Reviewer 1

Summary

This work examines surface-based observations at the Eastern North Atlantic (ENA) site on Graciosa Island in order to characterize the seasonal cycle and budget of boundary layer moisture. The manuscript is well-written and the authors have plenty of history and skill in working with these observations but I struggle to understand some of the important decisions that are made in the analysis process. My main concerns are related to the fundamental assumption of a well-mixed boundary layer at ENA and the severe limitations in selecting data to include only the extremes of a particular weather regime while also claiming to present an encompassing depiction of moisture variability at ENA. Both of these concerns are explained in my General Comments.

We thank the reviewer for the constructive comments that have led to changes and substantial improvements to the manuscript. Please find below our point-by-point responses to the comments. The comments are in black, and our responses to it are in green. Any changes to the article text are mentioned in blue.

General Comments

1. The authors assume a well-mixed boundary layer and justify their assumption by citing Albright et al. (2022) but the Albright paper was focused on the EUREC4A boundary layer, which is much closer to the Equator and actually located within the classical trade wind region. The ENA site often experiences decoupled boundary layer conditions and mid-latitude cyclone disturbances, invalidating this assumption that might be important for the conclusions presented in this work. The authors acknowledge that the assumption is potentially inaccurate but useful (lines 60-61). This manuscript should make more of an effort to justify this assumption. For example, this validation could take the form of showing that the decoupling index from ENA sondes is similar to EUREC4A or is generally low. Sonde profiles could also be shown, similar to Figure 2 in Albright et al. (2022). This could be included in the supplemental material, as the validation of the mixed-layer model for ENA is not really the focus of the manuscript.

   Albright, A. L., S. Bony, B. Stevens, R. Vogel, 2022: Observed sub-cloud layer moisture and heat budgets in the trades. Journal of the Atmospheric Sciences, DOI 10.1175/JAS-D-21-0337.1

   We thank both reviewers for bringing up this important point. The assumption of a well-mixed boundary layer is important to define the framework over which to study processes that affect clouds and moisture. The framework has been used for multiple decades now to understand the primary controls on the boundary layer vapor and (cloud) liquid field (e.g. Betts 1976 JAS for cumulus; and Brost et al., 1984 for stratocumulus). In our case, because we are using all data, there will be a mix of conditions, similar to the profiles shown by Lock et al. (2000 MWR). In theory the budget terms could be modified to include multiple mixed layers, or a mixed layer on top of a stable layer, or vice-versa. However, it is not possible to implement such a framework to the large data (~7 years) used in this study. It should be also noted that decoupling indices are usually calculated from radiosonde profiles, and at the ENA site we only have two radiosondes (00 and 12 UTC) per day, making it difficult to calculate budgets of a thermodynamically decoupled boundary layer. This is primarily because any stable or mixed layers above the surface layers are transient and do not persist for more than 6 hours. However, the boundary layer inversion is omnipresent as it is primarily controlled by large scale subsidence. Lastly, in a decoupled boundary layer the surface moistening and the entrainment drying are mediated by boundary fluxes through another layer. Hence assuming a decoupled boundary layer is well-mixed will essentially cause an imbalance in the equation 1, thereby yielding a residual term. However, the seasonal and annual average residual term (Table 2) are far smaller than any of the calculated terms. This suggests that although the boundary layer may not be well-mixed, it can be assumed to be well-mixed at seasonal to annual timescales.

To address the reviewers concern in more detail we have therefore examined all radiosondes and radiosondes during marine conditions and calculated a Decoupling Index (DI) defined as:

$DI = (Z_{CB} - Z_{LCL})/Z_{CB}$

Profiles with DI < 0.25 are classified as strongly coupled and profiles with 0.25 < DI < 0.4 as weakly coupled. With this definition more than half of marine profiles are strongly coupled and about 68% of the marine cases have DI < 0.4. Marine cases with low cloud base height (< 1.2 km) are statistically more coupled with 64% having DI < 0.25 and 80% having DI < 0.4.

