Responses to Reviewer 2

We wish to thank Dr. Paul Roundy for your constructive comments. In this revision, we have followed your suggestions and modified our work as follows: 1) provided additional information on the vertical normal mode initialization and added new Figure 4 to better interpret the wave stationarity as you suggested, 2) added more discussion to distinguish this work from our previous work (Wang et al. 2019) and streamlined some of our derivations to avoid redundancy, 3) included further discussion to connect our theoretical and numerical sections, and 4) corrected several typos and/or inaccurate expressions. Below please find our point-to-point responses to your concerns. For your convenience, all of our modifications are highlighted in the red font so you can quickly follow our changes in this revision.

1. Although vertical normal modes are a popular way to build simple models of convectively coupled waves, in the real atmosphere and in numerical models, they may not exist independent of the effects of coupling of waves to convection (which drives overturning circulations limited in vertical extent at the tropopause). Real waves, even convectively coupled ones, typically propagate vertically and the tropopause is not a limit to their movement. This fact implies that initializing a model with idealized normal mode waves will result in the model having to move toward a state consistent with its internal dynamics. This point might not refute the authors' overall arguments because initializing the model with a wave disturbance of the same type but more consistent with the model's native form of the wave, might still result in a similar outcome to what they showed.

Thank you for your insightful about the vertical normal mode for tropical waves. Yes, we are aware of this issue and in fact have adopted the same approach as in our early study (Vu et al. 2021) in which we have tried different wave initializations and run the model for 1 year as a model spinup so that the model could establish its own dynamics consistent with the physical options, boundary conditions, and domain setting. All of the analyses are then carried from the second year such that the impacts of normal mode initialization are minimized. As reported in Vu et al. (2021), this 1-year spinup is sufficiently robust in the sense that the key annual properties of the tropical atmosphere are consistent among subsequent years, regardless of period we have analyzed. In this regard, the issue with vertical normal mode adjustment is expected to be transitional and may have small effects on the overall model outputs as you also noted.

Another way to address this issue more conclusively is to use a very high model top along with many different vertical resolutions to examine the model sensitivity. This approach requires however additional sensitivity analyses such as different vertical wave profiles or model top boundary conditions to ensure the model stability, which are however beyond our computational resources at present. In this revision, we have provided in Section 3 some additional discussions related to vertical normal modes so readers are aware of this potential problem in our numerical simulations.

2. The authors' analytical solution suggests that stationary waves might occur in the tropics, if the assumptions of the simple model apply in nature. Yet nature can yield stationary waves through other mechanisms. The leading one is probably forcing by regional SST anomalies, interaction with topography, etc., which their model set-up would not include (as the authors already explain). Other possible sources of stationary waves include waves that would otherwise propagate, but whose propagation is balanced by advection. In a model environment that includes a steady background state flow, it's conceivable that such signals could occur with stability. In nature, this kind of steady basic state is implausible, because sea surface temperature patterns vary over time. I recommend that the authors analyze their model basic state for conditions that could lead to such stationary advection-balanced propagation for Rossby waves. It may be that the model background flow explains why Rossby waves can become stationary in the model but Kelvin waves cannot. In order for Rossby waves

to be stationary under conditions of balance by advection, they must be non-dispersive. This would place a control on which scales of the waves would be favored by this mechanism to become stationary.

We agree. In fact, there are several different pathways to trigger large-scale tropical waves such as terrain forcings, land-sea interaction, or ocean coupling that we are not able to capture with the theoretical and numerical models presented in this study. We did mention these potential mechanisms in our previous version (see, e.g., the second to the last paragraph of Section 1 or the last paragraph of Section 3 in the original submission), which explains why we have to introduce the Kolmogorov forcing to mimic those external pathways that the aqua-planet settings cannot capture.

One thing that we wish to take this opportunity to clarify further is that while the ITCZ model could suggest a potential stationary mode as shown in our study, there is no guarantee that this mode must exist in nature due to different large-scale factors that could break the stationarity as you correctly pointed out, even within the idealized framework. In fact, this study is built on our previous works (Wang et al. 2019, Pan et al. 2021), which presented more detailed analyses of the dynamical transition, stability, and bifurcation of stationary solutions. Any change in background flows, ocean coupling, or tropical wave types could break the stationarity, which is why we use the WRF model to search for the stationary modes as presented in this study, instead of relying entirely on the theoretical results. To our knowledge, the modelling approach is the best way to look for such stationary modes in the presence of full physics.

Regarding your suggestion of analyzing background flow for the EK and ER experiments, we concur that the background flow is one of the key factors that decide the stability of stationary solutions (this can be seen directly from the Rayleigh number defined below our Eq. 5). Any change in the background will modify this Rayleigh number and lead to the transition to a new state. Per your comment, we have now added Fig. 4 in this revision, which compares the mean flows between ER and EK experiments. As can be seen in the new Fig. 4, EK waves are indeed located in purely positive gradient zonal wind regime (i.e., $\partial\bar{U}(y,t)/\partial y > 0\ \forall y$) , which does not satisfy Rayleigh's barotropic instability criterion. Thus, all EK waves introduced into the model cannot intensify. ER waves, on the other hand, exist in the barotropically unstable zone ($i.e., \exists\ y_0 \mid \partial^2\bar{U}(y_0,t)/\partial y^2 = 0$), and so it supports the wave growth according to Rayleigh's theorem. That is, any disturbance introduced into this background could grow and maintain its structure subsequently.

Of course, the instability of any stationary mode depends not only on the mean state but also on other parameters such as latitude, level, or convergence/divergence (in our ITCZ model, the convergence/divergence is represented by the Kolmogorov forcing). Because the stability and related dynamical transition of stationary modes have been examined in much more details in our previous works, we don't repeat them here. The key point that we wish to highlight in this study is that the wave instability is realized only for certain waves with low zonal wavenumbers, which are captured in our ER experiments and support the clustering of global TC formation. This discussion has been now included in our revision per your suggestion. We hope that this could address your concern and thank you again for your comments and insights.