Review of ACP-2022-108

Overview:

The authors document 1 year measurements of the coastal ABL at Iquique, Chile, on the eastern border of the extensive SW Pacific stratocumulus region. The measurements include a cloud radar, a microwave radiometer and a Doppler wind lidar. This suite of instruments permits a very complete characterization of the thermodynamic and dynamic evolution of the regional lower troposphere, including the seasonal and diurnal variation, which are the focus of the paper. The text is generally well written and the results represent a valuable contribution to the field. I recommend acceptance after the major and minor comments below are properly addressed.

Recommendation: major revision

Major comments

1. Water loss at the top of the MBL

In several parts of the paper the authors indicate that water of the MBL is lost by evaporation into the free troposphere. I take issue with this concept which I believe is wrong. At the top of the MBL the main process is entrainment. In this process, free-tropospheric air is entrained into the MBL, and the MBL grows through this mechanism. It is true that the MBL dries because of entrainment but this does not represent a net loss of water for the MBL. To support my comment more quantitatively, let me consider 2 of the basic equations that represent a mixed layer evolution:

$$\frac{dq}{dt} = \frac{\overline{w'q'}_o - \overline{w'q'}_H}{H} \qquad (1)$$

$$\overline{w'q'}_H = -(q_+ - q)\frac{dH}{dt} \qquad (2)$$

where q is the MBL water mixing ratio, H is its depth, q_+ is water mixing ratio in the free troposphere and the covariances denote the turbulent water fluxes at the surface (subscript o) and at the MBL top (subscript H). These 2 equations are considered exact in the frame of mixed layer modeling (i.e. they do not include any parameterization assumption). In a typical situation, $q_+ < q$ and the water turbulent flux at the top is positive (eq. 2), which induces a decrease in q (eq. 1). This positive flux at the MBL top is what the authors appear to interpret as a water loss for the MBL. However, the total water content in the MBL is qH, for which we can derive an evolution equation by combining both equations above:

$$\frac{d(qH)}{dt} = \overline{w'q'}_o + \overline{w'q'}_H \left(\frac{q_+}{q - q_+}\right)$$

From this equation it is clear that the entrainment is a source of water to the MBL and not a loss. In 1-D equilibrium, the water loss process should be a precipitation mechanism (drizzle) and eventually subsidence and horizontal divergence.

2. Comparison of measurements with standard measurements at the airport and with M2016

The authors provide a wealth of new data about the MBL at Iquique but make little effort to put their data in connection with the existing operational data at the airport: surface meteorology and cloud base and cloud fraction. They therefore miss an opportunity of validating the airport and their data as well. This is especially needed when they find a substantial difference between LCL and cloud base, which calls for a more critical analysis of the data. The same can be said about the comparisons with the results of Muñoz et al. (2016). I believe the authors can be more explicit and critical in performing this comparison.

3. Description of seasonal and diurnal cycles.

The paper focuses on the diurnal and seasonal cycles of the data. However, I find that their figures representing this variability can perhaps be enhanced. See comments later.

4. Checking averaged evolution against specific cases

The authors present mostly averaged and climatological results with just 1 figure showing a particular case (Figure 2) with too little discussion of their measurements on that day. All their discussion is based on the average conditions. I'd like to see more evidence that individual cases are faithfully represented by the averages. For example, the case of Figure 2 does not show very clear the dissipation by cloud base growth that is present in the average fields.

Detailed comments

L15: Please add a reference to support your first sentence.

L16: Add comma after radiation

L18: equatorward

L21: What do you mean by "stabilized"? some of these mechanisms could produce the dissipation of the clouds (e.g. CTEI).

L24: Parenthesis missing in the references.

L25: The subject of the sentence is "stratocumulus" which is a singular noun, hence the correct verb is "has"

L28: ... and, accordingly, it determines...

L44: Replace "turbulence" by "eddies".

L46: ... entrainment, a too low ...

L64: Eliminate parenthesis around references. Replace ; by and

L73: ... waters, cloud cover...

L79: Latitude and longitude are incorrect

L79: What is DFG?

