Author Response to Reviewer Comments

Long-term Study of Gravity Wave Potential Energy and OH Airglow Emissions from 22 years of TIMED/SABER Observations

We thank the reviewer for the thorough and constructive evaluation. The comments have led to substantial improvements in the physical interpretation, statistical rigour, and reference accuracy of the manuscript. Below we address each comment and describe the corresponding modifications.

Major Comments

Reviewer Comment: The correlation between OH and Ep might not have a clear physical meaning. Temperature fluctuations affect both the H+O3 reaction rate and the Ep calculation, so both quantities respond to temperature changes. The correlation could be spurious, driven by a common temperature dependence.

Response: We agree that a contribution from co-varying background temperature cannot be entirely excluded, and we now acknowledge this explicitly. However, we provide two lines of evidence that the correlation is not purely spurious:

(1) The correlation exhibits systematic latitude and season dependence, peaking during winter at mid-latitudes ($r \approx 0.7$) and weakening to $\sim$0.3 at the equator. A temperature-driven spurious correlation would not produce this specific pattern, which instead matches the expected behaviour of gravity wave–OH coupling: during winter, mid-latitude gravity waves have longer vertical wavelengths that maintain phase coherence across the $\sim$8 km thick OH emission layer.

(2) The thermal/non-thermal decomposition analysis (Figure 11) demonstrates that temperature-driven effects account for only part of the OH variability, with substantial non-thermal residuals—inconsistent with temperature being the sole common driver.

We have also expanded the discussion of the two physical coupling pathways (thermal: wave-induced T' modulates the H+O_3 rate constant; dynamical: wave-induced vertical advection of O_3/H/O across steep gradients) with references to the foundational models of Walterscheid et al. (1987) and Schubert et al. (1991).

Change in manuscript: Discussion, Section 4, first paragraph: substantially rewritten to acknowledge potential for spurious correlation, present counter-evidence, and clarify the two physical coupling pathways. Introduction, Section 1: clarified that gravity waves modulate emission variance, not the mean intensity directly (citing Vargas et al., 2007). Confidence intervals added to all reported correlation coefficients.

Reviewer Comment: Since Ep increases exponentially with altitude, an upward shift of the OH layer produces an apparent Ep increase. The authors should consider using Ep per unit volume. Results may also differ from Liu et al. (2017).

Response: We fully agree that this is a critical consideration and have added a quantitative estimate of the altitude-related apparent Ep trend. Using a mesospheric density scale height of $H \approx 6$ km, the observed OH peak altitude rise of ∼0.04 km/yr over 22 years yields a total shift of ∼0.88 km, corresponding to $\exp(0.88/6) - 1 \approx 16\%$ apparent Ep increase from the density effect alone. The observed mid-latitude Ep trend (∼0.05 J/kg/yr relative to ∼40 J/kg mean) represents ∼6% over 22 years. Since the altitude-related apparent increase (∼16%) exceeds the observed trend, we now caution that the positive Ep trend could be largely or entirely attributable to the upward migration of the OH emission layer. We recommend Ep per unit volume ($\rho \cdot E_p$) analysis as important future work.

Regarding Liu et al. (2017): our results differ because (i) we evaluate Ep at a single altitude (the OH peak, ∼87 km) rather than over the 50–100 km range; (ii) our latitude coverage extends only to ±50°; and (iii) our analysis is subject to the altitude-tracking effect not present in the fixed-grid analysis of Liu et al. We now discuss these differences explicitly.

Change in manuscript: Section 2.3 (Methodology): added caveat about Ep per unit mass and density dependence. Section 3.3 (Results): added altitude caveat for Ep trends. Section 4 (Discussion): new paragraph with quantitative altitude correction estimate and explicit comparison with Liu et al. (2017). Section 5 (Conclusions): revised to note that Ep trends may reflect OH layer altitude migration.

Reviewer Comment: The text describes OH trends as "negative at mid-latitudes" but Figure 4 shows these are not statistically significant at 95% CI.

Response: We agree and have revised all trend statements to strictly distinguish statistically significant results from non-significant ones. Previous language implying physical significance where none was demonstrated has been replaced. The word "trend" now always appears with the appropriate qualification.

Change in manuscript: Section 3.3: "the annual trends show negative values" replaced with "the annual trends are not statistically significant at the 95% confidence level at most mid-latitude bins, where the 95% confidence intervals overlap zero despite negative central values." Similar revisions throughout Results and Discussion. Section 5 (Conclusions): bullet on OH trends now reads "though not all statistically significant at the 95% level." 95% confidence intervals added for all correlations via Fisher's z-transformation.

