1 Reply to Reviewer 1

1.1 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

We thank reviewer 1 for carefully reading the text and giving his valuable arguments. We will adress them point by point with reply in italic text, and new or revised text in red *.*

1.2 Major comments

1.2.1 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:

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

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

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 $q \cdot H$, for which we can derive an evolution equation by combining both equations above:

$$\frac{d(qH)}{dt} = \overline{w'q'}_o + \frac{q_+}{q - q_+} \cdot \overline{w'q'}_H \qquad (3)$$

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.

This is of course an idealized model and indeed exact under certain circumstances like a well mixed boundary layer which can be characterized by one single total water mixing ratio q including water vapor and liquid water, an infinitely sharp separation between boundary layer air and free tropospheric air characterized by $\Delta q = q - q_+$, horizontal homogeneity and accordingly no horizontal advection, no subsidence, etc.

Nevertheless with equation 2 the reviewer assumes that the height of the moist layer coincides with height of the BL, and any transport of moisture across BL-top expands the height of the BL (see Fig.1). This model intrinsically assumes that no moist air is lost to the free troposphere, i.e. $\overline{w'q'}_+ = 0$. And of course this assumption results in an increase of column water content qH of the BL when moisture is transported into the region just above the BL. With the puzzling result that for $q_+ = 0$ the entrainment flux does not contribute to the column water content. The reason is that in this case no water from the free troposphere can contribute to the water content of the BL.

In these kind of budget equations for the BL it is usually assumed that the entrainment flux is related by some fraction k to the surface flux (see e.g. Lilly, 1968 or Schubert et al 1979 or Oversloot 2013). A more sophisticated model would have to consider that a detrainment into the free troposphere would increase moisture content in the layer just above H without making it directly a part of the BL. This increase would depend on diffusivity in the free troposhere which would require additionally parameters like wind shear and
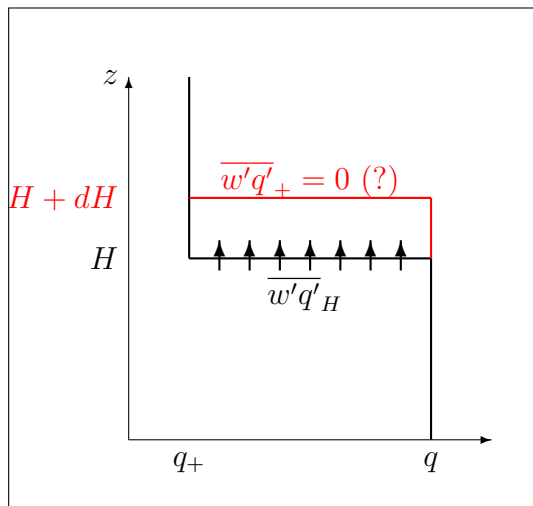
Figure 1: Schematic on equation 2. Black line is the q-profile in a BL with height H. Red lines indicate a moistening of a layer of thickness dH on top of the BL after a time step dt. Black arrows indicate the entrainment of moist air into the dry free troposhere which becomes thus part of the BL.

temperature gradient in the inversion and above which is beyond the scope of this type of models.

Equation 2 might be a good approximation for a convectively growing boundary layer where warm and moist plumes reach up into the free and dry troposphere and thus mediate at the same time a growth of the mixed layer and a moistening of the layer. This equation neglects that with a constant BL height H there might be entrainment of dry air into the BL which must be compensated by detrainment of moist air into the free troposhere. Both processes together give in total a loss of moisture to the free troposhere i.e. a positive $\overline{w'q'}_H$. But this does not necessarily mean that the BL grows. In our data we observe no significant change of BL-height with time of the day (see fig. 4 in the original text). Consequently we believe that there is exchange between the free troposphere and the BL.

CHECK: can we see $\sigma_w \neq 0$ at BL-top in the Wili and/or radar data ? can we include this in the paper ?

For our discussion about the presence of clouds and their relation to the moisture content of the BL it is not the water column content qH of the BL but rather q which defines whether clouds can form and were their base is.

It is true that drizzle, horizontal divergence and subsidence may contribute to the development of the BL and thus also the water content of the BL. In the case of drizzle it changes q only if it reaches the ground as in this case water is remove from the BL. In our data we see frequent drizzle which does not reach the ground as has been already discussed in section 3.6.

Where necessary we changed the text to point to possible mechanisms for drying or moistening the BL.

1.2.2 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 Munoz et al. (2016). I believe the authors can be more explicit and critical in performing this comparison.

We compared our data to the airport data. Whereas we found good agreement within instruments accuracy for air temperature and relative humidity. We realize that ceiling from the airport ceilometer and cloud base from the cloudnet algorithm show some systematic differences.

