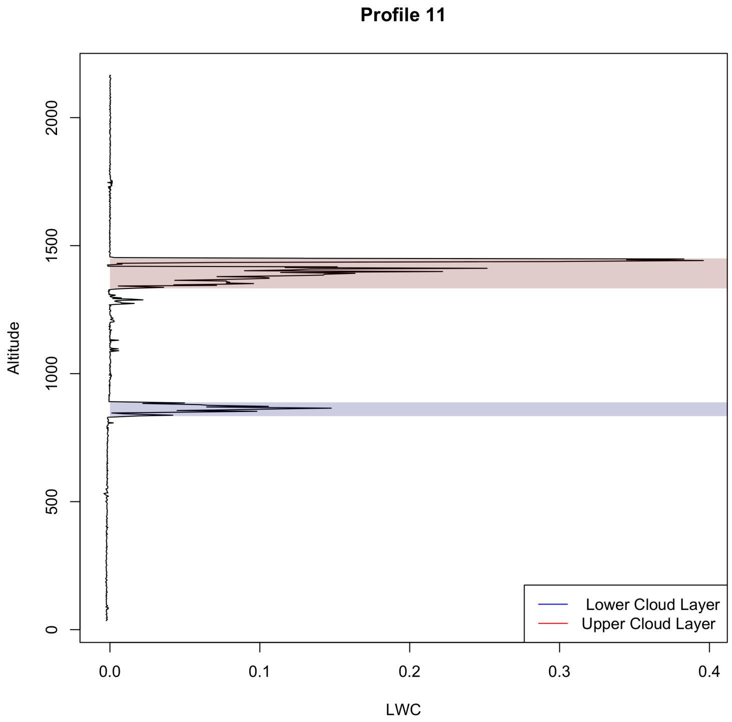


Fig. 1.



Profile 11
Altitude
2000
1500
1000
500
0
0.0
0.1
0.2
0.3
0.4
LWC
Lower Cloud Layer
Upper Cloud Layer

Fig. 2.