	All	Marine	Marine with CB<1.2 km
Number of cloudy cases	1825	681	469
DI<0.25	739 (40%)	351 (52%)	301 (64%)
DI<0.4	1205 (66%)	461 (68%)	374 (80%)

Selecting marine cases increases the percentage of coupled and weakly coupled cases compared to the entire dataset. The annual cycle of DI for the marine cases, shown in Fig. R1, shows higher variability in the summer months.

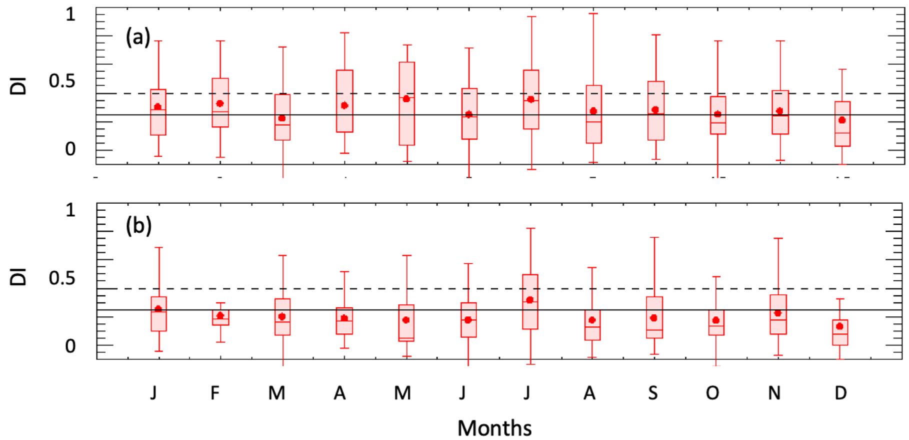

Fig. R1: Annual cycle of the decoupling index (DI) for (a) all marine cases and (b) marine cases with cloud base < 1.2 km. The solid line indicates the 0.25 threshold (strongly coupled) and the dashed line indicates the 0.4 threshold.

To evaluate whether the inclusion of decoupled cases affects the moisture budget, we repeated the calculations of eq. 1 including only cases with cloud base height < 1.2 km. This is shown in Fig. R2. When compared with the new Fig. 7b, the main features of the results are not affected. However, the inclusion of decoupled profiles in the analysis (Fig. 7b) enhances the contribution of the entrainment fluxes to the total moisture sink from ~18% to ~25%.

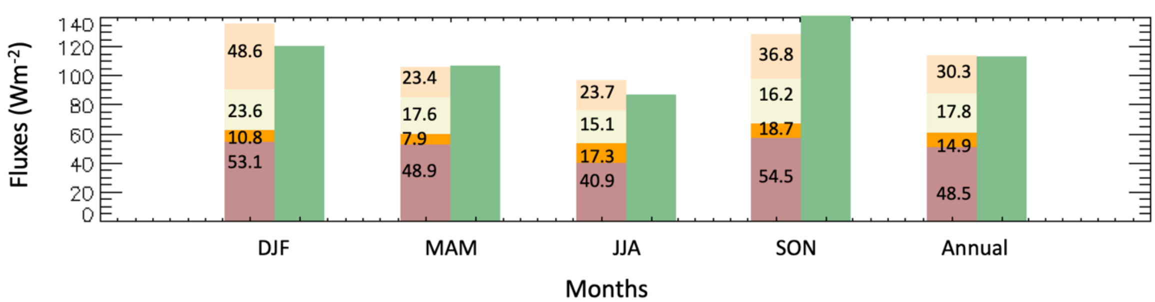

Fig. R2: Seasonal fluxes calculated only for cases with cloud base height < 1.2 km where coupled conditions prevail. Colors are the same as in Figure 7b (Advection: Dark brown; Local tendency: Orange; Beige: Entrainment; Precipitation: Pink; Evaporation: Green)

