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# *Interactive comment on* "Large-scale Dynamics of Tropical Cyclone Formation Associated with the ITCZ Breakdown" *by* Chanh Kieu et al.

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General comments: Overall this work presents a very compelling, albeit mathematically complex, physical argument in a simplified barotropic framework for the upper bound on the number of tropical cyclones that emerge from the breakdown of the ITCZ. This upper bound may have a direct role in setting the annual number of tropical cyclones on Earth, for which no current theory exists. I find the manuscript to be of very high quality in terms of both writing and physical framework, though I cannot fully evaluate the mathematical analysis, particularly for the Principle of Exchange of Stabilities, as it lies beyond the scope of my expertise. Nonetheless, if fully validated, this would appear to be a rather remarkable feat of dynamical systems theory for explaining the nature of the breakdown of the ITCZ on an Earth-like planet and its potential relevance



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to global constraints on tropical cyclogenesis.

Specific comments: P2L30: It would seem very relevant here to include the work of Patricola et al. (2018; https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1002/2017GL076081) , which found that filtering out AEWs did not alter the number of storms in the Atlantic, suggesting that disturbances from other sources (e.g. ITCZ breakdown) may take their place.

P3L5/Figure 1: I'm not sure this figure, taken from Kieu et al. (2018), is appropriate here, as it is included without explanation. If the figure is presented within the stated reference then the reference alone should suffice without the figure being reproduced here.

P11L25: I'm unsure where "3 x 3000 km" is coming from here. Lx/m = 3333 km.

P11L27: I'm not sure I agree with this statement that storm size "must be larger than a limit of ∼10ˆ3 km" – this may be a lower bound on size for this specific case of an equatorial band of TCs of equal size. In reality individual storms may take a range of sizes, and certainly there are instances of very small storms that appear to be have a diameter much smaller than this length scale. This is not incompatible with the model presented here, reality is simply more complex.

Given that it is an important parameter (aspect ratio, with Lx fixed), how would one plausibly define Ly for the real world? Is there some physical sense of what would represent the poleward boundary relevant to the system?

P14L34: Aren't these westward-moving disturbances simply Rossby waves? Based on Fig 2, at the ITCZ location (dashed line), Uyy=0 and thus the PV gradient is purely beta. Does their phase speed follow the barotropic Rossby wave phase speed for wavenumber equal to the unstable wavenumber predicted by the model (4 m/s)? I wonder if Rossby waves are a more appropriate analog than African Easterly Waves for the features in the model.

To what extent does this model reproduce the behavior of the traditional model for barotropic instability under parallel shear flow and the associated Rayleigh-Kuo conditions for instability? Based on Figure 2, it appears that the physical framework is the same. However, I am trying to understand the notion that the periodic state is a secondary stable state, which is in contrast to a traditional barotropic instability model in which the instabilities would be expected to continue to grow.

Technical corrections: General: Suggest simply using "genesis" rather than "TCG" – acronyms are overused.

P4L15: might note in the text for clarity that Delta here represents the Laplacian, which is typically denoted with nablaˆ2

P4L17: extra "the"

P4L21: parenthesis error

P6L11: replace "while" with "though it"

P5L28: "nonlinearity"

P7L12: I believe this should be no v-wind component

P8L15: "eigenvector"

P9L13: "turns out"

P10L19: "if it exists"

P13L10: "all the same"

P14L16: "nondimensional"

P14L20: issues with the parentheses

P14L24: "obtained from the"

P14L30: “holds for the”

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