The comparison between the airport meteorology and our measurements is discussed in the description of the meteorological measurement (Sect. 2.2.4) as follows :

Intercomparison of air temperatures (T_a) from our instruments and the operational airport measurements (DMC, 2022) about 1.5 km to the south, reveals good agreement with differences within ± 0.5 K. An exception from this is summer when temperatures rise above about 22°C and our T_a is on average 0.5^{o}higher. This excess can be explained by the lack of an active ventilation in our measurements. Dew point calculated from our instruments is systematically 0.8 K lower than from the airport data with scatter in the order of 0.3 K. This is equivalent to by 3% too low readings in our relative humidity measurements. Although his is within the range of the instruments accuracy, we corrected this difference.

The discrepancies between LCL and CBH and their relation to the airport data are discussed in detail in Section 4.2.

Intercomparison between LCL and Cloudnet cloud base height (CBH, fig.16d) shows some cases with LCL ¡ CBH, a strong cluster between 500 m and 700 m with LCL$\simeq$ CBH which coincide with daytime heights of LCL. But additionally there are several cases with LCL by up to 200 m hihger than CBH. Especially for CBH below 500 m appears a cluster with CBH by 100 m-200 m lower than LCL. This is not possible in a mixed BL and even in a stratified boundary it would require strong vertical humidity gradients.

The airport operates since 2017 a ceilometer (Vaisala CL31, see DMC 2022, del Rio et al. 2021a). A ceilometer provides 'ceiling', i.e. the largest height from which a pilot could see the earth surface, and especially Vaisala ceilometers do so by estimating visibility for a pilot using several assumptions (Martucci et al. 2010). In contrast hereto the CBH retrieval of Cloudnet (and some other ceilometers) is based on the fact that cloud droplets show strong backscatter. CBH is then identified by a combination of the values of the backscatter coefficient β and the occurrence of a strong vertical gradient in β (For details of the Cloudnet CBH algorithm see Tuononen et al. 2019). As visibility decreases only above cloudbase, ceiling is typically larger than CBH with differences strongly depending on the cloud and cloud droplet properties (Martucci et al. 2010).

We compared CBH from the airport ceilometer with Cloudnet and saw a systematical underestimation by Cloudnet especially during winter and night with average differences around 100 m. Investigation of time series revealed that these large differences coincides with drizzle especially during night and morning hours. On the one hand this might be due to the fact that it is difficult to differentiate between drizzle and cloud droplets in the backscatter signal especially as drizzle originates from within the cloud (Tuononen et al. 2019). As a result CBH may indeed become too low. On the other hand drizzle increases humidity below the cloud and thus may lower cloud base. In this case LCL based on surface measurements is not valid for an estimation of cloudbase any more.

1.2.3   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.

We found the suggestion by the reviewer interesting but had to realize that especially due to the varying height of the boundary layer a normalization of the height coordinate would have been necessary making additional plots and explanations necessary. For a concise description we therefore stay wit the plots as they are (see also replies below).

1.2.4   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 would 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.

We found selected several plots and put them together in one figure. Regarding the current size of the document we decided to put them in the appendix. We added the following sentence to the discussion of Figure 2:

Some sample plots for this day from the data of the three instruments can be found in the appendix.

1.3   Detailed comments

L15: Please add a reference to support your first sentence.
The reference was at the end of the second sentence. We moved it to the first sentence.

L16: Add comma after radiation
done.

L18: equatorward
Corrected.

L21: What do you mean by "stabilized"? some of these mechanisms could produce the dissipation of the clouds (e.g. CTEI).
To clarify we end that sentence with a colon, and added a sentence explaining CTEI and refer to the discussion in Wood (2012).

This system is stabilized by several feedback mechanisms: ... It has been hypothesized that evaporative cooling at cloud top may increase turbulent exchange between the moist boundary layer and the dry, free troposphere and thus dissipate the cloud (cloud top entrainment instability, CTEI). But it has been found that this mechanism requires special conditions and occurs less frequent than originally thought (Wood:2012).

L24: Parenthesis missing in the references.
Corrected.

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

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

L44: Replace "turbulence" by "eddies".
Replaced.

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

L64: Eliminate parenthesis around references. Replace ; by and
We follow here the rules of the publisher Copernicus: "If the author's name is not part of the sentence, name and year are put in parentheses" and "If you refer to multiple references at the same position all references are put in parentheses separated by semicolons". Thus not changed.

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

L79: Latitude and longitude are incorrect
Sorry, they were indeed wrong. Corrected.