	DJF	MAM	JJA	SON	YEAR
$\frac{\mathcal{L}}{g}\hat{p}\frac{\partial\langle \boldsymbol{q_t}\rangle}{\partial t}$	-10.8± 68.5 (7.9%)	-7.9±62.1 (8.1%)	-17.3±59.6 (17.8%)	-18.7±68.8 (14.8%)	-14.9±64.6 (13.5%)
$\frac{\mathcal{L}}{g}\hat{p}\langle \boldsymbol{v} \cdot \boldsymbol{\nabla} q_t\rangle$	-53.1±53.8 (39.0%)	-48.9±49.7 (50.0%)	-40.9±43.7 (42.2%)	-54.5±50.4 (43.2%)	-48.5±48.9 (43.8%)
$\frac{1}{g}\omega_e \Delta q_t$	-23.6±12.6 (17.4%)	-17.6±8.9 (17.9%)	-15.1±9.0 (15.6%)	-16.2±9.1 (12.9%)	-17.8±9.8 (15.4%)
$\mathcal{L}$ P	-48.6±97.5 (35.7%)	-23.4±54.4 (23.9%)	-23.7±45.9 (24.4%)	-36.8±60.5 (29.2%)	-30.3±54.4 (27.3%)
E	105.3±50.2	87.9±54.4	84.9±65.4	122.4±79.8	99.1±69.9
Residual	-30.8	-10.1	-12.0	-3.8	-11.7

Table R1: **Seasonal average values and standard deviation of the budget components for cases with cloud base < 1.2 km. In parenthesis are the contributions of each negative term to the total boundary layer drying. Residuals are computed as the difference between source and sinks (Wm^{-2}).**

This point is now mentioned in the introduction Lines 65-68:
"The validity of the mixed layer framework has recently been shown to be sufficient to explain synoptic and monthly variability in the sub-cloud layer (Albright et al., 2022) however our dataset includes a mix of coupled and decoupled cases, and it is therefore important to understand how often the assumption of a well-mixed boundary layer is verified at the site, and how it affects the results."

Section 4 Lines 203-207
"To this end we examined 1825 soundings of which 681 where marine conditions and calculated a decoupling index (DI) defined as $(Z_{CB}-Z_{LCL})/Z_{CB}$. We then classified as strongly coupled cases with DI<0.25 and as weakly coupled cases with 0.25 < DI < 0.4. According to this classification most marine cases (68%) are weakly or strongly coupled. The decoupling index is generally smaller when the cloud base is lower and cases with cloud base < 1.2 km have DI<0.4 in 80% of the cases."

The uncertainty associated with this assumption is discussed in Section 4.3 at lines 327-331:
"At this point we are in the condition to evaluate the impact of including decoupled conditions in the analysis. We repeated the budget computations including only a subset with cloud base < 1.2 km which present mostly coupled conditions. The results showed a diminished contribution of the entrainment fluxes that decreased annually from 26% to 18%. It is therefore

likely that the inclusion of decoupled conditions in the analysis leads to an overestimation of the moisture sink due to entrainment fluxes."

In the discussion at lines 508-510:
"Although the majority of marine cases at the site can be classified as coupled or weakly coupled, inclusion of decoupled cases in the analysis introduces uncertainties leading to an overestimation of the contribution of entrainment fluxes to the budget."

