Response to Referee #1

The manuscript "Observations of the boundary layer, cloud and aerosol variability in the southeast Pacific coastal marine stratocumulus during VOCALS-REX" documents the structure of the cloud topped boundary layer of the coast of Northern Chile, as depicted by measurements made during VOCALS-REX, satellite data and meteorological reanalysis. Though generally clear and well-written the paper could be improved by addressing a few minor issues before being published in ACP. Apart the specific points noted below, the paper would benefit from an assessment (or at least mention) of the typical biases of the various in-situ measurements that are discussed.

The comments by the reviewer are much appreciated. Brief discussions of the uncertainties in the in-situ measurements have been added in the text.

Abstract

The abstract is a bit too detailed. For e.g. is it really necessary to mention the Nd-CCN relationship here? It could be made more concise by leaving out non-essential information. Also, the last point, i.e. that the LWP does not only depend on CCN but that it also largely depends on meteorology, decoupling should be made more clearly.

The abstract has been revised to convey the most valuable findings. Superfluous information has been deleted.

Introduction

pp 15422 L24: Mention that the aircraft data are the ones collected by CIRPAS Twin Otter.

The text has been changed as suggested.

Section 2

pp 15423 L14: Aren't there 5 hours difference between Chile time and UTC?

The local time in the manuscript has been modified to be the standard local time. The difference is 4 hours between the standard local time at Point Alpha and UTC. During VOCALS-REx, it was daylight saving time in Chile (i.e. 3 hours difference between local time and UTC).

pp 15424 L28: How does QuickSCAT divergence compare to reanalysis?

During VOCALS-REx, the mean NCEP reanalysis divergence at the 1000 hPa level at 20^{o}S, 72^{o}W (i.e. Point Alpha) is -5.5x10^{-6} s^{-1} with a horizontal resolution of 2.5^{o}x2.5^o. The surface divergence from ECMWF Interim reanalysis with a horizontal resolution of 0.7^o x 0.7^o is -1x10^{-6}s^{-1}. It has been addressed in the manuscript.

Section 3

pp 15426 L16: By composite you mean the average or the median of the distribution?

The text has been changed to "the mean 700-hPa geopotential height"

pp 15426 L18: Didn't you mean "the continental cyclone"?

It should be "the continental cyclone". After further study, there was no sign that the continental cyclone had an impact on the study area, so "the continental cyclone" has been removed from the sentence.

pp 15427 L15: What satellite measurements are you referring to? The statement made here about the diurnal cycle is misleading. Normally the maximum of the diurnal cycle of LWP is during nighttime, but as the measurements probably don't cover the night period, the maximum appears at dusk and dawn. Also, why would the narrow band of high LWP be the only part of the region affected by the diurnal cycle?

This statement is from the statistic study of O' Dell et al. (2008). The LWP data set in the reference is gained from SSM/I, TMI/TRMM, and AMSR-E microwave sensors on eight different satellite platforms, beginning in 1988 and continuing through 2005. Their analysis (Fig. 1-2) shows the double peak diurnal cycle off the coast of South America.

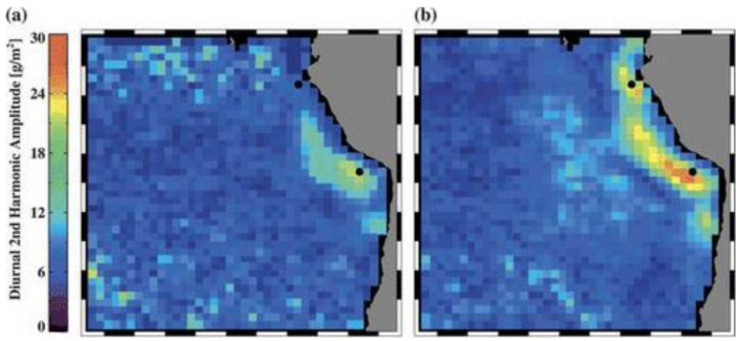

FIG. 1. Map of the amplitude of the second harmonic of the diurnal cycle for a region off the coast of South America for the months of April and October. The plotted region extends from the equator to 40°S, 70°–110°W. (O' Dell et al. 2008, Fig. 11)

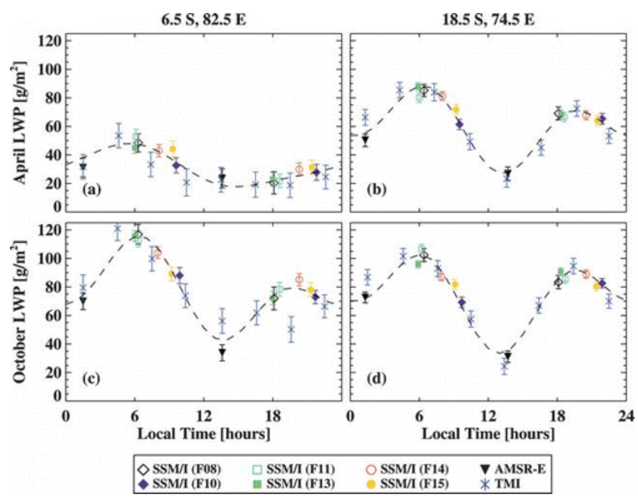

FIG. 2. Diurnal cycle fits for the two small regions off the west coast of South America, depicted by filled black circles in Fig. 11 for the months of (top) April and (bottom) October. The more northern location has a single-peaked diurnal cycle in April but a strong double-peaked cycle in October; the more southern location has a double peak in both months, and in fact throughout the year (not shown). (O' Dell et al. 2008, Fig. 12)

pp 15427 L22: Why are you comparing the LWP of these two points?

The sentences have been revised to "The GOES-10 average cloud LWP over Point Alpha during the observing time periods was about 27 g m^{-2} , and it increased westward to the value of about 43 g m^{-2} around 75^{o} W, 20^{o} S (Fig. 2b)." This paragraph aims to indicate the observed cloud over Point Alpha is thin and polluted compared with the cloud layer west of Point Alpha, which is considered as the offshore Sc layer.

Section 4

pp 15430 L22: Did you mean "combined with the higher 700hPa geopotential height"?

The text has been changed to: "The enhanced southerly wind from Oct. 31 to Nov. 8 combined with the 700-hPa geopotential height minima on Nov.1 and the following increasing …"

pp 15431 Eq1 and 2: How are z_{i-} , z_{i+} defined?

