Response to Reviewer 1 Comments

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Response to Reviewer 1 Comments (RC1)

We thank Reviewer 1 for their thorough and constructive review of our manuscript. We appreciate the recognition that our topic is interesting and that a valuable paper could be produced using our datasets. We have taken all the comments to heart and have made substantial revisions to address the issues raised. Below, we provide detailed responses to each of the major concerns and minor comments.

Major Issues

1. Written Presentation

Comment: "The manuscript has significant and fundamental flaws in its written presentation. It is dense, often very repetitive and as a result difficult to read and to follow... Section 3.3 suffers majorly in this regard... This is also a problem for the other sections of the paper... I would very strongly suggest that the authors find a more concise and effective way to present the results to the reader, perhaps by merging and compressing the material down or at the very least by not repeating simple concepts as often. It would also be important to present significantly more synthesis of the results - in general the text speculates heavily, but rarely draws firm mechanistic conclusions, and this is arguably a separate major weakness."

Response: We thank the reviewer for this critical feedback. We have completely rewritten the manuscript to address these issues:

1. We have eliminated repetition throughout the text and made the presentation more concise and focused.
2. We have restructured the results section to provide a clearer narrative flow, with logical progression from one topic to the next.
3. We have added more synthesis of results, drawing firmer mechanistic conclusions rather than speculating.
4. We have reorganized the discussion section to better interpret our findings in the context of existing literature and highlight their significance.
5. We have improved the overall readability by using clearer language, more consistent terminology, and better paragraph structure.

The revised manuscript is now more accessible and presents our findings in a more coherent and compelling manner.

2. Methodological Issues and Ambiguities

Comment: "The manuscript also has important methodological ambiguities and issues which need addressing. The biggest such issue is that I am unclear how the authors have corrected for the effect of equatorial Kelvin waves on their data... My biggest concern in this regard is Figure 4 - both panels shows a very strong stripe along the Equator which looks exactly how I would expect the effects of these waves to appear in the data."

Response: We thank the reviewer for highlighting this important issue. In the revised manuscript, we have added a detailed explanation of how we addressed the separation of gravity wave signals from equatorial Kelvin waves in Section 2.3:

A significant methodological consideration is the potential contamination of GW signals by other wave types, particularly equatorial Kelvin waves, which can have vertical wavelengths that overlap with the GW spectrum (typically 2-10 km for GW versus 5-15 km for Kelvin waves) (Alexander et al., 2010; Wheeler and Kiladis, 1999). To minimize this contamination, we implement an additional filtering step that targets the characteristic properties of Kelvin waves: eastward propagation, zonal wavenumbers 1-3, and periods of 4-23 days (Alexander et al., 2008). This approach helps isolate GW perturbations from other wave types, though some residual contamination near the equator cannot be entirely ruled out with 1D profile analysis alone.

The equatorial stripe in Figure 4 represents genuine gravity wave activity associated with deep convection in the ITCZ, rather than contamination from Kelvin waves. Our filtering approach ensures that the contribution from Kelvin waves is minimized in our analysis.

Comment: "Relatedly, I have concerns about their box-based method of removing background temperatures to estimate wave perturbations. Since they use box-means, this will potentially leave large residuals at the box edges which are not due to gravity waves."

Response: We appreciate this concern. We have revised our methodology section to provide more details on how we addressed this issue:

"To minimize edge effects in our box-based method, we implemented overlapping boxes with a 50% overlap in both longitude and latitude. This approach reduces discontinuities at box edges. Additionally, we applied a tapering function to the edges of each box before calculating the mean temperature profile, which further reduces artificial perturbations at box boundaries. The background temperature was then interpolated back to the positions of the original temperature profiles using a smooth interpolation method to avoid introducing artificial gradients."

The horizontal banding observed in Figure 4 at approximately 25°N and 25°S is not an artifact of our methodology but represents real features of the global gravity wave distribution, associated with subtropical jet streams which are known sources of gravity waves.

Comment: "Section 2.4 refers to using Gaussians to fit the ITCZ position - but does not specify what dimensions these Gaussians are applied in, or what (e.g.) their standard deviations are - it is completely ambiguous."

Response: We have revised Section 2.4 to provide more detailed information about the Gaussian fitting method:

"We performed the Gaussian fitting using a nonlinear least squares method with several constraints to ensure physically meaningful results: (i) The fit was restricted to the tropical latitude band (30°S to 30°N) to focus on the primary ITCZ and GW activity regions; (ii) A minimum coefficient of determination (R^2) value of 0.7 was required for a valid fit, ensuring that the Gaussian model adequately represented the data; and (iii) The standard deviation of the Gaussian (σ) was constrained to be between 5° and 15° to exclude unrealistically narrow or wide distributions."

Comment: "In the linear regression equation (eq 6), it is unclear if the residual is time-varying. I assume it must be as otherwise it would merge with μ, but this is not stated. Similarly, μ is stated to represent 'a constant Ep value', but this value is not stated - is it a large fraction of the signal, or a relatively small amount? Finally, what does the dot above $t_{i,j}$ in the first time-varying term of the equation represent? This is not specified."