L79: What is DFG?
'Deutsche Forschungs Gemeinschaft' = German Research Foundation . Text adapted.

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

This a somewhat unique setting at the coast of northern Chile, as the cliff usually drops from several hundred meters directly into the ocean with only narrow stretches of rocky beaches.

L114: Remove extra parenthesis
Removed.

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.
We removed this vague wording, the sentence is now:

From these brightness temperatures integrated water vapor (IWV), liquid water path (LWP), temperature profiles and water vapor profiles are derived.

In the following paragraph about the temperature profile we added the sentence:

Water vapor profiles are vertically coarsely resolved using MWR measurements, i.e. there are only approximately two independent pieces of information contained in the TBs for water vapor profiling.

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.
The actual absolute calibration of the MWR TBs is performed with liquid nitrogen as mentioned in the first paragraph of 2.2.2. We use a climatology of radiosondes from Antofagasta (about 400 km to the south of Iquique) to derive regression coefficients for multi-variate retrievals of liquid water path, integrated water vapor and temperature and humidity profiles. Since both radiosonde stations are basically located at sea-level, we do not expect any systematic offsets due to differences in pressure broadening of the absorption lines. In addition, the linear relationship between the TBs in the K-band and humidity is largely independent of temperature, respectively between the TBs in the V-band and temperature is largely independent of humidity, so that we do not expect any significant systematic differences when applying the Antofagasta retrieval coefficients to the TBs measured in Iquique.

L143-154: I believe this paragraph is better put at the end of section 2.2.2
We agree, we moved it to the end of this section.

L163: Please be explicit and quantitative on the vertical resolution attained by the instrument.
We adapted the text about vertical resolution:

An intrinsic feature of passive remote sensing is that spatial resolution

decreases with distance. As a result the inversion appears much broader than it is in reality, and it becomes impossible to identify bottom and top of the inversion layer. It will be also difficult to identify a moist adiabatic temperature profile in a cloud layer of a few hundred meters directly below the inversion.

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

Done.

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".

Of course one could discuss the naming of this classification scheme:
Is a convective boundary layer with mixing driven by surface heating a different boundary layer than a stratocumulus capped boundary layer where mixing is driven by processes at its top (both are mixed by large plumes). Or is their turbulence basically different: skewness has a different sign but plumes move only in the opposite direction)?
To avoid this discussion we follow here just the name Manninen et al (2018) gave to this classification scheme.

L206: Cloudnet

Corrected.

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

Yes, only below the cloud as the Doppler lidar cannot see into the cloud.

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.

TO BE DONE

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.

As the height of the detectable range varies with season the central feature of such a presentation is the difference in height. A scaling with the current detectable height range could solve this. But this generates ambiguities as this is either cloud base or top of the aerosol layer and may change from measurement to measurement. Although the idea is interesting we decided not to follow it.

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.

We added a paragraph and a table at the end of the paragraph of the Cloudnet description:

The instruments where equipped with battery backups to ensure controlled shutdown and recover in case of a power shutdown. Nevertheless this did not work in every case for every instrument and lead to several missing days. An instrument failure of the cloud radar lead to no data from end of May to begin of July. And damage of the radar radome forced us to shut down the instrument on January 22, 2019. As the Cloudnet retrieval can only be applied when all instruments provide data this lead to 243 days of data or 72% of the full year. Table 2 shows how many days are per month avaiable. There are few or no days with data in March, June and February and only 57% in January.

L262: Please indicate how the potential temperature was computed.

We added the following text at the begin of section 3.3:

Month	M	A	M	J	J	A	S	O	N	D	J	F
N/days	6	25	27	0	23	29	30	30	29	27	17	0
R/%	19	83	87	0	74	94	100	97	97	87	57	0

Season	MAM	JJA	SON	DJF
N/days	58	52	89	44
R/%	63	57	98	49

We use the Temperature profiles from the microwave radiometer to calculate the potential Temperature with respect to the surface as $\theta = T_{\mathrm{air}} + \Gamma \cdot z$ with $\Gamma = g/c_P$. This is not exact but deviations are below 900 m typically well below 0.1 K.

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.

We show potential temperature to visualize when and where the boundary layer is stable or neutrally stratified. This information would get lost when displaying air temperature. We consider this the more important information and therefore stay with potential temperature. As already described in section 2.2.2. about the microwave radiometer, the resolution of the temperature profile at boundary layer height is coarse and location and extension of the inversion should be regarded with caution. The strength of the temperature profile retrieval lies in the lower two thirds of the boundary layer.

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.