2. The manuscript employs a strict definition of "marine conditions", discriminated solely on the basis of surface wind direction measured at the ENA site, which immediately discards 70% of the available observations. While the desire to eliminate effects of the island on observations is reasonable, given the goals of understanding the marine boundary layer, I wonder if the wind direction limitation also limits the analyses to certain weather regimes and biases the conclusions. Is the boundary layer either "not marine" or strongly affected by the land surface if the wind is from the west (wdir=270°)? There is likely some extra aerosol loading from the island's natural and human activity but does that significantly affect the moisture budget over such a short distance from shore? I would like to see some discussion of this potential issue either in Section 2.2 or Section 6 or both.
The reviewer is correct in the sense that the annual cycle of water vapor and liquid water path doesn't change much if we consider the entire dataset. However, inclusion of all cases makes a difference in the budget calculations as can be seen in the figure below. The surface fluxes are affected however the main difference is the contribution of the advection term that now includes not only the drying contribution from the North, but a moistening contribution from the South. Without separating the marine conditions, it becomes difficult to identify the large-scale mechanism that influences water vapor and clouds at the site. In addition, during southerly wind conditions, warm and moist air is advected over colder ocean water. Hence, by theory, this leads to negative surface fluxes and no cloud formation. Because our primary goal is the study of marine boundary layer clouds, the selection of marine conditions is essential to probe the related processes.

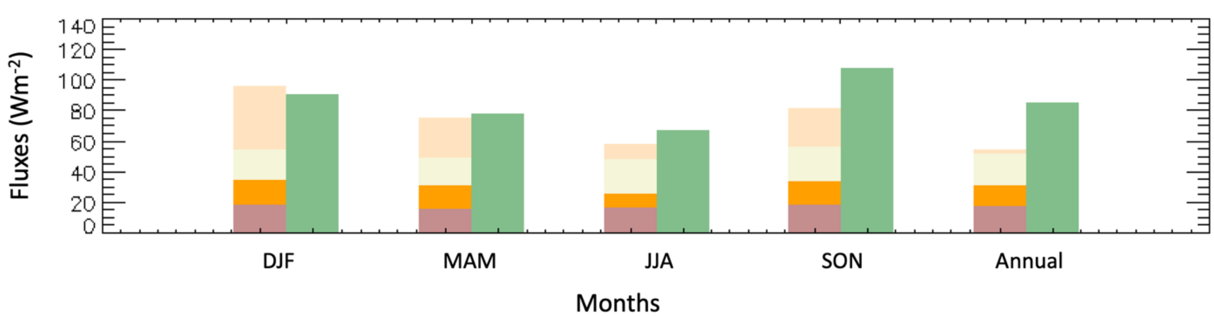

Fig. R3: Seasonal budget including all cases (marine and non-marine). Colors are the same as in Figure 7 (Advection: Dark brown; Local tendency: Orange; Beige: Entrainment; Precipitation: Pink; Evaporation: Green).

3. This manuscript seems to be more about moisture budgets during marine stratocumulus-topped boundary layers than simply “marine conditions”. I think a change to the manuscript title is appropriate in order to accurately advertise the analyses according to the targeted weather/cloud type. The first paragraph of the discussion section also mentions, “we have examined the factors that control boundary layer moisture at the ARM ENA site on a seasonal and daily temporal scale using 5 years of ground-based observations and reanalysis data” but the really only the fully-overcast stratocumulus time periods with a particular surface wind direction were analyzed.
   Probably due to poor phrasing in the initial version of the article, the reviewer got an impression that only overcast conditions were considered. The dataset used in the budget calculations includes all data (clear-sky, cloudy etc.), screened only for marine conditions (wind direction north of 90 and 310). The restriction on cloud fraction was imposed only for the small dataset used in the calculations of the adiabatic LWP that is now moved to the Appendix. We have clarified that in the text and also changed the title to better reflect the subset used. The adiabatic discussion is now moved to the Appendix following reviewer 2 suggestions. Thank you.

4. Many of the more interesting analyses and results are presented in the latter parts of the manuscript but there are so few samples due to inclusion of only 10 (hand-selected?) days. The conclusions formed from work explained in this manuscript would greatly benefit from an increased pool of data.
   We entirely agree with the reviewer. The main constraint to the analysis was the effort necessary to run the AERIoe retrievals. We are now in the process of developing a retrieval from the Raman lidar that will provide a large dataset of high-resolution humidity profiles at 10 s time resolution. This new dataset will allow to expand and refine this analysis. Thank you.