Reviewer Comment: The deep branch of the BDC extends only to the stratopause (∼50 km). How does BDC affect mesospheric OH? The decrease in atmospheric density and the downward shift of ozone (Kogure et al., 2026) are not discussed.

Response: We agree that the link between BDC and mesospheric OH requires clarification. The mechanism is indirect: changes in BDC strength modify stratospheric jet structure and planetary wave activity, which alter the filtering of gravity waves propagating into the mesosphere. The modified gravity wave momentum deposition drives changes in the mesospheric residual circulation, affecting the downward transport of atomic oxygen from the lower thermosphere into the mesopause region. This pathway is now described explicitly.

We also now discuss the density decrease accompanying mesospheric cooling, noting that competing effects (radiative contraction vs. photochemical redistribution) determine the net OH layer displacement direction, with the observed upward trend suggesting the photochemical effect dominates. Kogure et al. (2026) has been added to the bibliography.

Change in manuscript: Section 4 (Discussion): BDC paragraph rewritten to describe the indirect pathway via gravity wave filtering modification. New text on density effects and OH layer displacement direction, citing Kogure et al. (2026). Reference added to references.bib.

Reviewer Comment: The claim about "dominance of shorter vertical wavelength convectively generated waves" in the tropics is not supported by the cited references (Vargas et al., 2007 discusses cancellation factors; Fritts and Alexander, 2003 is a general review).

Response: We agree that the original claim was not adequately supported. Vargas et al. (2007) describes the cancellation factor for airglow layers but does not establish "dominance" of short-λ_z convective waves in the tropics. Fritts and Alexander (2003) is a comprehensive review that does not make this specific claim. We have removed the unsupported statement and replaced it with a more accurate description: the weaker equatorial correlations may reflect the broader gravity wave spectrum present in the tropics, where diverse source mechanisms generate waves spanning a wide range of vertical wavelengths (Sato et al., 2009; Alexander et al., 2010). We now cite Vargas et al. (2007) correctly for the phase cancellation mechanism.

Change in manuscript: Section 4 (Discussion): removed "dominance of shorter vertical wavelength convectively generated waves in the tropics." Replaced with: "The weaker equatorial correlations ($\sim$0.3) may reflect the broader gravity wave spectrum present in the tropics, where diverse source mechanisms (convection, shear instability) generate waves spanning a wide range of vertical wavelengths (Sato et al., 2009; Alexander et al., 2010). Waves with vertical wavelengths shorter than the OH layer thickness undergo phase cancellation that attenuates the integrated emission response (Vargas et al., 2007)."

Minor Comments

Reviewer Comment: Lines 50–53: The relationship between gravity wave activity and tropospheric weather has been well established. Also, horizontal propagation, critical-level filtering, and QBO/SAO filtering should be acknowledged.

Response: We have expanded the Introduction to include references beyond our previous work, citing Fritts and Alexander (2003), Sato et al. (2009), and Plougonven and Zhang (2014). We now discuss critical-level filtering, selective absorption by QBO/SAO/jet winds, and horizontal propagation.

Change in manuscript: Section 1, paragraph beginning "Gravity wave climatologies...": substantially expanded with broadened references and discussion of filtering mechanisms.

Reviewer Comment: Line 55: Gravity waves modulate OH emission fluctuations (variance), not the mean.

Response: Corrected. The text now states that wave-induced perturbations affect the variance of OH emission brightness, and that modulation of the long-term mean requires sustained changes in the background chemical environment.

Change in manuscript: Section 1, coupling paragraph: "These wave-induced perturbations affect the variance of OH emission brightness; direct modulation of the long-term mean emission rate requires sustained changes in the background chemical environment rather than transient wave passages (Vargas et al., 2007)."

Reviewer Comment: Lines 57–61: Physical mechanisms for how QBO, ENSO, and solar activity affect mesospheric OH should be explained.

Response: We now include brief mechanistic descriptions for each driver: Solar $\rightarrow$ Lyman-α H_2O photolysis $\rightarrow$ H production; QBO $\rightarrow$ stratospheric wind filtering of GWs; ENSO $\rightarrow$ convective anomalies $\rightarrow$ modified wave generation and BDC. Liu et al. (2017) is now cited explicitly for the long-term SABER analysis.

Change in manuscript: Section 1: new text with mechanistic descriptions and citations (Mlynczak et al., 2013; Marsh et al., 2006; Dunkerton, 1997; Baldwin and Dunkerton, 2001; Liu et al., 2017; Sassi et al., 2004; Calvo et al., 2010).

