Reply to anonymous Referee #1

Review of Yamazaki et al. On Tropospheric Pathway for QBO Influence on Stratospheric Polar Vortex

This paper presents evidence for the QBO having an influence on the stratospheric polar vortex (the Holton-Tan effect) via the troposphere, rather than via the stratosphere as in previous proposed mechanisms (though they show that other mechanisms are also at work, particularly in late winter). The idea is that the QBO-E stimulates convection in the tropical West Pacific and suppresses it in the western Indian Ocean, and these produce Rossby wave forcing that constructively interferes with the extratropical Rossby wave structure, and hence increases planetary wave forcing of the stratospheric vortex. This is based on observations of differences in tropical convective activity in different QBO phases and model simulations used to estimate the impact of this on extratropical wave activity. Overall I think the sequence of experiments holds together well and I like the tests done to check that the ENSO phase is not a strong confounding factor. I think a few more diagnostics are needed to show that the findings are robust.

We appreciate Reviewer #1 very much for the constructive comments and suggestions. We have carefully incorporated comments and suggestions, which have improved the manuscript in its content and presentation. Our responses to the specific comments can be found below in black (Reviewer #1's comments and suggestions) and blue (our responses).

Most significant comments:

1. Some diagnostics need to be shown to explain why the mechanism does not seem to occur from January onwards – has the apparent difference in tropical convection disappeared (which would suggest to me that it's not robust) or is the forcing of extratropical planetary waves not effective for some reason?

> Thank you. We think the stratospheric pathway is effective in midwinter, in short. We made an analysis on wave amplitudes and the results are now included in the revised version. The following sentences are included in the revised version.

"Why the tropospheric mechanism does not seem to occur from January onward? Is this because QBO-induced tropical convection anomalies disappear in midwinter? We examined the observed tropical convection difference between EQBO and WQBO for each month and found that it does not disappear but shifts slowly eastward. We then made simple diagnostics on seasonal change in observed wave amplitudes (Fig. 17). At 250 hPa, from September to November, wave-1 amplitude in EQBO is larger than that in WQBO at around the maximum latitude. This means that the maximum wave-1 amplitude is enhanced. In December, wave-1 amplitude in EQBO is enhanced in high-latitudes. Although this high-latitude enhancement continues to March, wave-1 amplitude at the maximum latitude of 50°N is reduced and no wave-1 amplitude enhancement in the troposphere is seen from January onward. On the contrary, the stratospheric polar vortex in EQBO weakens more in January (Figs. 1a and 16b). This corresponds to an enhancement of wave-1 amplitude at 100 hPa (Fig. 17f). Apparently, wave-1 amplitude in EQBO becomes larger than that in WQBO from November to February. For wave-2, the seasonal march at 100 hPa and that at 250 hPa are similar (not shown). We suppose the stratospheric processes discussed in many previous studies can account for the mid-winter Holton-Tan relationship. In mid-winter, the stratosphere undergoes vacillation without changes in the troposphere (Holton and Mass, 1976; Chen et al., 2001; de la Cámara et al., 2019). "