L101: What do you mean by "larger parts of the coast"? Please rephrase.

L114: Remove extra parenthesis

L134: The authors are too vague in describing the expected performance of the instrument: "to some extent" and "coarse vertical resolution". Please be more specific and quantitative.

L142: How site-specific is the calibration methodology of the MWR? The fact that you use radiosondes far from Iquique to perform the calibration introduces uncertainty in your results? Please comment.

L143-154: I believe this paragraph is better put at the end of section 2.2.2

L163: Please be explicit and quantitative on the vertical resolution attained by the instrument.

L190: It would be nice if you can show the position of these 2 points in Figure 1.

L205: I was confused by the terminology "Boundary layer classification". I was expecting different classes of BL, but even in 1 BL case the scheme classifies different layers of the BL in different categories. Hence I believe a better name would be "turbulent layers classification" or "turbulence classification".

L206: Cloudnet

L208: Only below the cloud? I get the impression you included the cloud layer as well.

L239: Figure 2 includes a panel with the cloudnet classification for the day. However you do not discuss it at all. In particular, it is curious that this specific case does not show the "averaged" behavior of the dissipation. The cloud base does not rise much during the morning and at some time the cloud layer suddenly disappears (see major comment). I'd suggest that for this example day, you add a new figure with measurements of your 3 instruments and the cloudnet classification and expand the analysis and discussion. In this manner, the reader can assess how well the averaged fields presented later represent individual cases.

Figure 2: In the cloudnet panel please mark the times of the satellite pictures. The legend text is not readable.

L240-L260: As the authors indicate, winds in Figure 3 show a marked diurnal cycle and subtle seasonal variation. Therefore, I believe that the representation in Figure 3 could be enhanced. For example, show just 1 diurnal cycle (e.g. SON) and then show the difference of the diurnal cycles between 2 periods (JJA-DJF). This will make clearer the seasonal changes. Alternatively, show the annual diurnal cycle and the monthly variation of the 19-21 UTC and the 06-08 UTC winds corresponding to the extremes of the diurnal cycle.

Figure 3: Please explain somewhere the reason of the reduced number of cases of some of the periods. Indicate if height is ASL or AGL. Also, please indicate whether all data in the figures is the average of the same number of days or the borders have less data. In the latter case, the robustness of the averages in the border should be discussed.

L262: Please indicate how the potential temperature was computed.

Figure 4: Please show the temperature fields instead of the potential temperature fields. The most distinct feature of the thermal structure in the region is the prominent subsidence inversion which is most clear from temperature fields.

Figure 4: The same suggestions for Figure 3 apply here. Please explore ways of showing the seasonal and diurnal variation of the fields more distinctly.

Figure 4: Please indicate number of days used in the averages in this case.

L271: The height of the maximum temperature gradient should be compared with the average of inversion base and inversion top. In this case the comparison with the results of Muñoz et al. 2011 is not so good, especially considering that IQQ is to the north of ANF. Please discuss.

L276: The near surface stability is probably strongly conditioned by the land surface where the measurements are made and therefore they do not necessarily describe the conditions above the sea and the argument about the surface fluxes is weak. Please discuss.

Figure 5: Please express the FOC as percentages in the figure so that the discussion in L280-281 is better followed.

L287-288: Please check whether this feature of the morning dissipation is due to the averaging or indeed happens in most of the days (see comment of Figure 2).

L298: Actually, I think the comparison with Muñoz et al. (2016) is not as good. The authors should make the comparison in more detail and compare also their results with the operational measurements performed at Iquique airport (see major comment).

L305: "water loss by evaporation at cloud-top". The authors must be careful about what they mean with this phrase. Do they refer to liquid water or to total water? In my view, entrainment at the top of the cloud layer does not imply total water loss for the MBL, as the entrained air is intruded into the MBL. Liquid water can be lost due to evaporation increased by the dry air entrained. It is not clear what exactly the authors mean here. Please explain and see also major comment.

L310: It appears that figure 8 is not discussed in the text.

