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Comment on acp-2022-615

Anonymous Referee #1

Referee comment on "Boundary layer moisture variability at the Atmospheric Radiation Measurement (ARM) Eastern North Atlantic observatory during marine conditions" by
Maria P. Cadeddu et al., Atmos. Chem. Phys. Discuss.,
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First review for ACP-2022-615

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.

General Comments

- 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

- 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 ($wdir=270^\circ$)? 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.
- 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.
- 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.

Specific Comments

60: Are decoupled conditions common at ENA? See my General Comment 1 for more.

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"?

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?

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 $z>3\text{km}$ to the total is higher in summer or winter. Certainly, the raw values of $\text{PWV}(z>3\text{km})$ 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 $\text{PWV}(z>3\text{km})$ peaks in the summer months, which was already stated in line 175.

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: 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.

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.

219-220: Many of the cases, even for relatively thick ($>500\text{ m}$) 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.

230: Does the liquid potion (q_l) 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.

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?

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?

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?

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?

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?

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?

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?

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?

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?

469: Please remind the reader what papers have proposed and promoted this mechanism.

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?

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.

538: Other papers have suggested this, too, right? They should be cited here.

Technical Corrections

238: It would be best if you used either "3rd and 4th" or "third and fourth".

418: Both the LWP and precipitation increase when columns are moist so you do not need the "respectively".

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.

436: A dryer is a household appliance for making wet close dry. You want "drier". I confuse these two often, too

474: Why is "downdrafts" in parentheses?