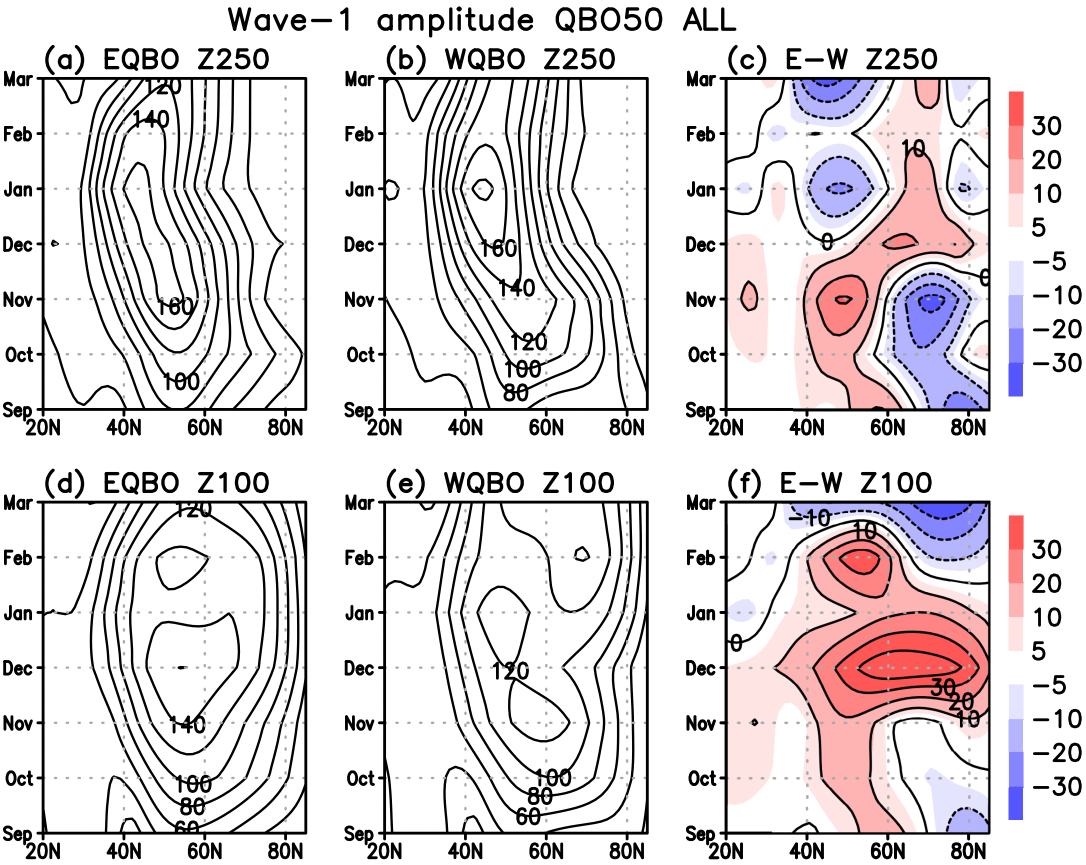


Figure 17. Latitude-month plots of observed wavenumber-1 amplitude. (a) EQBO composite at 250 hPa. (b) WQBO composite at 250 hPa. (c) Difference between EQBO and WQBO. (d)-(f) Same as in (a)-(c) but for 100 hPa. Contours in (a),(b),(d),and(e) are 20 m, and those in (c) and (f) are 10 m.

Holton, J. R., and C. Mass, 1976: Stratospheric vacillation cycles, J. Atmos. Sci., 33, 2218-2225, https://doi.org/10.1175/1520-0469(1976)033<2218:SVC>2.0CO;2.

Chen, M., Mechoso, C. R., and Farrara, J. D.: Interannual variations in the stratospheric circulation with a perfectly steady troposphere, J. Geophys. Res., 106, 5161-5172, https:// di.org/10.1029/2000JD900624, 2001.

de la Cámara, A., T. Birner, and J. R. Albers, 2019: Are sudden stratospheric warmings preceded by anomalous tropospheric wave activity? J. Climate, 32, 7173-7189. DOI:10.1175/ JCLI-D-19-0269.1.

2. L149 – it would be helpful to give some explanation of why tropopause temperature anomalies associated with the QBO would "provide favorable conditions for enhanced convective activity", with references. Having confidence that the QBO really has the impact on tropical convection that is shown is crucial for believing the mechanism, so this would be useful.

> Thank you. Although we do not know precise mechanisms by which negative tropopause temperature anomalies provide favorable conditions for enhanced convective activity, we suspect weak stability and subsequent increase in cloudiness in the tropical tropopause layer (TTL) are the

main two key elements. In addition feedback arising from cooling in the TTL and warming in the mid-troposphere by cloud longwave forcing may farther accelerate weak stability, thereby enhancing the convective activity, as noted by Giorgetta et al. (1999) for boreal summer season.

So we added the Giorgetta et al (1999) in the references and explanation is added in the revised version.

Peña-Ortiz et al (2019) examined QBO influence the tropical convection and showed QBO modulation of the tropical convection that impacts stationary waves and the polar vortex of the austral winter of the southern hemisphere.
We added this paper in Introduction and references.

Giorgetta, M. A., Bengtsson, L., and Arpe, K.: An investigation of QBO signals in the East Asian and Indian monsoon in GCM experiments, Climate Dyn., 15, 435-450, 1999.
Peña-Ortiz, C., Manzini, E., and Giorgetta, M.: Tropical deep convection impact on southern winter stationary waves and its modulation by the Quasi-Biennial Oscillation, J. Climate, 32, 7453-7467. DOI: 10.1175/JCLI-D-18-0763.1, 2019.

3. The authors note that the impact of the QBO on tropical convection is associated with a changed strength of the Walker circulation. Misios et al. (2019, "Slowdown of the Walker circulation at solar cycle maximum", PNAS) found that there is an impact of the solar cycle on the Walker circulation, so this could be a confounding factor. It would make sense to check that the impact of the QBO on tropical convection found here is not dependent on the solar cycle phase.

> Thank you for the comment on solar cycle. Given possible compounding influences we further examined solar cycle modulation of the QBO impact on tropical convection found in our paper. The following is the results of our analysis which show only small impacts from the solar cycle and the robustness of our main conclusions. We added the analysis in the revised version as Section 3.7 and figures in Appendix as Figs. A2 and A3.

