Response to Reviewer 1 (Dr. Daniel Chavas)

We would like to thank Reviewer 1 for your constructive comments and suggestions. In this revision, we have made a number of changes to take into account your concerns. Please find below our point-by-point responses to your comments and our corresponding changes.

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.

Your encouraging evaluation of our manuscript is much appreciated. We hope that our revision below will be now acceptable to you.

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.

Thank you for pointing out to us this work of Patricola et al. (2018), which is indeed relevant to our study. Their finding about the insignificant role of AEWs in modulating the number of the TC climatology in the Atlantic basin is noteworthy, as it is consistent with our model presented in this study in the sense that our model does not contain any local feature such as the African Jet that triggers AEWs. One thing that we wish to note is that the AEWs are just one class of a much broader group of easterly waves in the tropical region. While AEWs can be filtered as presented in Patricola et al. (2018), the other modes of easterly waves such as mixed equatorial Robssy waves can still exist. Anyway, both Petricola et al.'s results and our study herein appear to suggest an intrinsic mechanism at the large scale, which controls the climatology of the TC numbers beyond the basin-specific features. This work has been now cited in this revision.

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.

Our purpose of including this figure here is to highlight the key importance of episodic development of TCs at the global scale so one can estimate the global number of TCs annually. Essentially, we need two pieces of information to be able to determine how many TCs the tropical atmosphere can support annually, which include 1) the maximum number of TCs that the tropical atmosphere can produce for any given episode of TC formation, and 2) the frequency that the new episode of TC formation will occur. The analytical work herein addresses the first question, and the global modelling of TC formation shown in Figure 1 addresses the second question. Given that we plan to have an upcoming study that specifically tackle the second question in much more details, we have removed Figure 1 in this work per your suggestion.

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

Thank you. This is our typo. We really meant 3000 km, not 3x3000 km here. This has been fixed.

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?

Our main point in this discussion is that if all high zonal wavenumbers must be stable as found in this study, then any disturbance corresponding to a large wavenumber (i.e., a small size) that could potentially grow into a TC would not occur. So, only those with $m < 12$ (i.e., their diameters are > 3000 km) can have a chance, which agree well with the typical scale of a region where a TC emerges in the tropical region. Of course, this by no means eliminates the existence of a small TC such as midgets at the higher latitudes, because our analytical results can only provide an estimate for the size of the" hot spot" where a TC disturbance can develop. Talking about the size of a fully-developed TC is beyond our current work, as it involves various complex factors as pointed out in your several recent studies on the topic of the TC size. We have revised this discussion to avoid misleading impression.

Regarding the width of the tropical region, it is indeed hard to be precisely determined. We simply use a typical value of 20^0 for the tropical channel, based on the definition of the tropics up to the tropic of Capricorn (~23.5°). Additional analyses for a few different widths from 15-20^o do not show much difference, because the zonal scale is, after all, always an order of magnitude larger than the meridional scale.

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.

We totally agree. It is entirely possible that that easterly waves here are a mode of the equatorial mixed Rossby waves, because the spatial scale as well as the phase speed are consistent with westward-moving Rossby wave (meridional mode n=1). However, we also wish to caution here that the numerical procedure of finding the unstable mode on the central manifold presented in this section does not allow us to separate different modes of easterly waves. As such, the easterly waves here could be a combination of many different modes of west-ward moving Rossby waves and mixed gravity waves that we may not be able to link them specifically to the equatorial Rossby waves. This has been now mentioned in this revision.

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.

Our discussion in the previous version was indeed unclear, which may cause some confusion here. We would like to note that the periodic state is one of the stable branches of the Hopf bifurcation if the model parameter R is slightly greater than the critical value. As long as R is sufficiently close to the critical number R^*, this periodic state on the central manifold will maintain its stable structure. For a larger R number, it should be noted that the stability of the periodic state may no longer be ensured, because the central manifold function must be re-evaluated and a complex structure of the model state may arise. To some extent, this is what anticipated in the real atmosphere, because not all easterly waves can become unstable and turn into TCs. Only under some certain condition do the easterly waves become unstable.

Technical corrections:

General: Suggest simply using “genesis” rather than “TCG” –acronyms are overused.
Thank you. We have tried to reduce the use of the TCG acronym as suggested.

P4L15: might note in the text for clarity that Delta here represents the Laplacian, which is typically denoted with nablaˆ2
Thank you. The notation nabla has been now defined explicitly.

P4L17: extra “the
Corrected.

P4L21: parenthesis error
Corrected.

P6L11: replace “while” with “though it”
Modified as suggested.

P5L28: “nonlinearity”
Corrected

P7L12: I believe this should be no v-wind component
You are right. We really meant v-wind here. This has been now corrected.

P8L15: "eigenvector"
The typo has been corrected

P9L13: "turns out"
Modified as suggested.

P10L19: "if it exists"
This sentence has been modified

P13L10: "all the same"
Modified as suggested.

P14L16: "nondimensional"
Corrected.

P14L20: issues with the parentheses
Corrected.

P14L24: "obtained from the"
The typo has been now corrected. We thank Reviewer 1 again for your various suggestions and comments.