Specific Comments

60: Are decoupled conditions common at ENA? See my General Comment 1 for more.
Assuming the radiosondes statistics as representative of the entire dataset it seems that coupled and weakly decoupled conditions constitute about 68% of the marine data. The effect of including decoupled conditions is now discussed.

144: This may well limit cases to marine environments but other environments exist at ENA so any conclusions are only for the marine state. Please be sure to make that clear throughout the paper. Only 30% of cases are “marine”?
Thank you, we have re-enforced this in the discussion and added to the title.

148: Why such a strict requirement? Is this cloud fraction computed from hourly data so there are 24 values per day? When the manuscript says, “In the following discussion only boundary layer clouds with cloud fraction from the ceilometer greater than 0.99 were selected…”, does it mean all remaining analyses in the manuscript or just the brief remainder of Section 2 and Section 3?

We realize that this digression caused some confusion and we tried to clarify it in the revised version. For the adiabatic calculations we selected measurements coincident with radiosondes and for which the ceilometer reported cloudiness at least 99% of the time during the corresponding hour. This strict selection was imposed only to the adiabatic calculations, due to their sensitivity to cloud boundaries, and not to the budget analysis where all marine cases were considered. This discussion is now moved to the Appendix.

178: "a stronger contribution of the free troposphere to the total PWV in summer compared to winter". Examining Figure 3b, I find it hard to see whether the fractional contribution from z>3km to the total is higher in summer or winter. Certainly, the raw values of PWV(z>3km) are higher in the summer months. Maybe some clarification in 178 is appropriate to distinguish if you mean relative contribution or simply that the annual cycle of PWV(z>3km) peaks in the summer months, which was already stated in line 175.
We changed the phrasing now at Lines 172-175 to: "The annual cycle doesn't have the same amplitude in the upper and lower troposphere resulting in a stronger contribution of the free troposphere to the total PWV in summer compared to winter. The proportion of free tropospheric PWV to the total amount ranges from 14% in February to 20% in June."

We also add here in Fig. R4 for the reviewer a plot of the fraction of upper tropospheric water vapor (> 3 km) to the total water vapor. The upper troposphere contributions vary from ~16% in winter to ~23% in summer. The values are: 0.169722 0.154620 0.165167 0.178396 0.203754 0.221886 0.247377 0.203362 0.188243 0.172586 0.161159 0.178956.

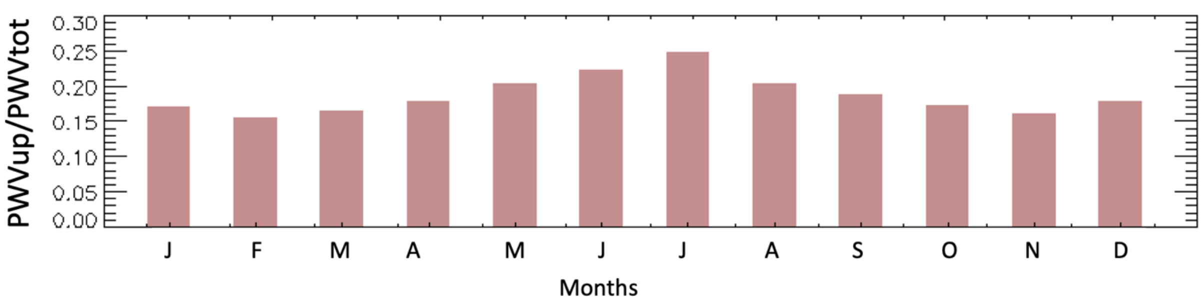

Fig. R4: Fraction of PWV > 3 km to total PWV.

Figure 3: The annual cycle in PWV would be easier to see in Figure 3a if the y-axis limit was reduced to 5 cm. Also, why are the lower y-limits not 0 in all cases?
Figure 3 (now figure 2) was changed as requested. We slightly extended the y axis to show the lower end of the error bar. There are no negative values.