The definitions have been added: "Where z_i^+ / z_i^- is the level 25 m above/below the inversion,…"

pp 15433 L9: The latent heat flux moistens the atmosphere.

The sentence has been corrected.

pp 15433 L20-24. Doesn't this suggestion that in this case the BL is more decoupled than in DYCOMS contradicts your earlier statement that the observed BL is very well mixed except for a few days?

The speculation has been removed since it is invalid in the observed well-mixed BL.

Section 6

pp 15441 L21: "yielding a mean value"

Revised as suggested

pp 15442 L7: Above is mentioned that only 4 days are affected by cirrus clouds.

The sentence has been rewritten to avoid confusion. Now reads "The largest differences occurred on October 24 and 27 and on November 1, 2, 4, and 8. Except for November 1 and 2, the GOES retrievals were contaminated by overlying cirrus clouds on the other four days."

pp 15442 L10-20. This discussion is not clear. The point you are probably trying to make is that in precipitating cases the large droplets sediment and the maximum radius is not at cloud top (the cloud is not adiabatic). But this message is not clearly conveyed.

To clarify the point, we have added the following sentence to the text, "...downward below cloud top because the larger droplets sediment leaves a greater concentration of smaller droplets in the upper portion of the cloud."

pp 15442 L23: What do you mean by the fact that GOES and MODIS agree at the overpass time? Do they have the same overpass time?

The GOES measurements taken at the Terra overpass time agree with the Terra measurements. The text has been changed to emphasize this point: "Although the GOES results at the Terra overpass time essentially agree with the MODIS retrievals,..."

pp 15442 L26: The way this is phrased it sounds like the in-situ measurements of Re are completely reliable. What is the typical measurement error? Even if the measurement error is small, the rest statement in this paragraph is true only for adiabatic, and unbroken clouds.

The in-situ measurements errors have been discussed in the relevant sections. The Re values from the PVM-100 probe is estimated within about 15% in the sample flow. We have rewritten the introductory clause to read, "Despite the bias in the GOES retrievals of Re relative to the in situ measurements, the good..." We also changed the end of the sentence to read,"...of the variation in LWP and Re, at least, for unbroken adiabatic clouds."

pp 15443 L2: use rather algorithms instead of models used in retrievals.

Addressed "...or to biases in the retrieval algorithms."

pp 15443 L15. It is confusing to discuss how the LWP was computed in the middle of this subsection. These two phrases would fit better in line 9, after "in Fig. 16".

This paragraph has been refined to address the issues.

pp 15443 L25: The fact that the LWP is higher on days with higher CCN concentration, does not necessarily mean that the LWP is higher because of higher CCN concentration even for days when the BL is well-mixed. Indeed, very fine variations in the thermodynamical state of the boundary layer (a few tens of degree or g/kg) are enough for doubling the cloud water content or for leading to its complete dissipation. This is also clear from Fig.6. This point should be made more clearly.

The abstract and section 7 have been revised to address this issue.

Figures

Fig. 1: It is hard to distinguish the low and the high pressure systems. Also, the coast line is difficult to separate from the pressure contours, and the diamonds showing Point Alpha are barely visible.

It has been replotted to be a color figure

Fig. 4: The trajectories start or end at 500/2000m?

Corrected

Fig. 5: Reference should be made to the black/red lines.

Addressed

Fig. 8: What do the black points represent?

The caption has been changed to: "Red solid symbols (10 flights) are the typical well-mixed boundary layer, non-drizzling Sc discussed in Zheng et al., 2010, black solid symbols (8 flights) are the cases with complications involving strong wind shear within the BL, moist layers above the inversion, and strong decoupled BL with cumulus below Sc."

Fig. 10: What is the time lag between MODIS overpass and the flights?

The MODIS data are for the overpass time of ~15:00 UTC. Therefore, the overpass was near the end of most of the flights (Table.1 in manuscript). The caption has been changed to: "Magenta symbols are GOES retrievals averaged for each entire flight mission over an area within a radius of 20 km of Point Alpha. Blue symbols are average Terra MODIS retrievals for a 0.5 °x 0.5 °region centered on Point Alpha taken at between 14:20 and 15:50 UTC."

Response to Referee #2

The paper by Zheng et al. entitled "Observations of the boundary layer, cloud and aerosol variability in the southeast Pacific coastal marine stratocumulus during VOCALS-REX" provides a useful characterization of the Sc cloud and aerosol system off the north coast of Chile during VOCALS. This includes assessment of CCN, aerosol microphysics and their relation to the meteorological environment and associated satellite derived properties. The paper provides a valuable summary of observations and descriptive reference for modelers and others interested this cloud system. The paper is well organized and complete but could benefit from some clarification before publication in ACP.

Thanks a lot for the positive assessment and these constructive suggestions. The modifications that have been made to the text are detailed below.

Abstract

I suggest a more abbreviated abstract with rather less emphasis on specific measured values (eg. LWP influenced by occasional moist layers aloft) and greater emphasis on the significant findings (agreement. or disagreement with satellites etc.). Entrainment values relative to offshore VOCALS values are worth mentioning but why a reference to some unstated BL value of the coast of the Northeast Pacific, particularly in the abstract. The significance (presumably the reason it is mentioned in the abstract) of the aerosol and cloud LWP relation is diminished by vague references to other processes influencing LWP. The latter should be addressed in the text unless the former is suspect – in which case it should probably not be in the abstract.

The abstract has been streamlined to focus on the key findings and to make our statements clearer.

Specific Comments

P15425 L28 deviations were less than 15% … Is this a cross wind displacement of 15% of the along wind trajectory distance or something else?

The deviations were less than 15 % of the total trajectory lengths. We deleted this error discussion in the revised version as Referee #3 suggested, because, generally speaking, in stratocumulus regimes it is horizontally homogeneous and vertically well mixed within the BL. Further, we only estimated the trajectories for the first 24-48 hours, thus the deviations of the trajectories should be ignorable.

P15425 L2 "..potential errors over coastal areas..?" Are these potential errors due to unresolved topography, sea-breeze effects or ???

Since we used NCEP reanalysis to calculate the trajectories, previous studies (please see the relevant references in the Introduction) found that the global and regional models meet challenges in the simulations of the coastal area, especially over the study region due to the steep topography of the Andes, which impacts the coastal circulations by both mechanical and thermodynamical means. As a result, we mention the potential errors from the NCEP reanalysis over this study region.

