Review of Viscardi et al. 2023, ACP
"Environmental Controls on Isolated Convection during the Amazonian Wet Season"

This study aims to characterize the environmental factors associated with the predominance of different convection modes over the Amazon basin during the wet season. Based on a systematic classification of the convection events predominant from late morning to evening in a 100 × 100 km sub-domain located in the Central Amazon, the authors obtain mean composites of 1D and 2D measurements and compare their most relevant patterns among different convection regimes.

Overall, the paper is well structured, and results are put into context by comparing them to the preceding literature. Although some of the analyses resemble previously published results, the use of different data and methodology compared to those adds relevance to this manuscript, especially considering the difficulty of generalizing in-situ atmospheric observations. Moreover, the moisture convergence analysis has not been previously explored using this set of observations, to the best of my knowledge.

However, I have some concerns regarding the impact of measurement uncertainties on the results, as well as the statistical significance of the findings. Additionally, I would like to see some aspects of the methodology clarified. I also found that some statements need further justification or consideration. Therefore, I recommend major revisions.

Major comments

1. One of my main concerns regarding the methodology employed in this study, is the impact of the errors in the retrieval of the water budget terms, on the calculation of the divergence term. I support the ideas suggested by the previous reviewer to assess this impact, such as repeating the analysis with a different data source, and further discussing the shortcomings and implication of these issues via the inclusion of a residual error term in the budget equation.

   Relatedly, the control volume over which the budget analysis is performed needs to be defined. Please discuss how well do the measured profiles represent the control volume, and its implications for the analysis.

   In the water budget equation, you have used the hydrostatic approximation $-\rho.\partial z = \partial p/g$, whereas it is employed to analyze cases where the non-hydrostatic component may be high, especially in the deep convection cases. The expression employed to calculate the first term may thus induce errors in CVV and LWP that ultimately affect the calculation of the divergence term, for a given E and P. Please discuss the implications of this assumption for the results and justify the validity of your approach.

2. The sizes of the regime samples seem relatively low, therefore I suggest analyzing the statistical significance of the differences found between the regimes for the different aspects considered in the results section.

3. For a reader that is not familiar with the data employed here, it may result difficult to keep track of the different measurement sources throughout the different sections, therefore I suggest including a table in the methodology section with the name and short description of each data source, along with the dimensionality of the data and its use in the paper.

4. Is the 2% threshold of 'rain coverage' used in the convection regime classification arbitrary? Please discuss how would the choice of a different threshold affect the results? I suggest showing

histograms of the rain coverage at each height interval ($h < 3$km, 3km$< h < 8$km, and $h > 8$km), for each cloud regime. This would complement Fig 5, and help understanding how much would the choice of the threshold impact the classification.

5. The adequateness of the 'bulk wind shear' (defined here as the difference in wind speed between two specific levels) to analyze the impact of wind shear on convection development is questionable. Different from other magnitudes that represent the entire atmospheric column or portions of its depth, the so-called 'bulk wind shear' depends on punctual wind measurements along the atmospheric column, neglecting the behavior at other levels. For example, as mentioned in section 4.4, the difference in the wind speed from 0 to 2km is generally higher in the deep-convection compared to the shallow-convection days, thus the authors conclude that 'low-level vertical wind shear is related to convection development'. However, Fig. 10a shows that the vertical wind shear is actually more intense in shallow cases at 8h, when considering the levels of maximum LLJ and the surface as a reference. A more apples-to-apples comparison could be done by, for example, calculating the bulk shear from the level of maximum wind to the surface, and complementing this information with the height of such maximum.

Minor comments

- **line 14:** Missing comma after 'even moisture'
- **line 46:** Define 'mid-levels', for clarity
- **line 72:** Typing error: 'a city border the Rio Negro'
- **line 77-78:** Suggested: '... a cloud mask based on time-height profiles of the cloud location'
- **line 83:** What do you mean by 'cloud frequency profile'? Relatedly, in Fig 5's caption, what is the percentage cloud frequency relative to? Is it the total number of cases? Please clarify.
- **line 107:** 'water vapor mixing ratio'
- **line 112:** Do you mean 'the 100-hPa depth layer immediately above the surface'?
- **line 126:** Need to define the control volume over which the conservation equation is being applied.
- **line 129:** Please rephrase for clarity: 'E and P correspond to the water mass fluxes associated with surface evaporation and precipitation' (specifying 'surface evaporation' avoids confusing this with internal phase changes in the control volume)
- **line 131:** 'with the divergence of water vapor'
- **line 132:** I believe this statement is incorrect. Since the variable here is total water mass mixing ratio, it is not affected by phase transitions associated with cloud formation in the control volume.
- **line 134:** I believe there is a mistake here. The terms in Equation 1 have units of mass flux, such as kg/m^2/s. Dividing by the density of liquid water would give units of m/s, which does not seem to be the intention. Moreover, to express the equation in terms of liquid and vapor water paths, it is not necessary to divide by anything, since the integral of the total water mixing ratio divided by the gravity acceleration with respect to pressure already represents the total water path in a hydrostatic atmosphere. This total water path can then be separated into vapor, liquid, and ice terms, as you mention below.
- **line 134:** 'ignore' should be 'neglect'
- **line 134:** Are you neglecting the time variation of the ice mass or the presence of ice itself in the analysis? Does the observed LWP you use include all condensate or does it discriminate between ice and liquid water?

