Review of manuscript #2025JD03420. First Round

September 19, 2025

This manuscript examines the influence of the Quasi-Biennial Oscillation (QBO) on global monsoon rainfall and related teleconnections using ERA5 and GPCP data. The authors emphasize seasonal differences, vertical structure (50 vs. 70 hPa winds), and the apparent linearity of phase progression. While the topic is timely and of interest, the manuscript in its current form requires substantial revision. The main weaknesses are an incomplete and selective literature review, a tendency to assert rather than demonstrate mechanisms, and an relatively simple, and my opinion therefore unconvincing, treatment of ENSO. I recommend major revisions.

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; García-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; García-Franco et al., 2023; Rodrigo et al., 2025).

2. **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?
- The claim of "linear progression" of QBO impacts across phases is descriptive only. Without quantitative regression or symmetry diagnostics, this interpretation remains speculative.

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.
   - 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.
   - 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.

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.

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.

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.

4. **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.

5. **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).

6. **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.

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