The authors are thankful to the reviewer for providing valuable comments and suggestions to improve the quality of this paper. We have incorporated all comments to the maximum extent in the revised manuscript. A point by point reply is given below:

Major Comments

1. **Incomplete and selective literature review**

- The Introduction omits several key works on monsoons as well as QBO impacts on convection and precipitation.
  The authors frequently argue that the global monsoon is a land-sea contrast phenomena, overlooking recent literature on the monsoons as a results of energetics and solar forcing (Bordoni and Schneider, 2008; Biasutti et al., 2018; Geen et al., 2020), which include papers they cite on their manuscript (Wang and Ding, 2008). If the authors choose to delineate the global monsoon as the ocean-land thermal contrast only, they should at least mention other existing views and explain why they choose this approach. I realize this is only tangentially related to the whole manuscript but it's an important distinction.
  Then, the authors provide only a handful of references and insight into the tropical connection between the QBO and the surface. For example, they cite Yasunari correctly as a paper that relates the QBO to the Walker circulation strength but they fail to also mention more recent work that has found similar evidence for this relationship using newer longer records (Huang et al., 2012; Hu et al., 2012; Garc´ıa-Franco et al., 2022). Literature on observed QBO relationships with the tropical atmosphere (Lee et al., 2019), as well as literature on potential mechanisms, including a QBO-ENSO relationship, is also not cited (Nie and Sobel, 2015; Garc´ıa-Franco et al., 2023; Rodrigo et al., 2025).
  Reply: The introduction has been revised after incorporating all the above suggestions and citations. We believe that the revised introduction is complete and addresses the gap left by the omission of the literature review.

Mechanistic explanation not substantiated

- The authors propose two routes of QBO influence: a Holton–Tan stratospheric pathway in DJF and a tropical UTLS–convection pathway in JJA. However, no direct diagnostics (e.g., tropopause temperature, OLR, wave activity fluxes) are provided to substantiate these claims. What is the main mechanism that relates the QBO to tropical convection? Stability, or shear, or both?
  Reply: We propose that the UTLS pathway operates during all seasons and that the H-T mechanism does not operate in summer. For the UTLS pathway we argue that QBO MMC temperature anomalies influence convective regions, which then affect local precipitation and radiation of planetary wave trains along a great circle route. We show effects on temperature, geopotential height, winds, and precipitation which are consistent with QBO modulation of the PNA and NAO patterns via this mechanism. We do not show wave activity flux. However, the teleconnection argument makes use of known mechanisms: modulation of tropical convective centers, modulation of Rossby wave source, modulation of Rossby wave train, modulation of extratropical centers of action. We are discussing statistically significant examples of this process. We show that the summer QBO circulation anomaly is likely related to the MMC temperature cold anomaly in the subtropics, which is the UTLS pathway, and that the H-T pathway does not exist in the summer. We cannot determine during boreal winter what the relative contributions of the UTLS pathway and the H-T / annular mode mechanism. The main mechanism that is emerging is that UTLS temperature anomalies are important. In the tropics there is a coincidence of reduced shear and cold anomaly geographically during QBO E (Hitchman et al. 2021), so they tend to co-vary.
  As part of our continuing research, we are conducting three separate detailed follow-up studies focusing on individual regions to explore the underlying mechanisms in greater depth.

- The claim of "linear progression" of QBO impacts across phases is descriptive only. Without quantitative regression or symmetry diagnostics, this interpretation remains speculative.
  Reply: We find that the anomaly patterns are similar but of the opposite sign for QBO E and QBO W for the statistically significant circulation anomalies that we are focusing on in this study. This is consistent with a linear relationship between QBO phase and phase of response in the teleconnection. However, you are right that we did not demonstrate any sort of "progression", rather that the two end points are consistent with linearity, so we removed "progression".