Figure 3: It would be nice to show how many observations are used in each month as the top panel of this figure. Annual cycles in wind direction and cloud type could play a role in the interpretation of these results.
We agree with the reviewer and report now the number of samples in Fig. 3 (now Figure 2). On the left axis is the number of radiosondes samples: 73 79 79 106 71 135 152 148 132 173 143 51

and on the right axis the number of retrievals: 647 578 775 994 536 1056 1300 1338 1283 1793 1372 384. The reviewer is correct that there is a seasonal dependence of the samples probably due to wind direction.

207: This actually appears to be a very *small* dataset of only 304 points! At this point in the manuscript, no ERA5 data are yet used, right? So why have the analyses been averaged up to hourly resolution? This is likely an overly-strict limitation that throws away too much valid data. As I understand it, Figure 4 is showing only 304 out of an initially-available 52608 points, only about one half of one percent of the total data. I realize that radiosondes aren't released every hour, but the cloud fraction>0.99, wind direction, and weak precipitation requirements are likely overly strict and therefore likely to bias the conclusions of these analyses.
At the beginning we were also surprised at the exiguity of the adiabatic sample. Looking at the numbers however, the reasons for this dramatic selection appear more reasonable: The adiabatic calculations can be done only where we have cloud top and cloud base well defined. Because there are only 2 launches per day, we have about 2000 marine cases. Out of these the cloud top height was found in 1190 marine cases. When we combine the ceilometer including the cloud fraction restriction, MWR, and sondes, we find that all data were present and valid in about 300 cases. Probably the only way to expand the dataset for the computation of the adiabaticity is to use the KAZR to identify the cloud top height and have continuous retrievals of thermodynamic properties This would likely increase the dataset but not of much.

219-220: Many of the cases, even for relatively thick (>500 m) clouds, have adiabaticity considerably greater than one, with some as high as 150% adiabatic. Please provide more discussion and validation of the cloud boundary argument. If these cases simply appear to be superadiabatic due to uncertainties in cloud boundaries, that uncertainty will likely also affect the cases that appear sub-adiabatic. Can these "superadiabatic" cases result from the microwave radiometer seeing elevated large rain drops instead of only smaller cloud drops? More discussion is needed here because this occurs for a large portion of the limited dataset even for relatively thick clouds.
We thank the reviewer for this comment. As the reviewer points out, the largest uncertainties are due to liquid water path and cloud boundaries. When we examined the calculations, we also realized that we had used the total liquid water path instead of the cloud liquid water path. When we changed this, we obtained 55 superadiabatic cases or ~14% instead of the previous 19%.
After this change, we also examined the effect cloud boundaries. Increasing the cloud thickness of 50 m decreased the mean f_{ad} from 0.56 to 0.45 or 20% of the calculated value. Similarly, decreasing the cloud thickness of 50 m increased the mean f_{ad} from 0.56 to 0.67 or 50%. It is therefore likely that, in addition to the uncertainty in LWP, uncertainty in cloud boundaries affect the calculations. We replaced the figure (now Fig. A2) to reflect the changes and added the following discussion in Appendix A lines 619-622 where the adiabaticity is now discussed:

"Increasing the cloud thickness of 50 m decreased the mean f_{ad} from 0.56 to 0.45 or 20% of the calculated value. Similarly decreasing the cloud thickness of 50 m increased the mean f_{ad} from 0.56 to 0.67 or 50%. It is therefore likely that, in addition to the uncertainty in LWP, uncertainty in cloud boundaries affect the calculations."

230: Does the liquid potion (ql) also include rain water mixing ratio? I assume it does from equation 1 but it would be helpful to be explicit about it in line 230.
Yes, we include rainwater mixing-ratio. Added "including rain" at line 210.

254: The MBL height is likely not constant during the 12 hours containing a given PBL measurement. Would it be better to use a moving polynomial interpolation?
It could be done but considering that at the end we average again it may not have an effect on the overall results. If we were looking at daily time scales it may make a difference though.