For the analysis we used time-series of the monthly and Nov-Feb mean sunspot number. The sunspot number data have been obtained from the World Center for Sunspot Index and Long-term Solar Observation (WDC-SILSO), Royal Observatory of Belgium, Brussels (http://www.sidc.be/silso/datafiles; Clette et al. 2014).

We added Section 3.7 as follows

3.7 Modulation by 11-year solar cycle

It has been known that the Holton-Tan relation is modified by the 11-year solar cycle (Labitzke, 2005, and references therein). Recently, Misios et al. (2019) provided strong evidence for weakened Walker circulation at the solar maximum. Recognizing possible compounding influences by the solar cycle on the QBO impact on tropical convection and extra-tropical circulation anomalies as discussed in our paper so far, we have made additional composite analysis as follows.

We used the Nov-Feb mean sunspot number as a solar index (SSN; Fig. A2), whose average value is 92.2. Winters above (SSN>92.2) and below (SSN<92.2) the average are classified as solar max and solar min winters, respectively. We also divided winters into EQBO, WQBO composites, and other winters as described in Section 2.2 (see Table A1 for the sample size of each category). As identified in Misios et al. (2019), the solar impacts on convective activity thus the Walker circulation have one to two years of time lag through the bottom-up mechanism. We thus shifted by one year when classifying solar max and min winters. This sampling scheme provides consistent results with theirs on the solar influence, i.e. stronger Walker circulation at solar minimum seen in Figure A3c.

The QBO signal (EQBO minus WQBO) in OLR is stronger in the solar min years with significantly enhanced convection over the western tropical Pacific. In the solar max years, enhanced convection over the western tropical Pacific is weaker and shifts eastward slightly. Despite some differences, the QBO signal characterized by enhanced convection in the western tropical Pacific is commonly found in both solar max and min composites.

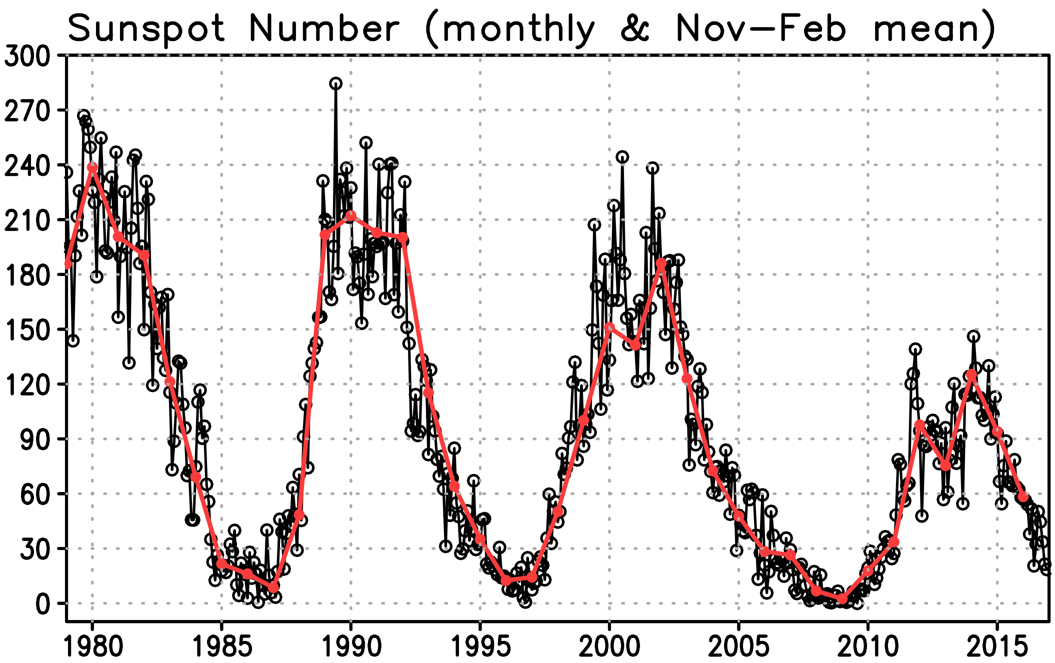


Figure RC1.1. (new Fig. A2) Monthly mean sunspot number (**black** with open circle) and November-February mean (red with closed circle).