3. Treatment of ENSO is insufficient

- The exclusion of "extreme" ENSO months using Niño-3.4 thresholds (±1 K) is not, in my opinion, an adequate control. Previous studies have shown important aliasing between ENSO and the QBO that is not easily dealt with (Domeisen et al., 2019). Authors should at least show that in their remaining data, the sampling between ENSO and QBO phases is symmetrically distributed, so that readers know that the results are not simply due to having more El Niño's during a certain QBO phase.
  Reply: We appreciate your concern regarding the possible influence of ENSO bias. In consistency with our previous studies (Hitchman et al., 2020; Kumar et al., 2022, 2024; Yoden et al., 2023), we adopted a simple approach of calculating direct composite differences between the QBO opposite phases. Any month has been excluded from QBO W and E composites for Niño 3.4 index thresholds (±1K). Using the same threshold value in each composite for evaluating ENSO neutral conditions helps to avoid El Niño related biases during a certain QBO phase.
- The manuscript mentions that similar results were obtained with a ±0.5 K cutoff, but these results are not shown. Authors should provide several figures to substantiate their claims.
  Reply: In response to the concern raised by the other reviewer and to maintain consistency with our previous study (Kumar et al., 2024), we have adopted a cutoff value of ±0.4 K. The corresponding figures are provided as supplementary information wherever necessary. For instance, when discussing the possible mechanisms for QBO – PNA teleconnection, Fig. S3 has been included for the ±0.4 K case corresponding to Fig. 9. We find that the patterns which we discuss are very similar using the lower threshold.
- A more robust approach would be to regress out ENSO (and ideally other low-frequency modes such as IOD/PDO) and test the sensitivity of the results. Authors may follow Gray et al. (2018) for this approach. Without such checks, the attribution to QBO remains uncertain.
  Reply: The multivariate regression of QBO W–E differences was the main technique employed by Gray et al. (2018). Their sensitivity tests showed that the QBO regression coefficients were unaffected by the inclusion or exclusion of solar, volcanic, ENSO, and trend terms. Their tests also confirmed that the results were essentially the same for both the ERA-Interim period since 1979 and the entire period from 1958 to 2016. We have included this discussion in the revised introduction to address the possible ENSO bias in the QBO influence. In this study, we have directly excluded ENSO events from each QBO composite phase using a threshold value to avoid the ENSO impact on the QBO influence.

Specific Comments:

1. **Role of the monsoon climatology section** The lengthy repetition of global monsoon climatology (largely reproducing Yoden et al., with only two additional years) adds little scientific value. If needed as a baseline, it should be explicitly framed as such and substantially condensed, or moved to Supplementary Information. References to review papers (Geen; Bordoni; Biasutti; Wang) would be more appropriate than re-presenting standard figures.
   Reply: To facilitate easy understanding of the geographical distribution of the seasonal mean basic state quantities, we use this figure as a reference when discussing the longitude–latitude sections of QBO composite differences in precipitation and horizontal winds (Fig. 5). Therefore, it is more relevant to keep this climatology section in the main text. However, as per your suggestion, we have shortened the discussion on global monsoon climatology, retaining only the essential content. Also, we have incorporated the suggested references.

2. **Figures and color scales** The chosen precipitation colorbar is counterintuitive: green corresponds to negative anomalies and warm colors to positive anomalies. This convention clashes with intuitive and widely used palettes. A more standard sequential blue scale would aid interpretation.
   Reply: We completely value your viewpoint. However, for the sake of consistency with our previous publications (Kumar et al., 2022; 2024; Yoden et al., 2023), we would like to retain the current color bar. This color bar was originally chosen to maintain consistency with certain standard references, including the Global Monsoon System series edited by C.-P. Chang. Please allow us to use the current color bar.

3. **Terminology and wording** There are several odd or overstated phrases, such as "chronic convection" or L122-"definitive description of global monsoon rainfall patterns." I do not agree with the fact that GPCP is a definitive description of precipitation. It is one of many available products, all of which have advantages and disadvantages. The overall wording should be revised to standard scientific expressions or rewritten to simpler expressions.

Reply: Ok. We have revised the sentence as “This dataset provides an overview of global monsoon rainfall patterns over both land and ocean”. We have also refined the text where necessary to ensure smooth and clear scientific expression. In the case of “chronic convecction”, the Webster-Miriam dictionary defines chronic as continuing or occurring again and again for a long time. This is what we mean by a region where there is usually deep convection during a given season. But perhaps there is some issue with the fact that people sometimes use chronic to refer to an illness. No, we are not trying to say that the atmosphere is ill, of course! But to allay such a concern we will refer to climatological areas of deep convection, as in “These warm anomalies coincide with the climatological locations of deep convection, characterized by cloud top temperatures less than 192 K and low OLR emission (Collimore et al., 1998)” on new lines 380-381.

Clarity of writing Some sentences are difficult to understand (e.g., line 458: “But, again refining that the influence of the QBO is manifested at a regional scale where localized atmospheric circulations exist”). The intended meaning should be clarified and redundant phrasing removed. I advise revising the manuscript with this in mind.
Reply: The language of the manuscript has been carefully revised to enhance its clarity and readability.