P15428 L4 Which "large area" do you mean or do you mean the entire indicated area?

Modified as suggested

P15428 L23 air masses did not "originate" from these locations but only passed over them. Also, large variations are common for wind direction in the vertical above the inversion on a given profile. Can you say anything about the representativeness of stratifying trajectories by using winds from trajectories at only 2000m altitude? May be worth indicating variability over profiles with a plot to say 3km or so.

It has been changed to "The trajectory histories of the above-inversion air masses". Fig. 3 shows that the middle-level flows (2000-3000 m) above inversion were generally similar.

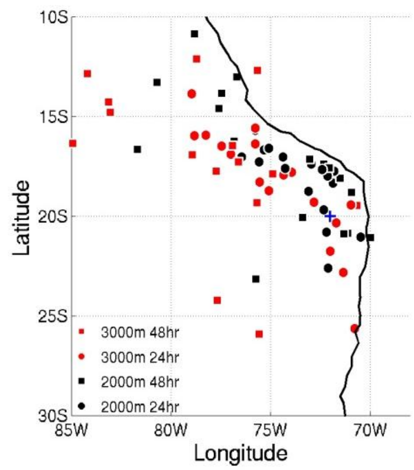

FIG. 3. Backward trajectories starting at 2000 m (Black) and 3000m (Red) from Point Alpha for the eighteen cases with the 24-hour point (Circle), the 48-hour point (Square)

P15433 L20+ Here and elsewhere comparisons to DYCOMS are made but I am failing to see the relevance without further discussion. If these differences are important and affect conclusions then a more complete comparison of experiments seems warranted. If not, then why do we care? Similarly, any significance is undermined in the last sentence (L23-24) that suggests maybe the differences simply reflect measurement and calculation differences.

The text has been revised to make relevance clearer. Further, the new abstract and discussion clarify the motivation for the comparisons with DYCOMS. The offshore stratocumulus during VOCALS-REx puts the observations of the coastal stratocumulus into a better context and allows for the definition of the unique features of the coastal stratocumulus in this region.

P15435 Eq. 5 I would expect the %error to depend upon not just the difference for profile vs. mean values but also the variance observed along the in-cloud leg. If the latter were high and the profile value differed by less than the variance then it seems this expression would not inform you of the uncertainty. There are also issues of cloud structure and scales and sloping profiles flown etc.

The text "The uncertainties associated with the cloud water variances along the in-cloud leg, the sloping profiles flown, etc. are not included in the error formula, which might increase the uncertainty of the LWP estimate in highly inhomogeneous cloud layers." has been added to make this point.

L 12-25 It would be preferable to have these LWP regressions and dependencies mentioned plotted.

Figure 4 plots the GOES and MODIS average LWP for each flight against the average Twin Otter LWP values. Also shown are the GOES averages at the times of the Terra overpasses plotted against the MODIS averages. The GOES LWP values were, on an average flight day, 6.7±18.4 g m^{-2} greater than the in situ values and had a squared correlation coefficient R2 = 0.84 (Fig. 4a). The MODIS average is 6.3±27.4 g m^{-2} less than the in situ values with R2 =0.59 (Fig. 4a). However, the MODIS average LWP is only 0.8+8.0 g m^{-2} less than the GOES averages over Point Alpha at the times of the Terra overpasses for all 17 days. For those data, R2 =0.98 (Fig. 4b).

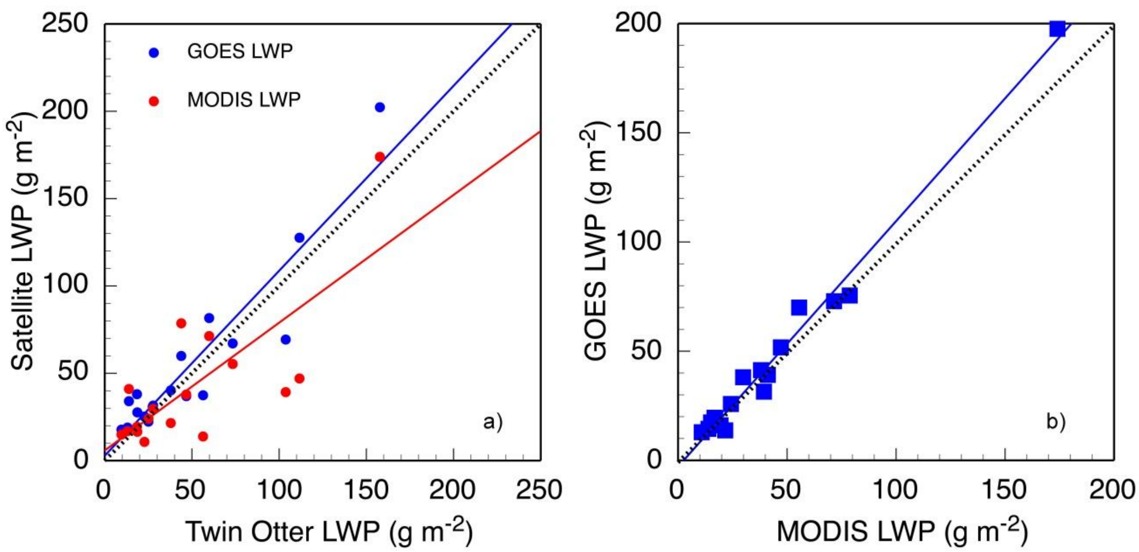

FIG. 4. Scatterplots and regression fits of average LWP for each flight day. (a) satellite averages vs in situ averages for entire flight day and (b) GOES vs MODIS averages at time of Terra overpass. Dotted line indicates line of perfect agreement**.**

P 15436 L1 Fig 7 suggests four flights and about 10 days with moist layer above inversion.

Also, it would be helpful to try and follow differences in LWP discussed if the data in Fig.9 included an indicator of which ones are which (moist layer, decoupled, shear).

The sentence has been modified to focus on the LWP variation rather than distract readers to the moist layer above inversion: "The highest cloud LWP values of 112±33 and 158±41 g m-2, occurred on Nov. 1 and Nov.2, respectively, on which days a moist layer above the inversion moved to this study region (Fig. 7)."

Table 1 included a descriptor for each fight days. After trying to represent different features in Fig.9, it does look busy and distracted. Therefore, we ask the readers to refer to Table 1.