Table RC1.1. Number of EQBO, WQBO, Solar max, solar min and other years

	EQBO	WQBO	Others
Solar Max	7	7	4
Solar Min	5	12	2

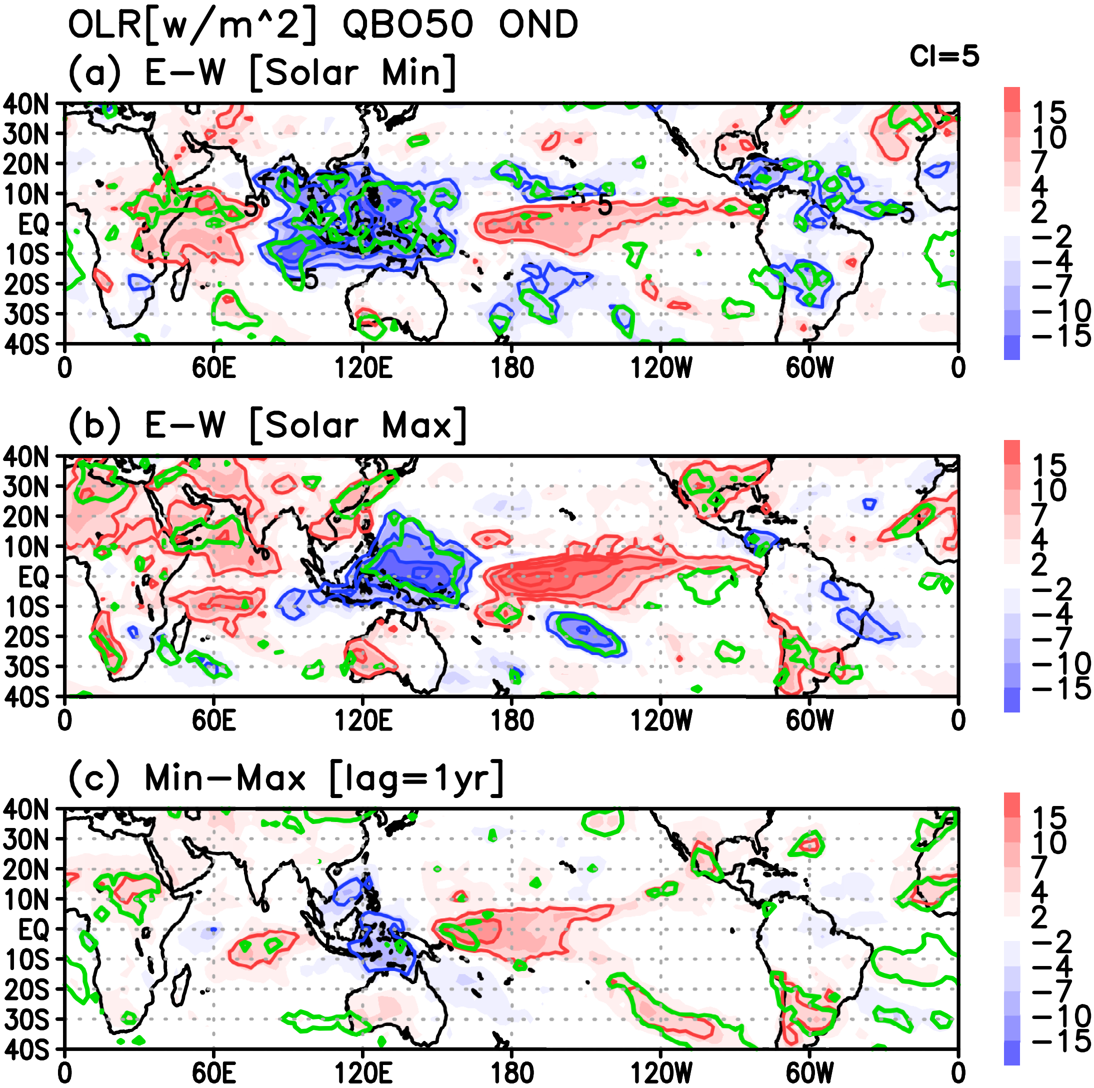


Figure RC1.2. (new Fig. A3) (a) October-November-December (OND) mean OLR differences between EQBO and WQBO winters for solar minimum winters. (b) Same as (a) but for solar maximum years. (c) OND OLR differences between solar minimum and maximum winters with 1-year lag. Green line denotes the statistically significant value at 95% confidence level.

References

Clette, F., L. Svalgaard, J. M. Vaquero, and E. W. Cliver, 2014:Revisiting the sunspot number: A 400-year perspective on the solar cycle. Space Sci. Rev., 186, 35–103.

Labitzke, K., 2005: On the solar cycle-QBO relationship: a summary, J. Atmos. Solar-Terr. Phys. 67, 45-54.

Misios, S., L. J. Gray, M. F. Knudsen, C. Karoff, H. Schmidt, and J. D. Haigh, 2019: Slowdown of the Walker circulation at solar cycle maximum, PNAS, 116, 7186-7191.

4. What method is being used for the statistical significance tests? What assumptions are being made for this? What has been done to check the assumptions are reasonable? This should be

clearly explained in the text. (Personally, I think using a bootstrap method is best as it requires relatively few assumptions to be made, but another method can be used if the assumptions behind it can be justified.)

