Responses to Reviewer 1

We appreciate Reviewer 1 for your thoughtful comments and detailed corrections. In this revision, we have taken into account your comments and made several changes including 1) added more discussion to distinguish this work from Wang et al. (W19) and shortened some derivations to remove repetitions, 2) added more discussion to better connect the theoretical and numerical sections, 3) provided additional information on the vertical normal mode initialization as well as a new figure (Fig. 4) to better interpret the wave stationarity, and 4) corrected different typos and inaccurate expressions that you have help pointing out. Please find below our point-to-point responses to your questions and comments (all of these changes are highlighted in the red font so you can quickly follow our revisions).

1. Section 2 is mostly repeating the equations in W19 with very little information about how this part is different from W19. If they are exactly the same, then there is no need to repeat it again as if it is a new result. If not, please clarify the differences and emphasize what's new.

Thank you for your comment on this. While this study shares the governing equation and the bulk of derivations with our previous study (Wang et al. 2019, or W19), we wish to mention that this work focuses on very different aspects of tropical dynamics and waves in global TC formation as compared to W19. Specifically, unlike W19 that examined the dynamical transition and the stability of the ITCZ model, this study focuses on the existence and the structure of planetary-scale stationary waves that can help us understand global TC clustering. In particular, the main results of this study are numerical simulations and analyses that bring new insights into the upper bound as well as the structures of stationary modes governing the global formation of TC clusters, which were absent in W19. This is the reason why we only briefly present the governing equation along with few most important steps in the main text, while moving all detailed derivations to Appendix A to avoid duplication. In this revision, we have modified our theoretical part further to better highlight the difference between W19 and our present study, and streamlined some derivations by referring directly to W19 as you suggested.

2. The connection between theoretical and modeling part is not stated clearly. The current layout gives me a very abrupt transition from Section 2 and Section 3. For example, what is the connection between the imposed ER wavenumber in WRF simulations and theoretically derived K bound; what are the associated magnitude of mean flows, Rossby number, Ekman number and Rayleigh number in the WRF simulations and the corresponding theoretical value of PSW wavenumber from the theory?

Your comment is well-taken. Although one can obtain all theoretical numbers such as the Ekman number, the Rossby number, or the Rayleigh number from model simulations, we wish to note that these theoretical numbers are derived from a highly simplified ITCZ model under idealized settings. Also, they are defined only under the strict conditions of 2D dynamics with the specific Kolmogorov forcing given by Eq. (2). This is the reason why we have to use the WRF model to further examine the stationary waves and their role in global TC clustering instead of using theoretical results. For the full-physics WRF model, the tropical dynamics is no longer reduced and so the exact value of those theoretical numbers will be less significant. What more significant is, however, the broad implication obtained from the ITCZ model, which suggests 1) an upper bound for stationary zonal wavenumbers, and 2) the meridional structure that stationary modes must possess. These properties are global dynamics and so expected to hold true from a large-scale perspective, much like the TC eyewall structure or warm core are the intrinsic properties of TCs that any model should capture, regardless of their exact magnitude. This explains why we choose 2 different tropical waves with different zonal wave number to valid these aforementioned broader results in the WRF model, but have not provided detailed analyses of all nondimensional numbers. This comment is indeed important and so we have added more discussions about this issue in this revision. We hope it could address your concern.

Technical corrections:

1. L121-124: expand this part to clarify the differences between the theoretical analysis in this manuscript and W19.

More discussions have been added.

2. L127: subscription missing in the streamfunction expression: 'LU_0' -> 'L_yU_0'

This typo has been fixed.

3. L123: a new character 'I' is introduced, which is exactly the same as A in W19. Why not adopt the same character?

We use '*I*' here to avoid confusion with W19's expression '*A*' in the derivations presented in Appendix A.

4. L150: 'max' on the exponent is a little misleading, I though it was taking some maximum value…

We have changed 'imax \pi' to 'ima \pi x' to avoid confusion.

5. L168-169, L363: a~0.06 and K~12 in W19, please be consistent.

Thank you for your detail checks. We have changed to a~0.06 and K <= 12

6. L250 and below: V20 is not defined. And I only found Vu et al. 2021.

This is our typo. We have now corrected this citation to Vu et al. 2021 (V21) in line 251.

7. In the table 2, the last two rows are mismatched.

This has been fixed.

8. L271-272: I think the purpose of the paper is to show with no land it is possbile to have PSW by earth internal dynamics, but here it reads very contradictory.

Our discussion here was unclear. What we really meant is that the external wave forcing is needed to excite tropical waves in the aqua-planet setting. Without realistic terrain, this external wave forcing is a reasonable mechanism that we can force the model to see whether these waves can maintain their stationary structure or not. This paragraph has been revised to make it clear.

9. L275: 'no significant impacts' on what?

We have added 'no significant impacts on TC formation' to this sentence.

10. The criteria in Table 3 are used according to Vu et al. 2021, should state this in text 'refer to Vu et al. 2021 for more details'

This clarification has been added.

11. For the numerical results, are the EK results the same as the ones in Vu et al. 2021?

The EK simulations in this study have been designed to match with Vu et al. 2021, and so yes we obtain similar results for EK waves as in V21.

12 Do the ER/EK waves imposed in the WRF model have seasonality? Can you show the seasonality?

Thank you for the question. We do see the seasonality of both ER/EK waves stirring mechanisms as shown in Figs 5, 6 and 7 in the manuscript. Please note that red dots in Fig. 5 correspond to TCs that form in the Northern Hemisphere summer, while blue dots are for the Southern Hemisphere summer. We have added more discussions here to better highlight the TC seasonality from our aqua-planet configuration.

13. Figure 5: it's very interesting to see the TC numbers differ in different ER experiments while the number keeps quite stable in EK experiments. Can you discuss a little bit why?

For EK wave experiments, the wave structures quickly dissipate upon being introduced into the model, which can be seen in Fig. 2. Therefore, TCs can form anywhere within the tropical region, and different wavenumbers of EK waves do not seem to influence the total global number of TCs.

For ER wave experiments, the wave structures do not fade way but they are well-maintained after introduced into the model. These different waves generate different hot spots for TCs to form (cf. Figs. 6 and 7). As such, the locations for TC formation are now governed by ER waves, leading to different total global number of TCs for different zonal wavenumbers. We have added this discussion in the revised version per your comment.

14. L339: missing ' in 'don't have'

This typo has been fixed. Thank you again for your comments.