P15439 L15 After suggesting several reasons why entrainment values might be low (an uncertainty?) the last sentence argues that implementing a suggested correction might increase the uncertainty. What are the authors suggesting as most appropriate? BTW - Similar caveats terminate other discussion paragraphs elsewhere and tend to obscure the points raised.

This part and the other discussion paragraphs have been revised to make the points clear.

P15439 L18-25. The authors use the operationally defined Na from the PCASP to characterize the accumulation mode number in both the BL and above the inversion. Most PCASP have a poorly known relative humidity but are generally measuring a wet aerosol in the BL and a dry one aloft. The same aerosol concentration will often be better resolved by the PCASP in the BL than it will be above the inversion due to a smaller dry size there. As size distributions tend to increase steeply near the lower detection limit of the PCASP, the comparison of Na above and below the inversion is problematic and probably biased low aloft. Some compelling arguments are needed here to support this claim.

The potential bias in the aerosol number near the lower end of the PCASP bins due to the different relative humidity in the BL and above the inversion has been mentioned in the context. CCN at 0.2%S results are not affected by the relative humidity, so we compared the PCASP values with CCN at 0.2%S above the inversion and within the BL (Fig. 5). Both have almost the same relationship above the inversion and within the BL, which gives us confidence to assume that the PCASP values are unlikely biased significantly low aloft during these observation.

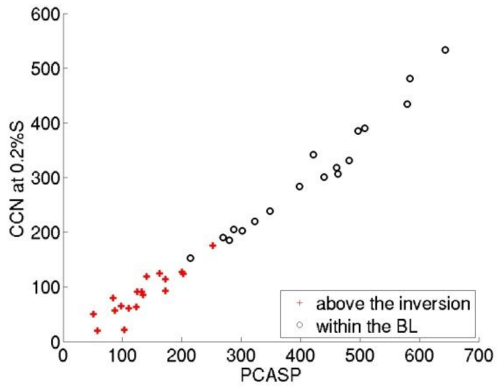

FIG. 5. Average CCN at 0.2%S and PCASP above the inversion and below the cloud base on all flight days

L 25+ The estimation of CCN at 0.2%S from measurements at 0.5%S is problematic. The variability in chemistry and in size-distributions for other VOCALS platforms suggests one should not assume the composition and size distributions vary insignificantly throughout a mission. The variability in aerosol size distributions are generally large and the referenced Allen paper (Fig 12) shows the largest variability for the coastal free troposphere. If there is size data to support this argument then it should be shown. If, not what implications does reasonable uncertainty have for this assessment?

Although the aerosol properties revealed significant variability from day to day over the coastal region during VOCALS-REx, the same study also proved that the diurnal variability of aerosol composition can be neglected (Allen et al. 2011, pp 5245). Since the 0.5%CCN on the first four days are lower than the PCASP aerosol number concentrations (Modified manuscript, Fig.13), we assume the PCASP aerosol spectrum covered the aerosols that activated at 0.5% supersaturation. The PCASP aerosol number size distributions on these days, shown in Fig. 6 (below) indicate that the aerosol composition variability is within about 15%. We also calculated the 0.2% CCN by Twomey's formula (Pruppacher and Klett, 1997): $CCN = C \times SS^k$, where SS is the superstation, and parameters C and k for each day are derived from 0.5% CCN data. Fig. 7 (below) shows that the derived values are very close to the linearly adjusted 0.2%CCN values adopted in the manuscript. In addition, the variations involved in the parameters C and k, shown as error bars in Fig. 7, are within the variations shown in the manuscript (Modified manuscript, Fig. 15). This gives us the confidence that the uncertainties resulted from the linear adjustment are within the standard deviations of the average CCN and the measurement uncertainties (estimated as about 15% in the sample flow).

To provide readers a better sense of the magnitude of the correction, the average 0.5% CCN values for the first four days have been added into the related figures.

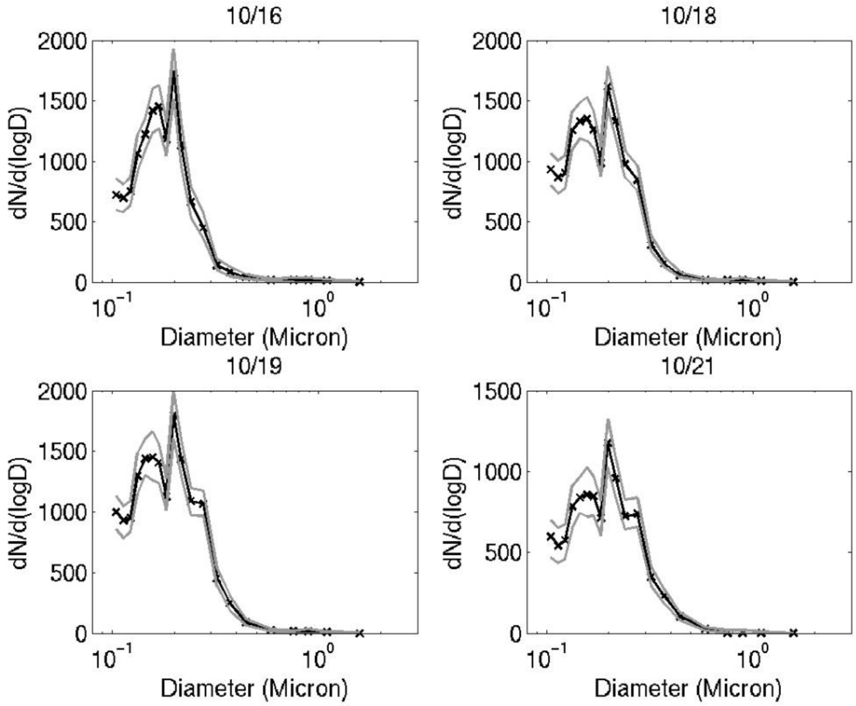

FIG. 6. PCASP aerosol number size distributions (/cm^{-3}) below the cloud base on the first four flight days (Oct. 16, Oct. 18, Oct. 19, and Oct. 21). The black line is the median aerosol number spectra and the gray lines indicate the interquartile range.

