

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

The authors made an effort to address the comments and suggestions from the initial review, and the revised manuscript shows improvement over the original version. However, further revisions are required, as outlined below.

Note: Suggestions to explain or expand on certain aspects refer to the manuscript itself, not just the authors’ response.

1 Major comment:

Making inferences about a population requires a rigorous assessment of the statistical significance of the results obtained from a sample. In other words, smaller samples increase the likelihood that random errors in observations could explain the observed behavior. Applied to this study, this means that ensuring the differences between shallow, congestus, and deep convection regimes seen in the sample can be generalized requires an understanding of how likely these differences are to occur simply by chance.

After being asked to include a discussion on the implications of the relatively small sample size for their results, the authors argue that they have considered all uncertainties, while relying on the sample’s standard deviation as an indication of “error.” However, the standard deviation of the sample only informs about the distribution of values within the sample and does not necessarily indicate how close the sample mean is to the population mean. To assess this, an evaluation of the standard error, which is related to the standard deviation of the sampling distribution of the mean, is needed.

Increasing the sample size may not be feasible nor within the scope of the paper, but discussing the limitations of the results obtained from this sample would significantly improve the conclusions of this study. Therefore, my recommendation is for the authors to apply a bootstrapping or other resampling technique to this dataset to better support their conclusions and account for the potential impact of the sample size on the results.

2 Minor comments:

1. To ensure completeness, it is suggested that the authors include the error propagation formula for the CONV term in the budget equation, as mentioned in their response, within the manuscript itself. This formula involves implicit assumptions, such as a lack of correlation among the terms $\partial_t \text{CWV}$, $\partial_t \text{LWP}$, EVAP, and PREC, which may not be immediately apparent. Explicitly stating these assumptions should enhance the clarity of the manuscript. Similarly, the authors should provide an explanation of how the standard error of the PREC term was obtained from the three considered data sources. Including this information will further improve the understanding of the methodology employed in the study.
2. The manuscript contains multiple statements based on speculation, which are acceptable since, in most cases, observations cannot provide measures of all elements potentially affecting the behavior of certain variables. However, such statements must be treated as speculative by including qualifiers such as “likely” or “presumably” instead of making assertions without proper evidence. While some instances of such statements were indicated in the previous review, similar

expressions can still be found in the updated manuscript. It is the authors' responsibility to thoroughly address and revise these instances throughout the text. As a reference, the following statements (lines 148-152) serve as examples: "... are uniform around the experimental site", "CWV and LWP relate to regions associated with the domain average of a cloud size scale", "our water balance is consistent for a temporal-spatial scale of ~ 1 hr and ~ 10 km." In these cases, the authors should consider rephrasing the statements to accurately reflect the speculative nature of their claims.

3. The text on lines 223 and 233 uses the phrase "little statistical significance." It is advisable to avoid referring to the statistical significance of results unless they have been objectively analyzed through parametric statistical tests or resampling strategies. In such cases, the methods and outcomes should be explicitly clarified in the manuscript, as previously mentioned.
4. The authors have argued that the results remain consistent when using different rain coverage thresholds for the convection regime classification, other than the 2% threshold mentioned in the paper. While it may not be practical to present all possible analyses, mentioning this consistency in the text would add value to the discussions and support the robustness of the findings.
5. The comment made during the previous review round about the statement "A mixed-layer parcel from the surface to 100hPa is used..." might have been unclear. To clarify, it is suggested that this statement could be rephrased as "the 100-hPa layer immediately above the surface" to provide better context and understanding.
6. The authors have clarified that they neglect ice in their analysis; however, the manuscript still states "neglecting the time-variation of ice," which should be revised to "neglecting ice" for clarity. Additionally, the authors justify this neglect by stating that the time variation in LWP is negligible in the budget analysis. Since ice water path was not considered in the study, further clarification is needed to support this justification. It appears that the authors assume $\partial_t IWC < \partial_t LWP$ in general, but this assumption is neither evident nor explicitly mentioned in the manuscript. To ensure transparency, the authors should provide a justification for this assumption within the manuscript.
7. In Section 2.3, it is recommended to clarify the term "rain" in "rain coverage" to specify that it refers to radar echoes exceeding a given threshold, regardless of whether the phase is ice or liquid. This will provide better context for the readers and improve the understanding of the methodology.
8. In the caption for Figure 6, it would be helpful to provide clarification on how the error bars for panels c and d were obtained.
9. Line 320 and Fig. 14's caption: $2.5 \text{ J/g} \neq 250 \text{ J/kg}$.
10. The author's effort in explaining the interpretation of Zhuang et al. (2017)'s results in their response is appreciated. However, the original intent of the comment was to recommend revising the explanation within the manuscript (lines 390-393) for improved clarity and understanding.
11. Line 27: "deep convection in either..." should be "deep convection either..."
12. Line 62: It is recommended to use an alternative term for "sensitivity," such as "assess correlations," to more accurately describe the intended analysis and avoid potential misinterpretations.
13. Figure 1's caption: It is recommended to clarify the intended meaning of the phrase: "The dotted circle describes the S-band radar domain with *available measurements*."
14. Line 107: It would be helpful to clarify the assumptions made in the vertical integration up to 200 hPa, in cases where the sounding only reaches up to 8 km.
15. Line 244: The term "mixed-layer MLCIN AND MLCAPE" contains redundant elements, as the "ML" prefix already indicates that these variables refer to the mixed-layer. Consider revising this to "MLCIN AND MLCAPE" for conciseness and clarity.

16. Line 261: The phrase “Changes in CWV” should be represented as $\partial_t CWV$ for clarity. Regarding “changes in condensate,” please specify if this refers to $\partial_t LWP$. Furthermore, it appears that “as” might be a more appropriate choice than “whereas,” and “low” instead of “lower.” Please review and revise the wording accordingly. It is important to note that negative CONV (or divergence) is not solely due to the mentioned factors, as EVAP is actually the dominant term. Please adjust the explanation to accurately reflect this information.
17. Figure 9: It is recommended to use a symmetric logarithmic scale for the y axis.
18. Line 282: The phrase “more significantly affected by PBL growth” can be reworded as “likely more influenced by PBL growth compared to other regimes” to enhance clarity and accurately convey the intended comparison.
19. Line 285: It is recommended to add “respectively”, at the end of the phrase.
20. Line 309: The term “more robust” in this context requires clarification. Elaborate on what is meant by “robustness” and how it is measured or determined in this case, as larger standard deviation typically indicates a higher level of variability, which might be interpreted as less robustness. Providing additional context will help readers understand the intended meaning.