> Thank you. Throughout the manuscript we used t-statistics for all significant tests. Data is assumed to be normally distributed with no significant serial correlation. For example, auto-correlation of the OND averaged OLR over the western tropical Pacific (130-160E, 0-10N) with 1-year time lag is -0.23. Since the variation in this quantity is so central to our claim on the QBO impacts we examined its distribution (see Fig. RC1.3.) for EQBO and WQBO. The mean OND-averaged OLR for EQBO is 218.6 W/m^2, and 229.2 W/m^2 for WQBO where the average for whole 37 years is 224.6 W/m^2. We further employed the bootstrap method (n=1000000) to test its significant. The p-value is 99.97% against the observed difference of 10.58 Wm^{-2}.

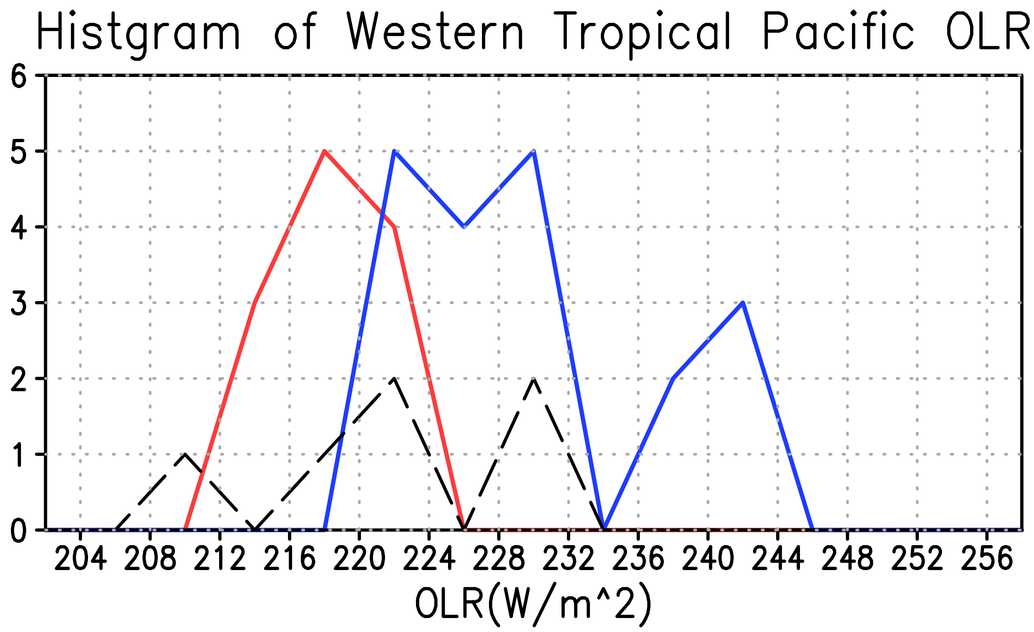


Figure RC1.3. Histgram of western tropical Pacific OLR for October-December mean. The interval of bin is 4 W/m^2. Red line denotes EQBO winters, blue line denotes WQBO winters, and black dashed line denotes other winters.

Other comments:

1. L32-35 It should be made clear that the “Holtan-Tan mechanism” is just one proposed mechanism.

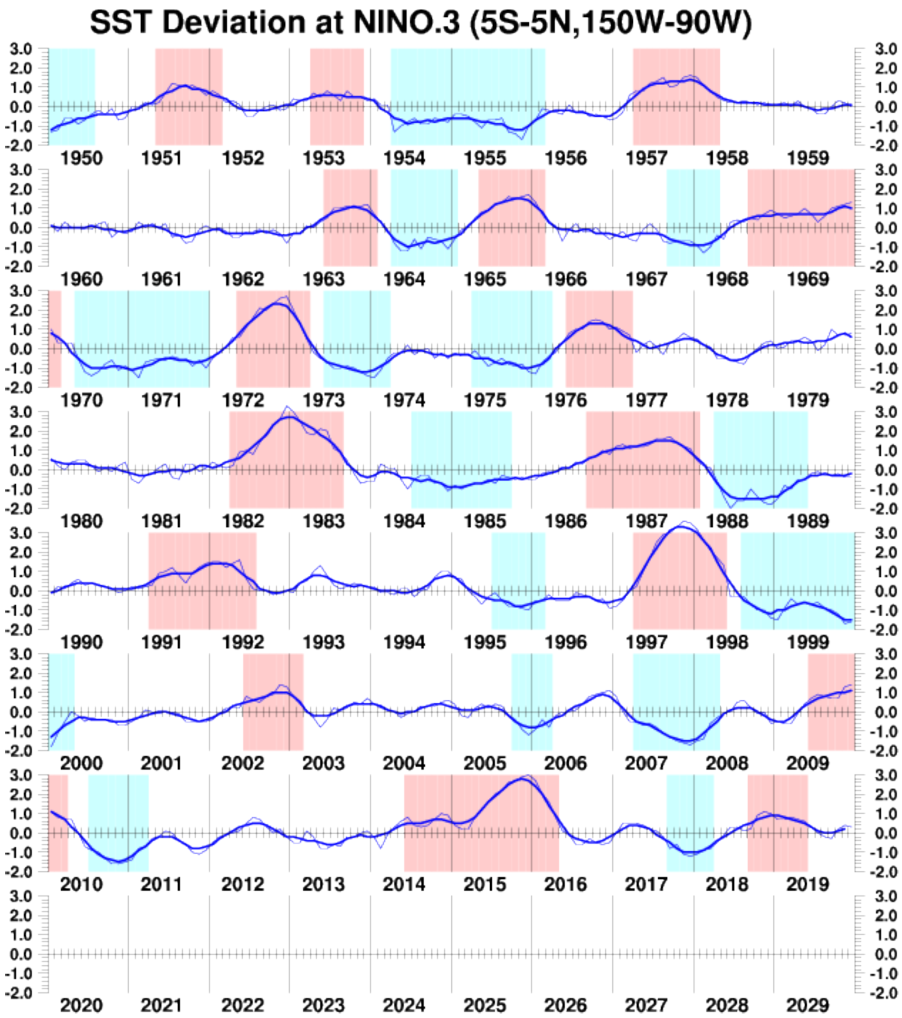
> Thank you. We modified the sentence following your suggestion and reviewr#2’s suggestion.

“Holton and Tan (1980, 1982) only showed a plausible mechanism, as the latitudinal position of the zero-wind critical surface of stationary Rossby wave is primarily controlled by the equatorial QBO. Recently, Watson and Gray (2014) posted this line of discussion with their model. Naoe and Shibata (2010) analyzed Holton-Tan relationship by a QBO-producing chemistry-climate model (CCM) and reanalysis data. They showed the conventional critical latitude mechanism that the equatorial winds in the lower stratosphere acted as a waveguide for planetary wave propagation did not hold. White et al. (2015) suggested the enhanced upward wave propagation at mid-latitudes due to the enhanced wave growth rather than the critical latitude mechanism, explaining the QBO-related change in mid-latitudes as well as the polar vortex change in high-latitudes.”

2. L80 The definition of ENSO phases used by the JMA should be given for clarity.

>Thank you. “The definition of ENSO used by the JMA is based on 5-month moving averaged SST deviation from the standard value at NINO.3 (5°S-5°N, 150°W-90°W). When the SST deviation experiences more (less) than +0.5K(-0.5K) over 6 consecutive months, it is defined as El Niño (La Niña). The standard value is defined by previous 30-year mean for each month.”

Above sentences has been added. The following is the figure from JMA for reference.



El Nino (pink) and La Nina (blue) periods defined by the JMA.

3. L90 It should be made clear that the “QBO signal” refers to the EQBO minus WQBO difference (rather than the difference from climatology).

> Thank you. We added the following sentence.

“Here, the QBO-signal refers to the EQBO minus WQBO difference rather than the deviation from climatology.”

4. L104 References are given claiming to show that “the performance of the model in the stratosphere is satisfactory”, but I couldn’t see an evaluation of the stratospheric performance in any of these references. Please point out where this can be found, give a reference showing this, show data yourselves or remove this remark. (I am not too bothered about the model’s stratospheric performance on the whole – for simulating the wave forcing from the tropical convection anomaly, it is the tropospheric performance that matters most I think, and it looks adequate from the figures given – I say this just so a lot of work is not done to validate the model’s stratospheric performance.)

> Thank you. The stratospheric performance does not matter in this paper. So we deleted the sentence. Also we deleted references (Jaiser et al 2016 and Hoshi et al 2019).

5. L134-5 What’s the relevance of the remark about vertical motion near the Equator being downward?

> We would like to have shown the QBO-signal is not zonally uniform. But, it is not so relevant and showing Figs.3a and 4a is enough. So we deleted the sentence. Thank you.

6. L167-70 Plots of the wave amplitudes as a function of latitude in the different QBO phases would be helpful.

> Thank you very much for the suggestion.

Latitudinal profiles of wave-1 and wave-2 amplitudes for EQBO and WQBO are shown below (Figure RC1.4; new Figure 9). “Peak values of the wave amplitude in November increase in EQBO for both wave-1 and wave-2 and regardless of in/exclusion of ENSO years.“ The figure and above sentence are added in the revised version. Also description for wave-2 was modified.