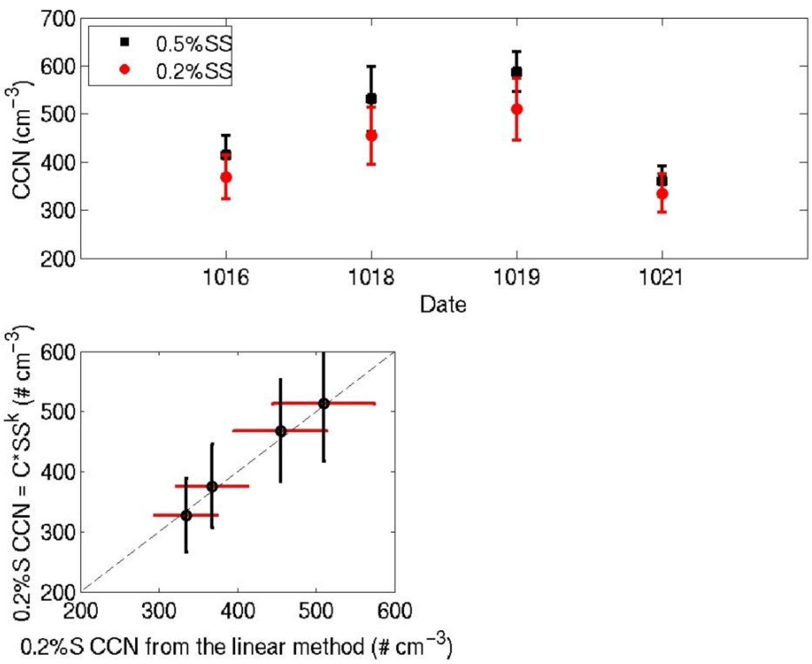

FIG. 7. (Top) the average 0.5% CCN number concentration compared with the linearly adjusted 0.2% CCN on the first four flight days. The black error bars are the standard deviations of the average 0.5% CCN, the red error bars are the standard deviations of the average differences between the 0.5% CCN and the 0.2% CCN. (Bottom) the 0.2% CCN derived from the linear method compared with the 0.2% CCN derived from

Twomey's formula. The black error bars are estimated from the variations of the parameters C and k, the red error bars are the same as these in the top panel.

P15440 …"The main aerosol source at Point Alpha was horizontal advection within the BL from the south rather than entrained from above the inversion…." Entrainment at point alpha will have little influence on the Na in the BL at point alpha. It will influence values further downwind (day or so) depending upon entrainment rate. The question is whether the Na advected to point alpha in the BL was influenced by previous entrainment of aerosol from above or directly by coastal emissions into the BL.

The sentence has been revised to address the issue, "…The main aerosol source at Point Alpha was horizontal advection within the BL from the south, part of which might be entrained from above further downwind days ago, rather than be entrained from above the inversion…."

P15442 L19-18 seems to contradict L 3-5.

The sentence has been revised to to clarify that this argument is for the clouds without drizzle sedimentation, "Since for the non-drizzling Sc cloud, the cloud droplet size increases with height and the aircraft measurement was about 50 m lower than the cloud top, this may explain part of the bias between the satellite retrieval and aircraft measurement."

P15443 L10 "take into account" should probably read "to be taken into account"

Corrected

Figure 6e. I suggest replotting with wind direction to say 0-400? In order to minimize the jumping from 360 to 0 degrees

Figure 6e has been replotted as suggested.

Response to Referee #3

This study documents the state of the stratus-topped boundary layer (STBL) near the coast of Argentina/Chile from _1 month of aircraft, radiosonde, and reanalysis data during the VOCALS campaign. It is a useful contribution to the literature because nearcoastal conditions in the SE Pacific haven't been studied in detail before. However, the paper reads more like a catalog than an attempt to answer any particular scientific question - which greatly reduces its impact. The text is generally clear and well-written with the exception of the points noted below.

The constructive comments made by Referee #3 have been very useful to improve the paper. The revises have been done and the reasons why we chose not to follow some suggestions are detailed below.

Major suggestions:

1. The paper currently reads like a disconnected set of BL properties. I think it would be much better to give us a description of the atmospheric state for various circumstances.

For example, understanding that the synoptic disturbance on Nov 1-2 brings warm, moist, and high-aerosol free tropospheric air which causes the BL to decouple is much more useful than knowing that sometimes in the month each of these individual conditions held. Reorganizing the sections based on regime or defining several regimes at the start and tracing regime properties throughout the existing sections would improve things hugely.

The well-mixed boundary layer with a sharp temperature inversion and associated hydro-lapse was the most common structure observed during the ten flights. Thus these simple cases provide the best conditions for exploring aerosol-cloud interactions and are the focus of this study. The time series presentation is used to show how the principal large-scale forcing parameters vary during the course of the observations. Although the synoptic disturbance on Nov 1-2 is interesting, it is a very difficult case to untangle, since the boundary layer and the conditions above it appear to be modulated by meso-scale variability due to coastal effects and not just a simple synoptic scale forcing. The observations available are inadequate to fully study the causes of the observed perturbation from the mixed layer state.

The primary purpose of this manuscript is to document the whole dataset obtained over Point Alpha during VOCALS-REx, which is necessary for all purposes of future numerical study including to explore the processes causing the air above the inversion is more moist than that over the offshore region, to explain the unusual vertical wind shear within the BL, to untangle these decoupled cases, etc. The introduction and conclusion have been rewritten to make the purpose of this work more explicit.

For the specific purpose of this manuscript, we finally decide to keep the structure as it is. As an alternative, we revised the abstract and summary to meet Referee's suggestion.

2. Temporal and spatial variability of quantities (e.g. synoptic situation) are put in separate sections. This again prevents us from understanding the situation in a holistic way and leads to some redundancy (e.g end of section 3.1 says what is shown in sect 3.4.

The relevant paragraphs have been refined to improve the overall presentation and to avoid overlapping.

3. You could improve the impact of your paper by using your obs to test whether reanalyses are doing a good job.

We do agree that the evaluations of the regional and global model simulations over this study region are very meaningful. Actually, another paper (Wang et al, 2011; please see references in this manuscript) published on ACP used some of the Twin Otter observations to evaluate a regional model simulations. However, the motivation of this manuscript is to document the unique features of the coastal BL, stratocumulus cloud and aerosol variations near the coast of

northern Chile, which has never been done before and needed to be done. The observations are available to the modeling community to aid in further comparisons with model simulations.

Minor Comments:

1. You may want to include "near-coastal" in your title since this is what differentiates your results from previous work.

The title has been modified as suggested

2. The NCEP-NCAR reanalysis is really old Why didn't you use something more modern? More generally, there are few observations in this region to constrain reanalysis, so it would probably be good to compare results from several reanalysis products to ensure you're getting the right picture. You could include these results as a comment response and put a sentence in your paper saying your result is unchanged by changing the reanalysis product.