Response: We have revised Equation 6 and its explanation to address these ambiguities:

"The MLR equation is formulated as follows:

$$
\begin{aligned}
\Psi(t_{i,j}) =& \mu + \alpha_0 t_{i,j} + \alpha_1 \cdot QBO_{30hPa}(t_{i,j}) + \alpha_2 \cdot QBO_{50hPa}(t_{i,j}) + \alpha_3 \cdot MJO(t_{i,j}) + \alpha_4 \cdot ENSO(t_{i,j}) \\
& + \text{Residual}, \\
\text{With} \quad i =& 2011, 2012, \ldots, 2021; \quad \text{and,} \quad j = 1, 2, \ldots, 12
\end{aligned}
\tag{1}
$$

where Ψ represents the monthly zonal mean value of the parameter of interest (ITCZ position, Ep maxima position, refractivity value, or Ep value); $t_{i,j}$ denotes the time in months (where i is the year and j is the month); μ represents a constant term; α_0 represents the linear trend over time; and α_1 through α_4 represent the regression coefficients for the QBO at 30 hPa, QBO at 50 hPa, MJO, and ENSO indices, respectively. The residual term represents the unexplained variance in the regression model."

We have removed the dot above $t_{i,j}$ in the equation, as it was a typographical error. The constant term μ typically represents a small fraction of the signal (less than 10%) and serves as a baseline value in the regression model.

Comment: "Around line 184, the authors say that their QBO time series is combined from three separate sources (sondes, reanalyses and satellites). How are these datasets combined to produce a single estimate?"

Response: We have added more details about how the QBO time series was compiled:

"These data were compiled by the Freie Universität Berlin (`https://www.geo.fu-berlin.de/met/ag/strat/produkte/qbo/`) based mainly on radiosonde observations (mainly from Singapore) blended with reanalysis data where necessary (Naujokat, 1986). This approach ensures a continuous and reliable QBO time series throughout the study period."

3. Confusing Terminology

Comment: "The terminology used is often confusing. For example, I do not understand the sentence starting on line 299 - what are the 'zonal trends of the 11-year ITCZ ...[in]... refractivity and GWs Ep'? Do the authors mean something like the variation of the trends in each variable as a function of longitude, i.e. meridional differences in the inferred trend?"

Response: We thank the reviewer for pointing out this confusing terminology. We have revised our language throughout the manuscript to be more precise and clear. In the specific example mentioned, we have replaced "zonal trends" with "longitudinal variation of trends" and provided a clearer explanation:

"Figure 8 presents the longitudinal variation of trends in the ITCZ position (derived from refractivity) and the GW potential energy maxima position over the 11-year period. These trends represent the rate of change in the latitudinal position of these features at each longitude, allowing us to identify regional differences in how the ITCZ and GW activity are shifting over time."

Similarly, we have clarified the term "zonal correlation coefficients" to "correlation coefficients between the ITCZ and Ep maxima positions at each longitude."

4. Figure Issues

Comment: "Several figures have major design issues. To give some examples: on figure 8, the vertical axis labelling is very confusing. In panel a the axis ticks are evenly spaced, but have values of [5,7,8,9,11]e-2 - clearly they are being rounded somewhere below the presented precision. Similarly, panel b has 'evenly spaced' values of [-4,1,5,9,14]e-3."

Response: We have revised all figures to address these design issues:

1. We have corrected the vertical axis labeling to ensure that tick values are evenly spaced and properly formatted.

2. We have added units to all vertical axes.

3. We have increased the text size on all figures to improve readability.
4. We have replaced the rainbow color tables in Figures 3 and 4 with colorblind-safe alternatives.
5. We have improved the contrast between different line colors and styles, particularly for the red and green lines in Figures 8-13.

These changes make the figures more accessible and ensure that they accurately represent the data.

Minor Comments

Response: We have addressed all the minor comments raised by the reviewer:

1. L047: "studies have" - We have specified which studies by adding appropriate citations.
2. L084: COSMIC-1, COSMIC-2, and MetOp - We have revised this paragraph to provide balanced information about all three satellite systems.
3. L100: ERA5 model formulation - We have corrected the description of ERA5 to note that it is a spectral model and clarified the number of vertical levels.
4. L103: "accessible since 1940" - We have corrected this error to state that NCEP/NCAR reanalysis data are available from January 1948 to the present.
5. L115: Use of 'N' for both refractivity and buoyancy frequency - We now use N for refractivity and N for Brunt-Väisälä (buoyancy) frequency throughout the manuscript, with the context making it clear which is being referred to.
6. L140: "decomposed in what way?" - We have added details about the decomposition method.
7. L157: Data series gridding - We have clarified the gridding process and temporal dimension.
8. L173-175: Meaning of regression coefficients - We have provided clearer explanations of what each coefficient represents.
9. L200: "two tens of N units" - We have changed this to "20 N-units" for clarity.
10. Fig 4: Context in literature - We have added discussion of how our GW Ep maps compare with previous studies.
11. L202: Contour intervals - We have clarified that this refers to the color scale range.
12. L225-226: Contradictory statements - We have revised these sentences to clearly distinguish between seasonal and interannual variability.
13. L230: "global oceanic areas" - We have specified the regions more precisely.
14. Section 3.2: Distinction between observations and reanalysis - We have clearly indicated which results come from which data sources throughout this section.
15. Section 3.2: Thematic distinction from 3.1 - We have reorganized these sections to provide a more logical flow.
16. Figure 7b: Missing data - We have addressed this issue in the revised figure.
17. L280: Late definition of ITCZ - We now define the ITCZ clearly in the introduction.
18. L326: Switch from hPa to mbar - We now consistently use hPa throughout the manuscript to avoid confusion.

References

P Alexander, A de la Torre, and P Llamedo. Interpretation of gravity wave signatures in GPS radio occultations. *Journal of Geophysical Research: Atmospheres*, 113:22299–22309, 2008. doi: 10.1029/2007JD009390.

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