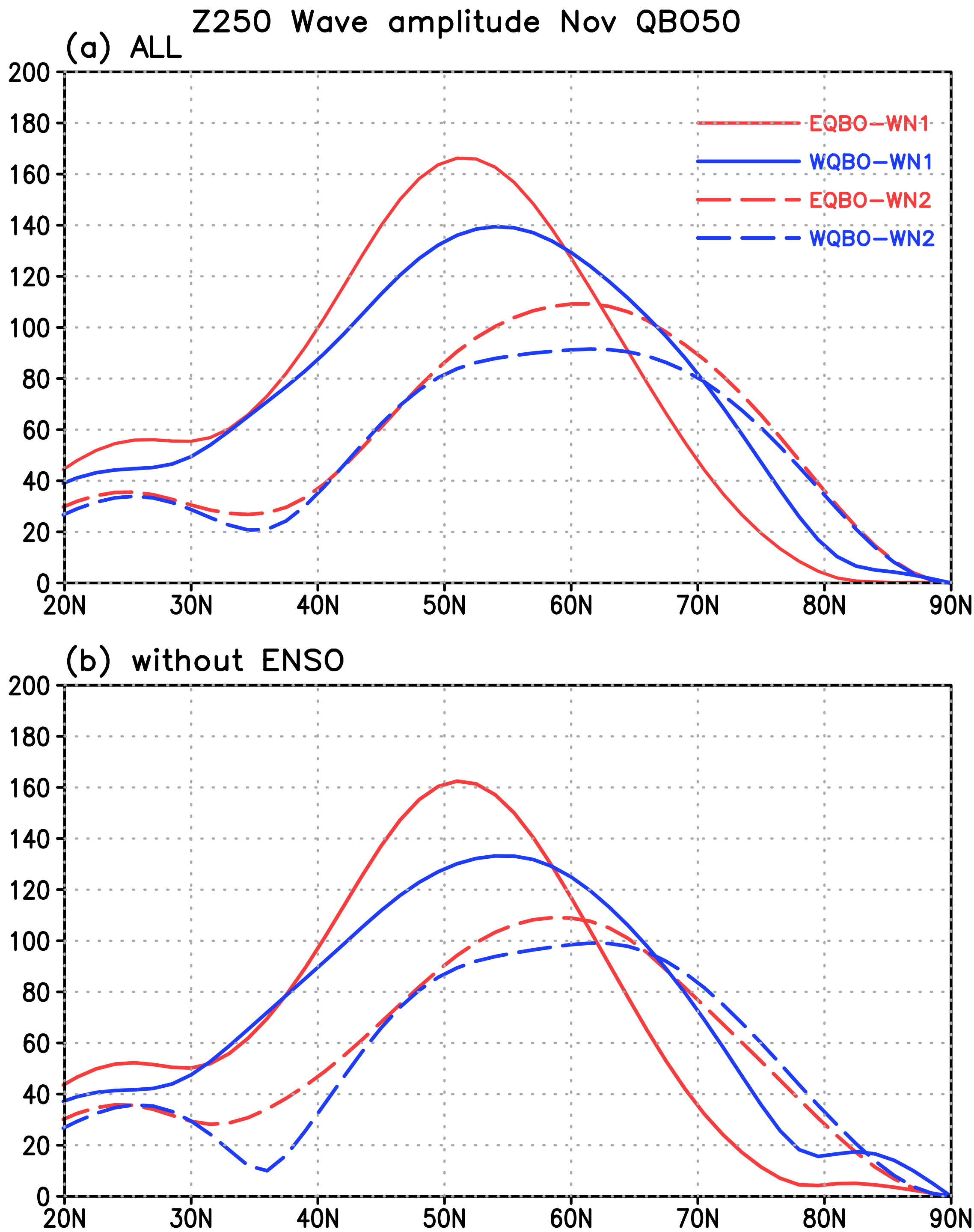


Figure RC1.4. (new Fig. 9) Wave amplitudes at 250 hPa as a function of latitude in the different QBO phases for November. Red (blue) solid line denotes wave-1 in the EQBO (WQBO) composite. Red (blue) dashed line denotes wave-2 in the EQBO (WQBO) composite. Y-axis denotes amplitude in m. (a) All composite. (b) Composite without ENSO winters.

7. L183 It seems worth noting that the wave-1 response to the convective forcing shows a large signal in the North Atlantic that is not shown in the full response in fig.9. Is the right interpretation that higher wavenumbers are cancelling out the response in the North Atlantic?

> Thank you. Indeed, the full response in (original) Fig. 9 in the North Atlantic shows positive anomalies, but the magnitude is small. On the other hand, negative anomalies over the North Pacific are large in the full response. Wave number decomposition makes wave-1 amplitude large. However, we consider the full response as more representative here, and wave-1 response is somewhat artificial, though the wave number decomposition is critical in evaluating wave propagation into the stratosphere.

