Dynamical imprints on precipitation cluster statistics across a hierarchy of high-resolution simulations

Claudia Christine Stephan and Bjorn Stevens

October 1, 2024

General comments

This well-written manuscript presents an analysis of high-resolution global simulations to investigate the mechanisms driving the power-law behavior of tropical precipitation clusters. Using a hierarchy of ICON simulations with varying degrees of realism, the authors effectively demonstrate that the presence of stirring and large-scale vertical overturning dynamics, such as planetary and synoptic-scale variability, are key in producing the observed power-law distributions in precipitation statistics.

I believe this study has the potential to shed light on important open questions in the literature regarding the statistical behavior of precipitation clusters and their link to large-scale dynamics. The methodology is sound, the figures are clear, and the article is well-structured.

However, my main concern with the manuscript is the lack of emphasis on the motivation for studying these power-law behaviors and the underlying mechanisms. The authors should better articulate how this work advances the understanding of these phenomena and their significance to the broader scientific community. Additionally, there is a disconnect between the figures and the text. While the figures have the potential to convey a cohesive story, the text provides only a brief discussion, which limits their impact. A more in-depth discussion of the figures would strengthen the manuscript.

1 Specific comments

- L59–L63: While the authors state that a 10 km resolution is sufficient to resolve convection, how might the results change with a finer resolution? For example, would a higher resolution capture more small precipitation clusters or column water vapor (CWV) islands, and would the power-law behavior still hold under these conditions?

- L64–L86: Rather than merely describing the technical details of each simulation, the authors should clarify the motivation behind selecting these specific simulations. What scientific questions or hypotheses are addressed by each choice?
- L100: Could the authors provide more context regarding the choice of the 2 mm/hr threshold, especially in relation to thresholds used in other studies?
- L102: Is there any anticipated sensitivity of the results to the definition of pixel connectivity in the analysis? It would be helpful if the authors could discuss this aspect.
- L130: The claim that the CTL simulation closely resembles observations feels somewhat overstated, particularly given notable differences in regions like the Maritime Continent and Australia. It might be more accurate to soften this comparison.
- L142: The statement, "Vertical velocity spectra are useful for comparing the prevalence of different scales of vertical motion between the simulations," is key for understanding a major part of the analysis. The authors should expand on this and explain the significance of different slopes in the vertical velocity spectrum in greater detail.
- L154: Could the authors clarify the motivation for defining the reduced CWV?
- L215: In line with my earlier comment on the study's motivation, the conclusion section could more clearly emphasize the contributions of this work and suggest specific questions or directions for future research that stem from the findings of this study.

2 Technical corrections

- L69: typo "input4MIPS"?
- L108: ..."Perimeter λ and..." , you introduce λ as the perimeter in L104, so it would be clearer to define λ there when you first mention it.
- Figure 6: The x-axis label should explicitly say "max reduced CWV" to align with the figure caption.
- Figure 8: Specify which panels correspond to the slopes derived from Figs. 7, C1–C3 to enhance clarity.