Reviewer Comment: Line 95: SABER retrieval errors increase with altitude. Are there systematic trends in retrieval quality over the mission?

Response: We now acknowledge that SABER retrieval errors increase with altitude (particularly above 90 km) and note that our analysis focuses on the OH peak altitude ($\sim$85–87 km) where retrievals are most reliable. We state that no systematic temporal trends in retrieval precision have been identified (Remsberg et al., 2008), but caution that residual long-term drifts cannot be entirely excluded.

Change in manuscript: Section 2.1: new sentences added after the validation statement.

Reviewer Comment: Line 133: More methodological details on the GW extraction method. What horizontal wavenumber filtering? Also, check Eq. (4) in Ayorinde et al. (2024) for a possible factor-of-4 error.

Response: We have added details on the spatial Fourier decomposition (wavenumbers 0–6), the vertical wavelength filtering range (3–20 km), and cited the foundational methodological papers (Ern et al., 2004; Preusse et al., 2002). Regarding Eq. (4) of Ayorinde et al. (2024): the present manuscript uses Eq. (1), which correctly includes the factor of 1/2. The actual analysis code implements the correct formula.

Change in manuscript: Section 2.3: rewritten to include methodological details and foundational citations.

Reviewer Comment: Line 160: How much data is excluded by the screening criteria? Justify the 1000 J/kg threshold.

Response: We now report that the N^2 criterion removes approximately 3% of profiles (predominantly at high altitudes), the Ep>1000 J/kg threshold removes fewer than 1% of remaining profiles, and justify this threshold as corresponding to ∼15 K temperature perturbation amplitude, which represents the upper range of observed GW amplitudes in the MLT region.

Change in manuscript: Section 2.3: data exclusion percentages and threshold justification added.

Reviewer Comment: Line 205: The polynomial terms in the MLR model are not strictly orthogonal.

Response: Corrected. We now state that the subtraction of the mean squared value *reduces* (rather than ensures) the correlation, yielding a residual correlation coefficient of less than 0.05, which confirms near-independence but does not guarantee exact orthogonality.

Change in manuscript: Section 2.4: "ensures that the quadratic term is orthogonal" replaced with "reduces the correlation...; polynomial terms are not inherently orthogonal."

Reviewer Comment: Line 225: QBO30 and QBO50 may be multicollinear. Report their correlation.

Response: We now report that the Pearson correlation between QBO30 and QBO50 is $r = -0.39$, indicating substantially independent information, and that both are retained because they represent different phases of the QBO vertical structure.

Change in manuscript: Section 2.4: new sentence reporting $r = -0.39$ and justification for retaining both indices.

Reviewer Comment: Line 232: "Lean (2018)" is cited for the quadratic solar term. This reference discusses solar irradiance since 850 CE, not the use of quadratic terms in MLR.

Response: The reviewer is correct. We have replaced this citation with Zhao et al. (2020), who explicitly adopt both linear and quadratic solar terms for mesospheric trend studies using the same MLR framework.

Change in manuscript: Section 2.4: "\citep{Lean2018}" replaced with "following the approach adopted by \citet{Zhao2020} for mesospheric trend studies."

Reviewer Comment: Line 267: Remove "austral."

Response: Removed.

Change in manuscript: Section 3.1: "austral JJA months" → "JJA months."

Reviewer Comment: Line 268: Southern Hemisphere winter Ep values should be compared with Liu et al. (2017).

Response: We now explain why our results differ from Liu et al. (2017): (i) Ep evaluated at the OH peak altitude (∼87 km) rather than over the 50–100 km range; (ii) latitude coverage limited to ±50° rather than extending poleward; (iii) local mesospheric conditions at the

OH peak differ from the stratospheric environment.

Change in manuscript: Section 3.1: new paragraph with explicit comparison to Liu et al. (2017) and Geller et al. (2013).

Reviewer Comment: Line 285: Note that part of Ep variation may result from OH altitude changes.

Response: Acknowledged. We now note this caveat in Section 3.3 and provide a quantitative estimate in Section 4.

Change in manuscript: Section 3.3: caveat added. Section 4: quantitative altitude correction ($\sim$16% apparent increase from 0.88 km shift).

Reviewer Comment: Line 479: Addressed in Major Comment 4.

Response: See Major Comment 4 above.

Reviewer Comment: Line 500: The claim that solar activity directly affects the polar vortex needs more nuance and proper citation.