Line 273: If you've limited the analyses to when wind direction indicates "marine conditions", why are there *any* cases with v>0? Disagreement between ENA observations and ERA5?
Marine conditions were determined using *hourly averaged* MET measurements at the site. In each individual met hours there could be wind in other directions. In fig 5 (now Fig. 3) we plot hourly ECMWF data. Individual hours from the ECMWF dataset could also have occasional prevailing wind in other directions as shown by the whiskers extending to positive values, the mean values are consistent with the average MET measurements at the site. The ECMWF data are averages over a 25 km x 25 km grid and mostly signify the background wind conditions. Hence there are small number of cases where the local wind as measured by the surface met station is from the North, while the background prevailing wind is from the South. Thank you.

276: Why omit the extremes? What evidence do you have that these extremes are simply instrument noise instead of real events that should be included in the analyses?
We agree that it is hard to distinguish between real rain extremes and effects due to splashing of raindrops at the surface. It is also likely that during hours of extreme precipitation other sensors, such as the MWR have increased uncertainty. Nonetheless we repeated the budget calculations with all precipitation data and have updated figure 7 (now Fig. 5) and table 2. The conclusions are unchanged, but there is an increase in the precipitation contribution during winter months. Uncertainties in the rain rate measurements are one of uncertainty components of the budget.

	DJF	MAM	JJA	SON	YEAR
$\frac{\mathcal{L}}{g}\hat{p}\frac{\partial\langle \boldsymbol{q_t}\rangle}{\partial t}$	-8.4± 67.5 (6.2%)	-7.6±62.2 (7.1%)	-13.2±58.5 (13.7%)	-9.6±66.5 (7.5%)	-10.1±63.5 (8.9%)
$\frac{\mathcal{L}}{g}\hat{p}\langle \boldsymbol{v} \cdot \boldsymbol{\nabla} q_t\rangle$	-54.1±51.3 (40.0%)	-51.9±47.5 (48.9%)	-40.1±41.6 (41.6%)	-56.9±51.2 (44.3%)	-50.4±48.2 (44.2%)
$\frac{1}{g}\omega_e \Delta q_t$	-27.8±14.1 (20.6%)	-25.5±12.1 (24.0%)	-22.4±12.5 (23.3%)	-31.5±15.9 (24.5%)	-25.8±14.3 (23.4%)
$\mathcal{L}$ P	-44.9±89.9 (33.2%)	-21.1±46.9 (19.9%)	-20.7±60.5 (21.4%)	-30.5±63.5 (23.7%)	-26.8±61.8 (23.6%)
E	119.7±57.7	106.7±62.2	87.1±48.1	141.2±68.5	112.7±63.3
Residual	-15.4	0.6	-9.4	12.6	-1.4

Table 2: Seasonal average values and standard deviation of the budget components. In parenthesis are the contributions of each negative term to the total boundary layer drying. Residuals are computed as the difference between source and sinks (Wm^{-2}).

301: Air density is not constant in the boundary layer. Is there confirmation that the vertical change in atmospheric density matters much less than changes in PBL height? Or do you mean that the profile of air density is relatively constant in time?

We mean that the air-density is horizontally constant but changes vertically. This assumption is needed for deriving mixed layer budget equation, and is regularly applied while utilizing mixed layer budgets (e.g. Kalmus et al., 2011 J. Climate; Ghate et al., 2019 QJRMS). Thank you.

387: How were these cases chosen? The manuscript says "The selected days displayed persistent boundary layer cloudiness and at times precipitation" but how were they identified? By eye? Some thresholds on cloudiness?

The cases were a subset of a larger sample of marine stratocumulus cases. There were not additional constraints, besides data availability. The exiguity of the dataset is mostly due to the complexity of running all retrievals, especially the humidity retrievals that require AERI, Raman lidar, and MWR. We are working on a larger dataset for which we are developing a Raman lidar retrieval of water vapor at 10 s time resolution.