The average sea level pressure (SLP) and 700-hPa geopotential height from ECMWF Interim reanalysis are shown in Fig. 8 (top). The comparisons of SLP and 700-hPa geopotential height at Point Alpha between NCEP and ECMWF reanalysis (Fig. 8, bottom) indicate that the variables in both products varied within the same range and shared similar tendencies such as the weakening of SLP near the end of the observation, the 700-hPha geo height minima on Nov. 1 and the following increasing, etc.

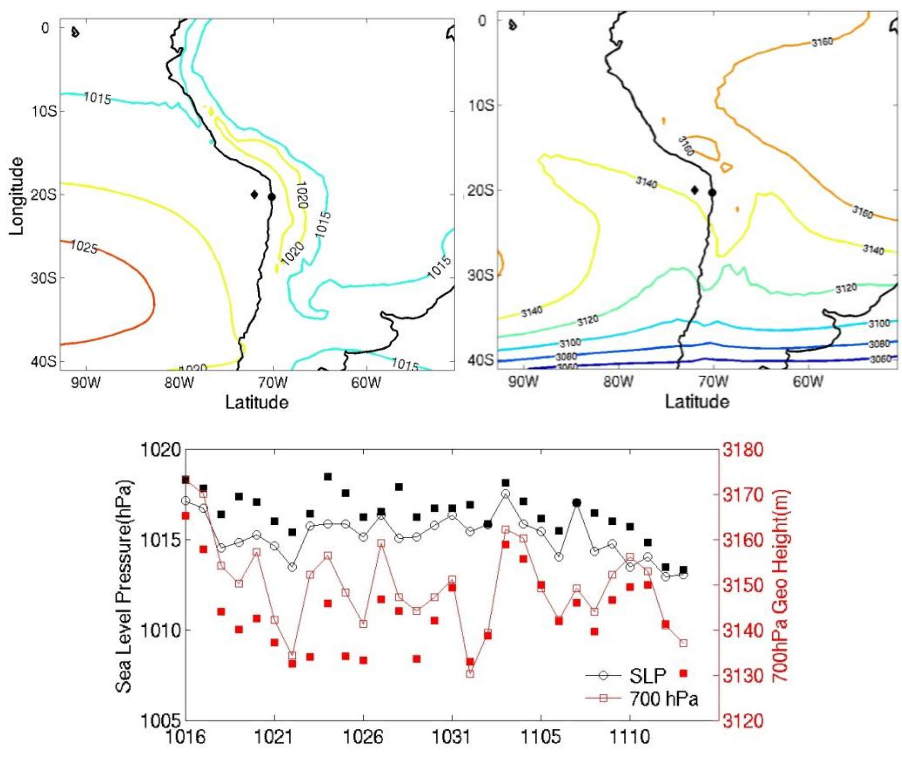

FIG. 8. (Top left) Composite SLP (Oct. 16 – Nov. 13, 2008). The contours are every 5 hPa ; (top right) composite 700-hPa geopotential height; (bottom) Synthesis of sea level pressure and 700-hPa geopotential height at Point Alpha from NCEP NNRP reanalysis (12:00 UTC, open symbols) and ECMWF Interim reanalysis (12:00 UTC, solid symbols).

3. Abstract says conditions were well-mixed, but you say later that this isn't always the case.

Abstract has been refined to clarify the issue.

4. p. 15427 L 10 "Re TO a horizontal" not AT.

Corrected

5. p. 15428 top paragraph: I'm confused about the timing of the various data sets. If I understand correctly, flights took place in local morning and you used radiosonde data for the morning, but satellite data for all times of day? You should at least put in a few sentences clarifying this and optimally you should only use data for the morning time in all of your analysis.

We only used data for the morning time except QuikSCAT surface winds. The text has been added to clarify this issue: "Because QuikSCAT winds over this study area were available two passes per day and the available time was random, it is impossible to estimate the surface divergence field during the aircraft mission time. Therefore, we averaged all 58 QuikSCAT surface wind passes over this study area from Oct.16 to Nov. 13, 2008 to estimate the climatological features of the surface wind and divergence fields during VOCALS_REx."

6. p. 15428 L 22 "from broader regions" -> "and came from a wider variety of directions".

Modified

7. Sect 2.3: Can you say that relatively homogeneous conditions in Sc make HYSPLIT accuracy less important? In any case, I think you only use HYSPLIT data back to 48 hrs, so I don't think the concern in L 25 is relevant.

The discussion has been deleted as suggested

8. p 15426 L 20: Shouldn't the subtropical high be building in austral spring? I'm confused about this and the next sentence, which seem to give contradictory explanations.

This could be the subseasonal variability of the subtropical high. Fig.9 (a-b) shows SLP from Oct. 16 - 29 and from Oct. 30 to Nov. 13, 2008. The subtropical high weakened and withdrew westward at the second half of the observation period. Further, compared with that at the beginning of the project, the sea level pressure over the SE Pacific ocean reduced at the end of the project associated with the mid-latitude trough event occurring at the end of the project (Nov. 13, 2008, Fig. 9 c-d). The statistical study in George and Wood (2010) also found that the sub-seasonal variability in SLP is dominated by a strength variance in periods of 10-20 days and a location variance in periods about 10 days.

Although we used NCEP reanalysis in this manuscript, ECMWF Interim reanalysis (Fig. 9) showed the same variability as well. Here we present the results from ECMWF reanalysis as a supplement answer to the comment #2.