4. **Hadley cells** The manuscript frequently suggests a relationship between their results and potential modulation through ’Hadley cells’ or local meridional circulations. Perhaps the authors could provide more evidence of this by calculating more diagnostics about the Hadley cell, as in (Schwendike et al., 2014).
Reply: Yes, we agree that referring to the modulation of the Hadley cell is not appropriate based on the current analysis. We have removed reference to local Hadley cells. Instead, we aim to highlight the changes in upward motion and their association with the strength of the Bonin High. In the revised manuscript, we have updated the text to read as follows:
L549- L555 “Another aspect of this system which may be important in this context is that deep tropical convection is suppressed over Indonesia during QBO W, which implies reduced subtropical downwelling. The lower tropospheric anticyclone which dominates the circulation east of Japan during summer (the Bonin High, Enomoto et al., 2003) is one region of subsidence. With reduced convection in the adjacent tropics, one might expect a reduction in subsidence and strength of the Bonin High. In our analysis, the reduced subtropical downwelling is evident as a significant upward W-wind anomaly accompanied by notable meridional wind contours, suggesting a weakening of the region of subsidence over east of Japan (Fig. 10c, d).”
Thanks for the suggesting the nice reference (Schwendike et al., 2014), which has been incorporated into the manuscript to describe the climatological vertical motion field during DJF when discussing the mechanism for QBO modulation in of the North Atlantic Ocean (L567-570).

5. **Language consistency** The English throughout is uneven. For instance, “the deep convection” and “deep convection” are used interchangeably, and “QBO modulation” is sometimes written without the article. A careful language edit would improve readability, but comments here are made in a constructive spirit, recognizing that the authors may not be native speakers so this is merely a suggestion.
Reply: As replied in the above comment, the language of the manuscript has been carefully revised to enhance its clarity and readability.

References

Biasutti, M., Voigt, A., Boos, W. R., Braconnot, P., Hargreaves, J. C., Harrison, S. P., Kang, S. M., Mapes, B. E., Scheff, J., Schumacher, C., et al. (2018). Global energetics and local physics as drivers of past, present and future monsoons. *Nature Geoscience*, 11(6):392–400.

Bordoni, S. and Schneider, T. (2008). Monsoons as eddy-mediated regime transitions of the tropical overturning circulation. *Nature Geoscience*, 1(8):515–519.

Domeisen, D. I., Garfinkel, C. I., and Butler, A. H. (2019). The teleconnection of el niño southern oscillation to the stratosphere. *Reviews of Geophysics*, 57(1):5–47.

Garc´ıa-Franco, J. L., Gray, L. J., Osprey, S., Chadwick, R., and Martin, Z. (2022). The tropical route of quasi-biennial oscillation (qbo) teleconnections in a climate model. *Weather and Climate Dynamics*, 3(3):825–844.

Garc´ıa-Franco, J. L., Gray, L. J., Osprey, S., Jaison, A. M., Chadwick, R., and Lin, J. (2023). Understanding the mechanisms for tropical surface impacts of the quasi-biennial oscillation (qbo). *Journal of Geophysical Research: Atmospheres*, 128(15):e2023JD038474.

Geen, R., Bordoni, S., Battisti, D. S., and Hui, K. (2020). Monsoons, itczs, and the concept of the global monsoon. *Reviews of Geophysics*, 58(4):e2020RG000700.

Gray, L. J., Anstey, J. A., Kawatani, Y., Lu, H., Osprey, S., and Schenzinger, V. (2018). Surface impacts of the Quasi Biennial Oscillation. *Atmospheric Chemistry and Physics*, 18(11):8227–8247.

Hu, Z.-Z., Huang, B., Kinter III, J. L., Wu, Z., and Kumar, A. (2012). Connection of the stratospheric qbo with global atmospheric general circulation and tropical sst. part ii: Interdecadal variations. *Climate dynamics*, 38(1):25–43.

Huang, B., Hu, Z.-Z., Kinter III, J. L., Wu, Z., and Kumar, A. (2012). Connection of stratospheric qbo with global atmospheric general circulation and tropical sst. part i: Methodology and composite life cycle. *Climate dynamics*, 38(1):1–23.

Lee, J.-H., Kang, M.-J., and Chun, H.-Y. (2019). Differences in the tropical convective activities at the opposite phases of the quasi-biennial oscillation. *Asia-Pacific Journal of Atmospheric Sciences*, 55(3):317–336.

Nie, J. and Sobel, A. H. (2015). Responses of tropical deep convection to the qbo: Cloud-resolving simulations. *Journal of the Atmospheric Sciences*, 72(9):3625–3638.

Rodrigo, M., Garc´ıa-Serrano, J., and Bladé, I. (2025). Quasi-biennial oscillation influence on tropical convection and el niño variability. *Geophysical Research Letters*, 52(10):e2024GL112854.

Schwendike, J., Govekar, P., Reeder, M. J., Wardle, R., Berry, G. J., and Jakob, C. (2014). Local partitioning of the overturning circulation in the tropics and the connection to the hadley and walker circulations. *Journal of Geophysical Research: Atmospheres*, 119(3):1322–1339.

Wang, B. and Ding, Q. (2008). Global monsoon: Dominant mode of annual variation in the tropics. *Dynamics of Atmospheres and Oceans*, 44(3-4):165–183

Reply: We have cited all above suggested references at appropriate place in the revised manuscript.