Response: We have replaced the direct claim with a more nuanced statement based on Gray et al. (2010), noting that solar UV heating modifies the stratospheric temperature gradient and zonal wind (the "top-down" mechanism), but that direct solar modulation of polar vortex strength remains uncertain. Gravity wave filtering is now attributed to background zonal winds rather than specifically to planetary waves.

Change in manuscript: Section 4, solar response paragraph: rewritten with Gray et al. (2010) citation and caveats.

Reviewer Comment: Line 510: Verify that Randel et al. (2009) and Sassi et al. (2004) support the claim about reduced atomic hydrogen transport.

Response: The reviewer is correct that these references discuss ENSO effects on stratospheric circulation and temperature but do not explicitly state "reduced atomic hydrogen transport." We have reworded to state that ENSO-related changes modify the transport of ozone and its precursors, and that accompanying circulation changes modify the supply of atomic oxygen to the mesopause region. The unsupported reference to Fernandez et al. (2004) has been removed.

Change in manuscript: Section 4, ENSO paragraph: "reducing atomic hydrogen transport" replaced with "modifying the supply of atomic oxygen to the mesopause region." Fernandez et al. (2004) removed; Calvo et al. (2010) added.

Reviewer Comment: Line 517: CO_2 cooling causes atmospheric contraction, potentially shifting the OH layer *downward*, not upward.

Response: We now acknowledge that both upward and downward shifts are possible depending on the competition between atmospheric contraction (downward) and photochemical redistribution of O_3 (upward). The observed upward trend in OH peak altitude suggests the photochemical effect dominates. We cite Kogure et al. (2026) for the influence of CO_2-

induced changes on mesospheric ozone distribution.

Change in manuscript: Section 4, trend paragraph: new text on competing displacement effects with Kogure et al. (2026) citation.

Reviewer Comment: Line 525: The strong Ep trend may be apparent, driven by altitude changes.

Response: We fully agree and now provide the quantitative estimate described in Major Comment 2, concluding that the Ep trends should not be interpreted as unambiguous evidence for increased gravity wave activity.

Change in manuscript: Section 4: new paragraph with quantitative estimate. Section 5 (Conclusions): revised bullet point.

Reviewer Comment: Line 546: Clarify "non-thermal character" of the QBO response. Also discuss QBO–SAO separation.

Response: We now define "predominantly non-thermal character" as referring to the residual OH variability remaining after the temperature-driven component is removed. We also add a note that the SAO modulates gravity wave activity at low latitudes and that separating SAO from QBO effects requires further work.

Change in manuscript: Section 4: QBO paragraph revised with clarification and SAO note.

Reviewer Comment: Line 565: Conclusions should be consistent with statistical significance.

Response: Conclusions now include appropriate caveats about statistical significance wherever trends are reported.

Change in manuscript: Section 5: all trend-related conclusions revised for consistency with 95% CI criterion.

Reviewer Comment: Provide 95% confidence intervals for correlation coefficients.

Response: Added throughout, estimated via Fisher's z-transformation.

Change in manuscript: Sections 3.5 and 4: 95% CIs added (e.g., "0.6–0.7 (95% CI: 0.5–0.8)"). Section 5: CI added to conclusion bullet on correlations.

Reference Corrections

- **Plougonven and Zhang (2014)**: Title corrected from "Internal gravity waves: from instabilities to turbulence" (Staquet & Sommeria, 2002) to "Internal gravity waves from atmospheric jets and fronts." Journal corrected from Annual Review of Fluid Mechanics to Reviews of Geophysics. DOI corrected to 10.1002/2012RG000419.

- **Lean (2018)**: Citation removed from the quadratic-term justification and replaced with Zhao et al. (2020), who explicitly use this approach for mesospheric trend studies.
- **Ern et al. (2018)**: The BibTeX key reads "Ern2018" but the year field correctly reads 2017. This paper was published in Geophysical Research Letters, Vol. 44(1), pp. 475–485, doi:10.1002/2016GL072007. No change required to the content.
- **Vargas et al. (2007)**: Now cited correctly for the cancellation factor concept only, not for the "dominance" of short-wavelength convective waves.
- **Fernandez et al. (2004)**: Removed from the ENSO–BDC discussion, as this reference does not support the specific claim about atomic hydrogen transport.
- **Kogure et al. (2026)**: Added (ACP, 26, 665–680, doi:10.5194/acp-26-665-2026).
- **Gray et al. (2010)**: Added (Rev. Geophys., 48, RG4001, doi:10.1029/2009RG000282).