397: Why are daily averages used? Do the winds not change at all during each of the 10 days? These cloud base winds come from ERA5, right?

The wind speed at the cloud base was derived from the interpolated sondes and the variability of the wind speed during the selected days was low. We now add this information at line 383. Below is a table with the average wind speed at cloud base and standard deviation for the selected days.

Date	Wind speed at cloud base, m s^{-1}
20170316	9.0±0.6
20170317	8.2±1.0
20180805	4.5±0.5
20180904	5.5±1.2
20180331	5.9±1.8
20190404	11.0±1.5
20190604	7.8±1.6
20190608	5.1±2.7
20190625	6.3± 0.9
20190626	8.3±0.7

400: So there are 345 mesoscale chunks during the 10 selected days? The chunks from within a given day have the same amount of temporal averaging but there are different temporal averaging times between the 10 days?
The chunks have the same amount of spatial (10 km) averaging, but, because of the different wind speed, the temporal averaging times vary between the 10 days.

433: In line 428, the manuscript states that "moist and dry neighboring columns … were compared to those of its preceding and following neighbors. For clarification, are all of the 345 mesoscale perturbations considered but only 143 columns had durations longer than 10 minutes or was there a requirement for Figure 10 that only moist columns that were neighbored by dry columns be considered?
All perturbations were considered, and they all alternated positive and negative columns.

Figure 11: There are really few cases here. I think the filtering is too strict. How can we know that these 5 cases constitute a representative sample the full population for PWV perturbations from 0.8 to 1.0 cm?
We agree that the largest perturbations are not representative because of the scarcity of samples and reinforce this concept in the text. We would like to keep this part that we feel is important and are working on a larger dataset. The extended dataset requires the development of new high resolution vapor retrievals from the Rama lidar. We added the following statement at line 430: "The few cases in the last bin are not sufficient to provide a valid sample from which to draw definite conclusions."

469: Please remind the reader what papers have proposed and promoted this mechanism.
Added Bretherton, C. S. and P.N., Blossey, 2017 at line 457.

473: Why might the doppler lidar retrievals be invalid in a given layer? Could these observational limitations imposed by the doppler lidar result in a biased view of the vertical motion?

The Doppler Lidar signal attenuates vertically near cloud base, which usually is fluctuating. Hence there are instances where samples will be available near the surface and most of the sub-cloud layer, while very few samples near the cloud base. Hence the averages calculated from these samples can potentially lead to incorrect conclusions. To get around this issue, we have only averaged heights where at least 80% of the time Doppler Lidar samples were available.
The sentence is changed to the following, “Only heights where at least 80% of the Doppler lidar readings are valid were used in calculating these averages.” Now line 460-461.

Figure 12: It would be simple and helpful to add uncertainties to (c) and (d) by subsampling/resampling techniques, which would strengthen the claims about differences between the two distributions.
We reran the 2 figures for 3 subsets of approximately half the original size and now report in the figure the boundaries as error bars.

538: Other papers have suggested this, too, right? They should be cited here.
We have cited almost all the articles showing convective aggregation earlies in the section. We therefore added: “as hypothesized in the previously cited works.” At line 535-536. Thank you.

Technical Corrections
238: It would be best if you used either “3rd and 4th” or “third and fourth”. -Done

418: Both the LWP and precipitation increase when columns are moist so you do not need the “respectively”. Changed-Thank you

418: Parentheses are for clarification and references, not the exact opposite of what was just stated. You could remove the content of the parentheses and the sentence would be much clearer to the reader. Sentences like, “Moister columns correspond to regions of increased liquid water path and precipitation.” and “Increased moisture is associated with increased liquid water path and precipitation” are understood more easily. Additionally, in the preceding paragraph and in the following sentence, parentheses are used for clarification instead of opposition. - Changed

436: A dryer is a household appliance for making wet close dry. You want “drier”. I confuse these two often, too. Done, thank you for catching this.

474: Why is “downdrafts” in parentheses? Removed, it was probably a residue from a previous version.