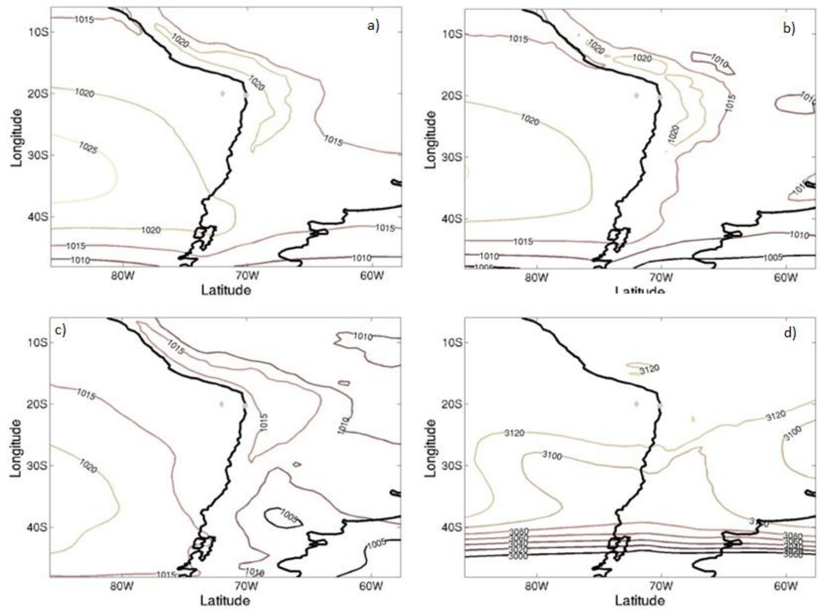

FIG. 9. a) Average SLP (Oct. 16-29, 2008); b) average SLP (Oct. 30 - Nov. 13, 2008); c) SLP on Nov. 13, 2008 12:00UTC; d) 700 hPa geopotential height on No. 13, 2008 12:00 UTC from ECMWF Interim reanalysis

9. Sect 3.4: SLP is calculated from 30m legs... is this SLP or pressure at 30m?

Revised text: "The sea level pressure was also calculated from the aircraft 30-m level observations of pressure and virtual temperature on corresponding days; …"

10. Sect 4: you present vertical profiles from the aircraft as if the cross-section has no horizontal component. How far is the aircraft traveling in the horizontal as it does these profiles? Do you think this will affect the results?

The aircraft made slantwise soundings on most flight days. The horizontal deviation between the end point and the start point of the sounding was about 35-50km. This deviation is usually within the solid cloud area, which was expected to be horizontal homogeneous. Further, the soundings consistent with the average horizontal-legs values on most flight days (e.g. Fig. 10, case was chosen randomly, and Fig. 11) give us confidence that the slantwise sampling does not affect the results.

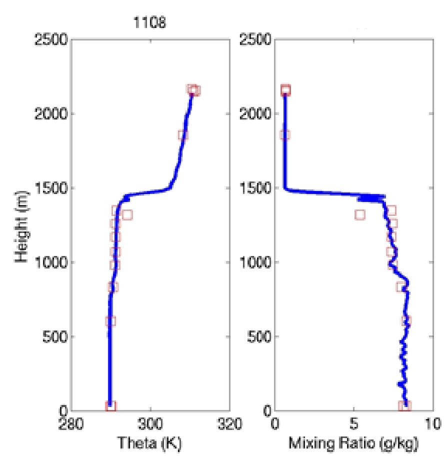

FIG. 10. The soundings of potential temperature and mixing ratio on Nov. 8. The average values from the horizontal legs are denoted as red squares

11. Fig. 7: there's an interesting high-frequency temporal oscillation in these plots. Is this the diurnal cycle?

The soundings were launched at Iquique located at the coastline of northern Chile west of Point Alpha. It indicates the diurnal cycle of the continental BL.

12. p 15431 L8: decoupling is different than breakup. What exactly do you mean here?

The text has been rewritten as: "suggest that if αq exceeds a certain value (~ 0.3), which indicates that the upper part of the BL is clearly drier than the lower BL, the BL is decoupled. Thus the Sc layer may break up and transit into trade wind cumulus."

13. p 15431: you list 3 decoupled days on line 12, then later talk about how Nov 1-2 are also decoupled. Confusing.

The text has been revised to clarify the confusing point: "…, except for Oct. 29, Nov.1-2, Nov. 4 and Nov. 8. On Oct. 29, Nov.4, and Nov. 8, the BL was decoupled and small cumulus developed below the Sc layer as indicated by the LWC profiles and the flight reports. Because of the synoptic forcing (Nov. 1 and Nov. 2, Fig. 6.), the air above the inversion was no longer dry. …"

14. Sect 4.2: you mention in-BL wind shear for 24th Oct and 4th Nov here, but in other places in the text you also mention Oct 27th as having shear. Also, I'm a bit confused how the Oct 24 and 27th BL can be well mixed yet support strong wind shear. Wouldn't momentum mixing quickly destroy this structure? Are you sure you're not mis-sampling the BL top here? Does the cloud structure consist of shallow Cu here? This is a good example of a case where fully describing the BL structure for a regime would be useful.

On Oct. 27 there was a strong wind shear above the inversion instead of within the BL, and it might cause an entrainment zone thicker than 100 m. A clear in-BL wind shear on Oct. 24(Fig. 11) was captured by two soundings and several horizontal legs. Meanwhile, the thermodynamic variables were still well mixed shown in both soundings and horizontal legs. The cloud layer that might destroyed by the wind shear, was thin and broken without shallow Cu according to the flight record. As mentioned in the manuscript, SST on Oct. 24 was about 1^{o}C higher than the observed SST trend (Manuscript Fig. 12a), which could impact on the cloud layer as well.

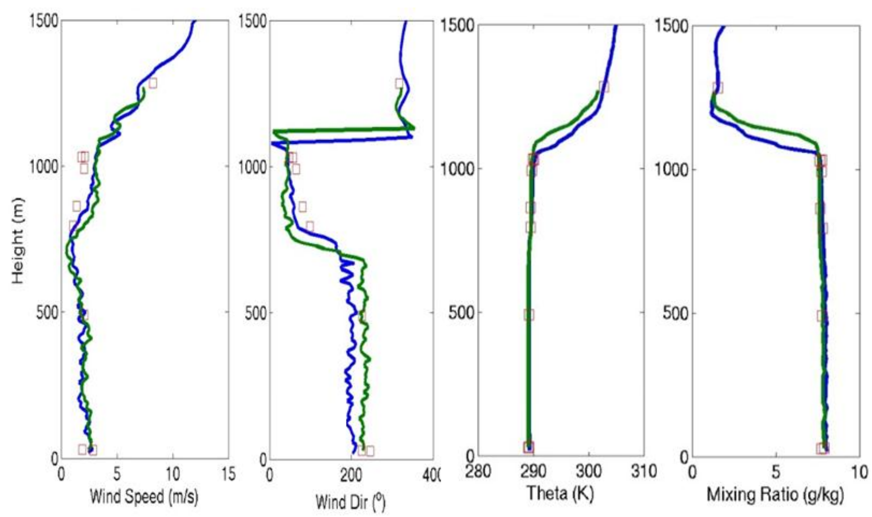

FIG. 11. The soundings of wind speed, wind direction, potential temperature and mixing ratio on Oct. 24 The average values from the horizontal legs are denoted as red squares

15. p 15432, L22: "the *z_i-*normalized cloud base"

The definitions of z_i^+ and z_i^- have been added in the text: "Where z_i^+ / z_i^- is the level of 25 m above/below the inversion,…"

16. p. 15433: I'm confused what assumptions you make to get the values on line 4.