Same argument as above: Our focus is here to show stratification as a function of season and daytime. Temperature differences with respect to one season would make an interpretation in terms of stratification difficult. Additionally the variation of boundary layer height with season would become

a dominating feature in differential fields. That would require additional explanation how to interpret in terms of stratification. An alternative could be to use a normalized height but would require an additional plot with seasonal course of boundary layer height. We considered all these options already internal when developing this analysis and decided that this form of presentation is the best.

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

The analysis is restricted to the same days as the Cloudnet retrieval. We added a respective sentence in the figure caption.:

Figure 4. ... The analysis here is restricted to the same days as the cloudnet retrieval.

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 Munoz et al. 2011 is not so good, especially considering that IQQ is to the north of ANF. Please discuss.

Indeed we falsely compared directly the height of the 'radiometer inversion' with the inversion base height z_B in Muñoz et al 2011. We adapted the interpretation as follows:

Despite the coarse resolution of the MWR this agrees with the annual course of the median inversion base height for the period from 1979-2007 in Antofagasta shown by Muñoz et al. (2011). Nevertheless we have to consider that cloud base, and thus probably also inversion height in Iquique, is on average by about 180 m higher than in Antofagasta (Muñoz et al. 2016). Additionally the inversion height data from the Antofagasta radiosonde (Muñoz et al. 2016) show beside a weak decrease of -16 m per decade, a large year to year variability in the order of ± 100 m. This makes it difficult to put our observations in relation to these long term analyses.

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.

Lobos Roco et al (2018) did not refer to the near surface stability. We clarified this as follows:

Lobos Roco et al. (2018) have shown that during stable stratification between their site at 1100 m ASL and the airport at 53 m ASL, typically no

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clouds are present.

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

We replaced the figure by one which shows the frequency of occurence of clouds detected by Cloudnet with cloud-base <2 km asl during different seasons. We slightly adapted the discussion:

The Cloudnet target classification scheme is used to investigate the frequency of occurrence (FOC) of warm clouds with cloud-base lower than 2 km asl (Fig.5). Most clouds occur in winter and spring, when during night about 90% of the time clouds are present, while during summer and autumn this reduces to 25% and 45%, respectively. During daytime FOC reduces by about 30%-points compared to the night meaning nearly no clouds during summer afternoon and somewhat more clouds in autumn and spring. In winter FOC stays allways above 60%.

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).

We checked that: It occurs indeed on many but not all days. Those days when it does not follow this pattern it seems to be connected to advection (inferred from satellite images) or drizzling clouds. This pattern also agrees with Munoz et al 2016. The very regular diurnal course of the LCL (Fig.16) indicates that this is the mechanism,

L298: Actually, I think the comparison with Munoz 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).

We carafully reanalyzed the Munoz et al. (2016) dataset and revised the text:

Munoz et al.2016 found in a 33 year dataset from Iquqique, centered around 1997, a similar pattern with cloud bases higher during daytime than during night. Nevertheless they found lower night to day amplitudes (50 m-75 m) and also cloud bases which were especially in autumn and winter by about 200 m-300 m higher hen we observed now. This difference can only partly explained by the trend of -100 m/15yr they found. From this comparison it seems as if this trend has accelerated.

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.

We mean here water loss into the free troposphere. We adapted the sentence:

... water loss by detrainment and thus evaporation into the free troposhere at cloud-top ...

L310: It appears that figure 8 is not discussed in the text.
That is true. Given the length of the text we decided to drop it.

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.

We have specified the LWP error discussion. Please note that we are discussing a random uncertainty of individual LWP observations, thus the uncertainties for seasonal means will be significantly lower. We have now added to the text:

The theoretical retrieval uncertainty of LWP is determined to 10 g/m^2. Calibration and absorption model uncertainties can lead to systematic LWP offsets which can mount up to a maximum error of 20-30 g/m^2. These effects have been corrected for by applying a clear-sky offset correction, which is applied to all-sky observations. Thus, we expect a random uncertainty for individual values of not significantly more than 10 g/m^2, which further reduces for seasonal averages by a factor of $1/\sqrt{N}$. I.e. LWP uncertainty is dominated by the uncertainty of the radiosonde data preprocessing.

L316: It is noteworthy that in JJA the maximum LWP is found very early in the night and values decrease thereafter. Please discuss that.
This is indeed an interesting feature. We added the following text:

It is noteworthy that in winter (JJA) the maximum LWP is found in the

L319: Eliminate "The height of".
Done.

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?
The LWP retrieval is based on a multi-variate regression of the 7 K-Band TBs. These channels not only contain information on the LWP, but also water vapor. In TH80 and TH95 clouds are detected under different water vapor conditions. This leads to different regression coefficients and thus to different TB/LWP dependencies.