L314-315: The phrase about the uncertainty of the LWP retrieval is not clear. Please provide an explanation of the uncertainty mentioned (20-30 ug/m2). And if this is the case, then figure 9 should in some way convey that huge uncertainty in the plotted values. As it is now, it is

misleading because the uncertainty appears to be described by the 4 retrievals which are almost identical.

L316: It is noteworthy that in JJA the maximum LWP is found very early in the night and values decrease thereafter. Please discuss that.

L319: Eliminate "The height of".

Figure 9. I'd have expected that TH80 retrieval always produced larger LWP than TH95, but this is not the case in the afternoon, especially winter and spring. Can you comment on that?

Figure 9: Please indicate variability and/or uncertainty of these figures.

L331: frequency

L338: I suggest that the authors make an effort to analyze in more detail the drizzle occurrence, considering that they have so much data available. Scatterplots of day-to-day indices of drizzle, cloud depth and LWP are missed. For example, the simplest parametrizations of drizzle in Sc clouds is to define a threshold in maximum LWC above which drizzle begins. As the authors have estimates of LWP and cloud base and top heights, such relationships can be tested. I strongly suggest that they explore such relationships.

L343: See my concern with the "BL classification" terminology.

L361: What do you mean by "counter-intuitive"? I suggest the adjective "contrasting" or be more specific about the intuition behind the comment.

Figure 12: Can you separate your data in Cloudy/Clear nights and see how these diurnal classification changes? I believe that such analysis can provide more physical insights.

L366-367: Again the argument of water loss by evaporation. Liquid water perhaps, but entrainment of tropospheric air into the MBL does not imply water loss for the MBL but the opposite. Please see major comment.

L376: in the cloud layer mixing produces moist-neutral conditions which are different than the dry-neutral conditions in the mixed CBL.

L385: I miss a consideration of subsidence, especially if the authors are trying to describe some type of equilibrium condition. I believe no equilibrium will be attained if subsidence is not included in the analysis, as turbulent entrainment always tends to deepen the MBL. Also, in this section drizzle is not mentioned although it must be important in an equilibrium analysis.

L407: There is ample literature on the relationship between lower-tropospheric stability and Sc-capped MBL, which should be cited.

L410: Again the idea of water loss at the top of the MBL. See major comment.

L412: and again.

L417: and again.

L422: All the analysis in this section endeavors to show a cause-effect relationship between SST, subsidence and clouds. However, as the authors are discussing long-term averages I see a conceptual difficulty in establishing a cause-effect relationship. In my opinion, the most can be said is that there is an association between the SST, subsidence and cloudiness in these averages.

L425: Please see a previous comment on the "dissolution from the bottom" effect in the averages in relationship to the fourth panel of figure 2.

L427: Eliminate extra "in the"

L434: What do you mean by "on the order of the values" ? what values?

L436: Again the concept of water loss to the free troposphere.

L437: Do you mean "winter"?

L440: Please correct the "dew" subindex.

L441: The 400 m difference is huge and deserves more analysis. The 3.2 K surface super adiabatic condition needed to explain the difference conflicts with the cloudy conditions and the overall assumption of the paper that their measurements represent the coastal conditions over the sea surface. The authors should make an effort to compare their results with measurements at the airport (cloud base and surface variables), as well as LCL and cloud bases reported previously by Muñoz et al. (2016).

L456: stratocumulus

L456: An alternative hypothesis for the daytime coastal clearing can be induced subsidence by the strengthening of the westerly component of the coastal wind during the day. I believe the data analysis of the authors is not sufficient to discard this hypothesis which has no need of an upper branch in the circulation. Their measurements of this circulation are only marginal as shown in figure 3.

L462: The term "Rutllant cell" was coined in a paper by Houston.

L462: "cannot be inferred"

L463: Replace "as it is " by "as the latter is"

L466: A scatter plot between LCL and cloud base would be of interest to appreciate their relationship beyond mean conditions.

Figure 15: The quality of this figure is substandard. Some panels show the variability and others do not or do so partially. Ranges of the vertical axes are all different, which makes comparisons difficult.

Figure 16 caption: two dimensional frequency distributions