I assume it is a horizontally homogeneous well-mixed BL with a typical depth of 1000m as observed.

$$\left.\frac{\partial\theta}{\partial t}\right|_s = \frac{\overline{w'\theta'}|_s}{H}$$

$$\left.\frac{\partial q}{\partial t}\right|_s = \frac{\overline{w'q'}|_s}{H}$$

where the left hand side of Eqs are the time tendency of the BL temperature (Θ) and water mixing ratio (q) due to the surface fluxes, H is the BL depth, $\overline{w'\theta'}|_s$ and $\overline{w'q'}|_s$ are estimated from the surface sensible heat flux (~10 W m-2) and latent heat flux (~50 W m^{-2}).

17. Sect 4.3, end: Your argument that w'w' is weaker here than in nocturnal studies because the BL is decoupled during the day doesn't make sense in conjunction with your assertion that the BL is well-mixed.

The text has been deleted because the speculation is invalid considering the observed well-mixed BL.

18. p. 15434 L7-10: the wording of your entrainment zone explanation could use work.

Superfluous words deleted, and the wording has been improved to: "The entrainment zone starts at the level where the soundings clearly begin to transit to the free troposphere, and ends at the level where these soundings totally lose the BL features."

19. p. 15434, bottom: steadiness of BL depth at pt Alpha could be due to anti-phasing between solar heading and diurnal cycle of subsidence rather than weak coastal influence.

The mentioned sentence has been deleted due to a lack of convincing evidence.

20. p. 15435 L 12 (and other places): be consistent in using local time or UTC (I prefer local time).

Revised as suggested

21. p. 15437, L 5: I think you mean seasonal rather than annual cycle. Also, getting the standard deviation of monthly anomalies would in addition to the typical change across that month would put the SST jump in better context.

The standard deviation has been added as: "The seasonal cycle of the monthly SST increases from October to March. The climatological SST increase was about 1.1±0.4oC from October to December at Point Alpha, …"

22. eq 8-10: Aren't eq 8 and 9 essentially equivalent to eq 3 and 4? Shouldn't you be able to read the resulting info off Fig. 8? I see this section as redundant. I also don't think the 30m buoyancy flux is particularly interesting since moisture doesn't have a big effect on buoyancy flux (as you show) in unsaturated conditions.

Eq 8-9, and Fig. 13 have been deleted.

23. p. 15438 L 14: 1-2 Nov is 2 days, not 3.

Corrected

24. p. 15438 L 14: I don't see why a negative moisture jump invalidates the flux-jump calculation of entrainment.

Theoretically, the air aloft is wetter than that within BL, the total water flux at inversion should be negative (turbulence brings water from above in the BL), but the total water fluxes calculated from near-inversion legs on these days are positive, so we will get a negative entrainment rate on these days, which is physically unrealistic. Therefore, we decided not to include these entrainment estimates.

25. p. 15438 L 20-21: You should not include data points where you expect the method to fail. Along these lines - shouldn't you not use flight legs that are 80m below the inversion? On a similar note - don't use the cirrus corrupted GOES data (p. 15442, L 8).

The related places have been modified to address this issue. The cirrius corrupted GOES data have been removed. We have extrapolated the entrainment rates to the inversion with the similar processes adopted by the observational study of Gerber et al. (2005), and estimated the uncertainties as well.

26. p. 15439, L 19: why the exclusion of cases with Nd<15cm^{-3}? This arbitrary choice will bias the mean Nd results.

The average N_d was recalculated without the exclusion of samples with N_d < 15/cc. The new average N_d values are very close to the previous values with a difference less than 5/cc (<3/cc on most flight days). Therefore, we believe the exclusion does not make bias the mean N_d substantially.

27. p. 15440, L 6: your correction method for converting 0.5% supersaturation to 0.2% is not well explained. What was the correlation between 0.2% and 0.5% values when both were available? What is the magnitude of the correction you apply?

The same question has been answered and addressed in Referee #2's comments.

28. p. 15440, L10: How do you distinguish between drizzle water content and cloud water content?

The text has been revised as ", when the mean in-cloud drizzle water contents observed by the CIP probe were 0.0490 g m^{-3} and 0.0075 g m^{-3} respectively." to explain the drizzle water content was measured by the CIP probe which detects drizzle droplet size ranging from 25 to 1500 micron, while the cloud water content was measured by the PVM-100 probe.

29. p. 15441, top: I don't understand this argument.

This argument seems irrelevant and has been removed.

30. p. 15441, L 13: how did you choose the functional form for your Nd equation?

This functional form has been commonly used to estimate how sensitive the cloud droplet number concentration could be to the change of the aerosol number concentration (e.g. Twomey 1977, Feingold et al 2001).

31. p. 15444, last sentence: weak w'w' contributing to weak entrainment is a good point that you should mention in sect 5.4.

Revised as suggested

32. p. 15445, L 20-23: I don't understand this sentence.

This sentence has been rewritten as: "these aircraft observations lasted no more than five hours on each flight days and the time gap between two flights were mostly longer than 24 hours on most days. This limited us to capture the continuous evolution of the cloud layer and the processes affecting the boundary layer structure and clouds at the time scales between five hours to days, which could also be critical for the cloud LWP variation (e.g. Albrecht et al., 1995b; Sandu et al., 2008)."

33. Fig. 1: I don't understand what the shaded region means.

The text "The Andes region is shaded to avoid the artificially high sea level pressure region due to the high altitude" has been added in the caption.

34. Fig. 8: What are the black dots?

The caption has been changed to: "Red solid symbols (10 flights) are the typical well-mixed boundary layer, non-drizzling Sc discussed in Zheng et al., 2010, black solid symbols (8 flights) are the cases with complications involving strong wind shear within the BL, moist layers above the inversion, and strong decoupled BL with cumulus below Sc."

35. Fig. 10: How did you get LWP error bars?

The caption has been modified to: "The estimated errors in LWP from Eq. 5 are indicated by the error bars."