8. L218-9 This could do with being more quantitative e.g. compare the simulated and observed wave-1 amplitude change averaged over 30-60N.

> Thank you for the suggestion.

Change of wave-1 amplitude average over 30-60N is as follows.

Observed E-W :	14.3 m
CONV1-CNTL:	12.0 m
CONV2-CNTL:	10.7 m

The simulated values are similar to the observed value.
We made the amplitude plot which is quite informative. We add the figure and the following text in the revised version.

"Wave amplitudes at 250 hPa for all simulations are shown in Fig. 15. Compared with the observed QBO difference (seen as EQBO minus WQBO in Fig. 9), simulated differences between CONV1 (CONV2) and CNTL are similar in magnitude and latitudinal profile. For example, simulated wave-1 amplitude averaged over 30-60°N is 12.0 m (CONV1 minus CTRL) and 10.7 m (CONV2 minus CTRL), which is in good agreement with the observed difference of 14.3 m between EQBO and WQBO. Wave-1 amplitude is also peaked at around 55°N for all cases. We also confirm that convection over the tropical western Pacific is most significant for enhanced extratropical planetary wave."

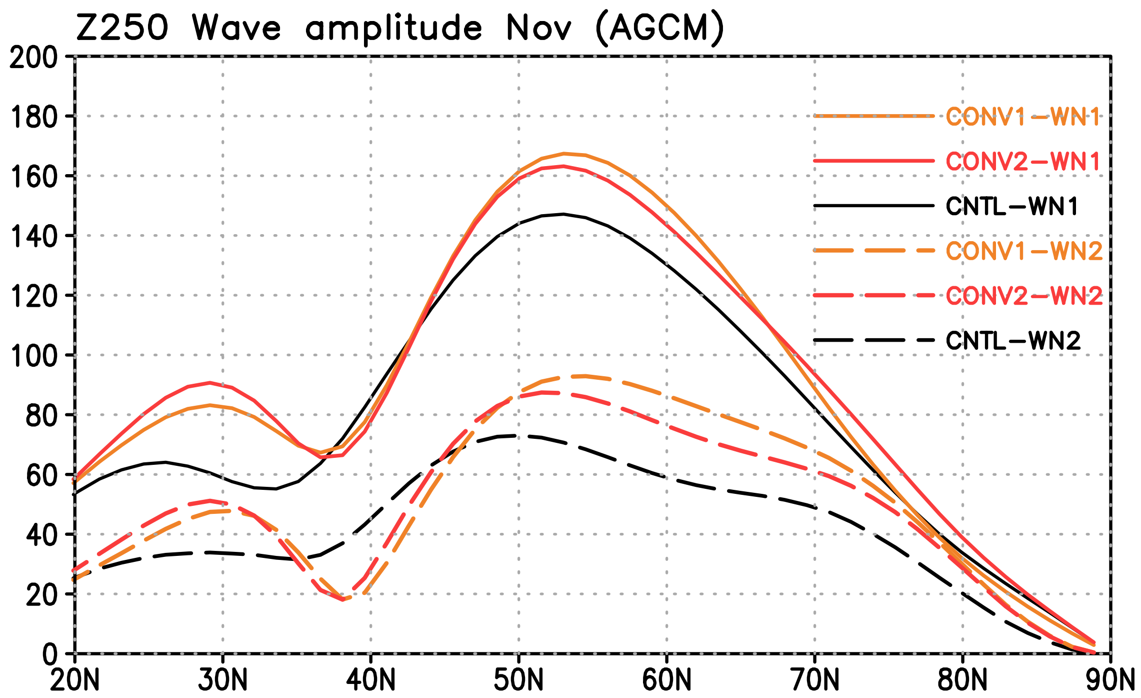


Figure RC1.5. (new Fig. 15) Latitudinal profile of the wave amplitudes at 250 hPa in AGCM simulations for November, based on 60-year mean. Black lines show the control simulation, orange lines for CONV1 simulation, and red lines for CONV2 simulation. Solid and dashed lines denote wave-1 and wave-2 components, respectively. Y-axis denotes amplitude in m.

9. L223-6 The text could do with being clearer about which results are from observations/reanalysis. (This goes for other parts of the results section as well.)

> Thank you. "Blue lines are based on reanalysis data and red lines from the simulated results."
We added the above sentence.
Below sentence was added for Fig. 9.
"Red lines are based on EQBO composites and blue lines from WQBO composites."

10. Appendix B – is there a reference supporting this method?

> We are not aware of specific references. The method is a crude approximation. But, we think the horizontal advection is negligible and the balance between diabatic heating and vertical motion is kept in the tropical troposphere on a monthly time scale.