Figure 9: Please indicate variability and/or uncertainty of these figures.
L331: frequency
Corrected.

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.
These are of course interesting points and they would would give enough material for an independent paper. We included the section about drizzle to show that also this data is available. We keep the suggestion of the reviewer in mind for another publication.

L343: See my concern with the "BL classification" terminology.
As already said above: we stay with the name given by the authors.

L361: What do you mean by "counter-intuitive"? I suggest the adjective "contrasting" or be more specific about the intuition behind the comment.
It seems as if our intuition is different from the one of the reviewer, but maybe that is defined by our home climate zone. We changed the sentence to

:

This seasonal difference in convective turbulence reflects the different stratification during the two seasons.

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.

This is would be a good idea but reviewer 2 already complained that the paper is too long. We keep it in mind for an upcoming analysis.

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.

As already argues entrainment of dry troposheric air into the MBL also include detrainment of moist air into the free troposhere.

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

This is indeed the case, but it does not change the conceptual idea we want to bring forward here. We adapted the sentence to:

Comparable to a CBL, intense turbulent mixing in the SCBL leads to neutral and moist-neutral stratification below the cloud and in the cloud, respectively.

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.

We added a description of the role of drizzle and subsidence:

As long as the SCBL is well mixed and in a stable state, cloud-top energy- and mass-fluxes must be compensated by fluxes at its bottom. If these fluxes are not balanced, the SCBL will change its temperature and/or water content. Radiative emission at cloud top must be balanced by sensible heatflux from the ocean surface. Loss of water vapor by detrainment into the free troposphere must be compensated by evaporation at the ocean surface. Drizzle removes liquid water from the cloud. But as long as it does not reach the surface it does not change the water content of the MBL. A further factor is large scale subsidence. In a CBL warm rising plumes cross the inversion and

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thus lead to a growth of BL height and this growth is reduced by subsidence (see e.g. Oversloot and Vila-Guerau de Arellano, 2013). In a SCBL this role of resistance against subsidence is taken by the entrainment of air from above the BL (Wood, 2012), nevertheless eintrainment rates remain a factor of uncertainty.

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

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

We dropped the sentence.

L412: and again.

We dropped that part of the sentence. The paragraph reads now:

When the MBL has the same temperature as the ocean, plumes from cloud-top can reach the ocean surface and moisture from the ocean is mixed into the boundary layer and the cloud layer. In winter we have accordingly two mechanisms which support a persistent thick stratocumulus cloud deck: coupling of the MBL to the ocean surface and decoupling of the MBL from the free troposhere by a strong inversion. These winter conditions are accompanied by stronger large-scale subsidence caused by the then closer located center of the south-east pacific high pressure system. This subsidence further sharpens the inversion and slightly lowers its height (Fig.4) compared to summer. As a result the winter season coincides with times of most frequent cloud presence (Sect. 3.4, Fig. 6).

L417: and again.

Not changed, as we believe that detrainement exist.

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.

We changed the begin of the last paragraph of this section to:

We believe that our one year data show that . . .

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.

We see that in many cases cloud base increases after sun rise. We therefore see no reason to make here any change.

L427: Eliminate extra "in the"
Removed.

L434: What do you mean by "on the order of the values" ? what values?
We simplified the sentence to:

Nevertheless, day to day variability of IWV at every hour is large and these diurnal variations might be just random.

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

Additionally, it must be noted that IWV is the column integrated water vapor which means that entrainment of dry air from the free troposphere does not change the IWV.

L437: Do you mean "winter"?
Yes, we do. Corrected.

L440: Please correct the "dew" subindex.
Done.

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 Munoz et al. (2016).
We changed the whole paragraph as already written in the reply to the major comment about such a comparison.

L456: stratocumulus
Corrected.

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.

This is an interesting hypothesis. But wind lidar data show in cloud free moments the upper branch of the circulation. And we do not see significant changes of the (MWR-)inversion height, or cloud top height. Nevertheless it is possible that at the western end of the cloud gap subsidence plays a role.

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

... the land-sea-breeze with the 'Rutllant cell' as desribed in Rutllant et al.(2003) ...

L462: "cannot be inferred"
Corrected.

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

L466: A scatter plot between LCL and cloud base would be of interest to appreciate their relationship beyond mean conditions.
We replaced the lower right subplot by a joint pdf of LCL and cloudnet cloud base height, and adapted the discussion as described in the reply to the major comment at the begin.

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.
The range of the figures was indeed too small to show the full range of the standard deviation. We adapted the range of the figures.

Figure 16 caption: two dimensional frequency distributions
Adapted.