- **line 138:** If you follow the suggestion I mentioned earlier and avoid dividing by any additional factors, the terms in the equation can be represented as EVAP = E and PREC = P. I recommend maintaining one naming convention for consistency and clarity.
- **line 141:** The data in Fig 9 seems to have a higher frequency, intervals seem to be of 12 min.
- **line 152:** Do these times define what you call 'diurnal cycle' throughout the paper? If so, please clearly define the term 'diurnal cycle' when it is first used in the text, to ensure that readers understand the time period being discussed.
- **line 153:** By 'rain coverage' are you referring to large hydrometeors, independent of the phase. Please clarify.
- **line 157-164:** By 'precipitation', do you mean 'rain coverage'? If so, please correct the text here for consistency.
- **line 165-166:** Above, you list the conditions for each type of convection regime, while here you list conditions that apply to all regimes, right? If so, I suggest clarifying this.
- **line 173:** I suggest 'the propagating-convection category occurs more frequently during the wet season', for clarity.
- **line 195:** I suggest rewording 'because' to: 'associated with'.
- **line 200:** Suggested rewording: 'coincides' to 'is consistent with'
- **line 207:** Please add the figure numbers.
- **Fig 6c,d:** Is the difference between the profiles for different regimes larger than the standard deviation of the measurements? The length of the bars is not evident in panels a and b, so at least commenting on that would be helpful.
- **line 214:** I suggest providing a quantitative measure instead of 'is quite similar'. Maybe something like: 'the difference between the moisture profiles for different regimes is less than 1/x of the maximum difference at lower levels'.
- **line 217:** Suddenly starting to talk about the tropical oceans reads a bit strange. If the goal is to mention contrasting results, I suggest rephrasing it to smooth the transition, for example: 'Moreover, these results contrast with studies over tropical oceans, where free-tropospheric humidity has been shown to play a more significant role...'.
- **line 223:** Where it says '...at 2 LST', it should be noted that since the classification was done for times between 10-20 LST, measurements at other times of the day might include different types of cloud than those in the regime in question.
- **line 231:** Where is says 'despite slightly larger latent heat', add a reference to the section of the manuscript where this is shown.
- **line 233:** 'water vapor convergence', ditto
- **line 242:** Please maintain the naming convention (MLCAPE for CAPE).
- **line 249:** It appears that you are calculating 'accumulated values' by numerically integrating the mm/day values over time intervals of approximately 12 minutes. However, it's unclear how the resulting values are still being expressed in mm. Given that the time step (dt) is 1/120 days, it seems that the resulting unit should be mm multiplied by the time step, which would be mm/120.
- **line 249:** Instead of 'variation in LWC', I suggest using '∂_tLWC', for clarify.
- **line 250:** 'dominated' seems to imply that CONV is the largest term among those in the budget analysis, so I would suggest rewording, for example, 'shows mostly water divergence'.

- **line 251:** The 'divergence' term responds to changes in the total water, not just water vapor. I understand that the convergence is expected to occur in the form of water vapor, since horizontal transport of condensate is less likely, but this should be somehow clarified, at least in a footnote.
- **line 251:** Suggest: 'relatively neutral'
- **line 256:** I suggest framing the discussion slightly different: instead of saying that high evaporation balancing the divergence leads to low precipitation, it is seems more intuitive to think it as high evaporation and low precipitation requiring strong divergence for closure.
- **line 257:** Ditto
- **line 258:** Is CWP a typo? Do you mean CWV? As commented before, it is clearer to say $\partial_t CWV$ than 'the variation in CWV'.
- **line 259:** What do you mean by 'accumulation of water vapor'? Are you referring to Fig. 9d? If so, please clarify. Similarly, clarify the panels you are referring to in the rest of the text.
- **line 269:** Please expand briefly on how the LLJ is affected by the PBL growth. Do you mean that the PBL growth slows down the LLJ? Please clarify that this is one possible explanation, since other factors may be involved in regulating the intensity of the LLJ.
- **line 271:** Better refer to the layer between $\sim$ 600 and $\sim$ 350 hPa or so, since at 300 hPa the wind tends to be intense in the deep case.
- **line 275:** Suggested: 'The most notable difference'
- **Fig 9, caption:** Instead of referring to the number of the subsection, I suggest referring to the number of the equation.
- **line 295-297:** Although the 6-km level is taken as a reference here, the corresponding 'bump' in the wind profile starts at 4km and reaches 8km height (as you mentioned in the discussion of Fig. 10). Moreover, it would be incorrect to say that only because it happens at levels higher than the top of the cloud, the wind pattern does not matter for the convection development. For example, the wind profile impacts the gravity wave pattern induced by convection itself, and these waves may then feedback on the dynamics of the convective clouds [e.g. CHK86].
- **line 308:** 2.5 J/g = 2500 J/Kg, not 250 J/kg. But there is no need to mention both values anyway.
- **line 321:** Based on this results, you can only state that CAPE is not a good indicator for precipitation. This analysis doesn't involve convection strength, but precipitation only.
- **Fig 14, caption:** 'average' should be 'averaged'. Also, what do the vertical bars mean?
- **line 328:** 'Results showed that isolated deep convection is associated with more extensive and longer-lived clouds throughout the diurnal cycle.' Do you mean compared to shallower clouds? I suggest omitting this statement, as such feature can be considered 'common knowledge'.
- **line 339:** Please clarify what you mean by 'noted significant differences between morning atmospheric conditions and convective intensity'.
- **line 366:** It should be 'captured', instead of 'capture'.
- **line 380-382:** Did they suggest that wind shear would both favor and hinder convection? Please clarify.
- **line 406:** 'this value', which one?

References

[CHK86] Terry L. Clark, Thomas Hauf, and Joachim P. Kuettner. Convectively forced internal gravity waves: Results from two-dimensional numerical experiments. *Quarterly Journal of the Royal Meteorological Society*, 112(474):899–925, 1986.