Response to Reviewer 3

We wish to thank the reviewer for the careful and thorough assessment of our manuscript. The comments were very relevant, including those concerning the additional material presented in the Supplement. We believe that the manuscript has been substantially improved in response to the reviewer's comments. Our point-by-point responses are provided below in blue.

Major comments 1 and 5: Physical meaning of TraLI and scope of the conservative criterion

Section 4, lines 412-440; Equations 14-15; lines 385-408; Section 5.2, lines 509-510
Equation 15 requires lower and upper levels with $S_0 < S_c$, does not require an intervening layer with $S_0 > S_c$. A profile below the threshold throughout the column therefore satisfies the criterion, although it contains no propagating cavity at the target wavelength within the adopted approximation. This differs from the waveguide configuration described in Section 3.4. Could the authors add the intermediate propagation condition, or explicitly describe TraLI as the spatial co-occurrence of two potential evanescent regions?

Response: Following the reviewer's comment, we revised the definition of TraLI, adding the mentioned condition to Eq. 15 and commenting on it in the text at L495, L502 and L600. While important for the definition of TraLI, this additional condition does not alter the results discussed in the manuscript, in particular those shown in Fig. 8b-d, which depict the set of profiles $\mathcal{A}$ introduced in Eq. 15. This has been carefully verified.
In Section 5 (Section 4 in the former version of the manuscript), we decided to retain the discussion on the upper- and lower-TraLI separately, as these exhibit markedly different temporal variability and provide relevant information on the occurrence and spatial extent of upper- and lower-tropospheric reflecting levels in our study region.
In the Supplement we nevertheless show the evolution of TraLI for August and November 2024 (chosen as examples for their different upper- and lower-TraLI conditions), with the criterion on the intermediate propagative layer included (new Fig. S11). While the resulting TraLI values are, as expected, generally lower than the corresponding upper- and lower-TraLI (because the two reflecting levels must co-occur spatially and be separated by an intervening propagative layer),[1] the days characterized by higher values of both upper- and lower-TraLI (Fig. S11a,e) also correspond to higher TraLI values (Fig. S11b,f). Therefore, introducing the intermediate propagative layer condition does not significantly affect the results or the interpretation presented in the manuscript.

Even with this condition, a candidate waveguide does not establish that trapped modes are excited. Likewise, cloud bands establish wave activity without independently identifying vertical confinement. The statement that high TraLI is sufficient for trapping should therefore be qualified to reflect what the index and observations actually establish.

Response: We agree that the identification of a potential waveguide does not establish that trapped modes are excited. TraLI was indeed explicitly introduced as a measure of the likelihood of trapping-favourable environmental conditions over a spatial domain and not as a diagnostic of the excitation of trapped modes. Regarding the interpretation of banded cloud patterns, we agree that cloud bands do not by themselves demonstrate vertical confinement. However, persistent and coherent wave-like patterns extending over several cycles are a commonly used observational signature of wave trapping (Lindzen and Tung, 1976; Nappo, 2012). These manifest as modulations of brightness temperature in vapor-sensitive satellite imagery or as banded cloud patterns in visible

[1] In this respect, we note that TraLI is largely controlled by the spatial extent of the least extended of the two reflecting regions: for example, if the upper-TraLI is 100% and the lower-TraLI is 50%, then TraLI cannot exceed 50%.

imagery (Vicari et al., 2024). For the cases highlighted in Figure 8, the observed wave activity extends over large spatial scales, consistent with the presence of trapping-favourable environmental conditions identified by TraLI. In conclusion, as already stated in the previous version of the manuscript, we use the observations as evidence of wave activity and interpret them in conjuction with the trapping-favourable environmental conditions diagnosed by TraLI. We however acknowledge that the wording might have been ambiguous in some places and have clarified the text where necessary (L513-515 in the revised version of the manuscript).

The proposed direction favours upper-level evanescence, but this does not generally optimize the complete waveguide: the lower boundary and intervening propagating layer must also be compatible, especially when wind direction changes with height. Setting $c = 0$ does not provide a universal limit either. For antiparallel U_h and c, retaining c increases $|U_h - c|$ and reduces the first Scorer term, potentially allowing evanescence where the zero-speed approximation does not. Consequently, failure of the adopted criterion does not generally rule out trapping at another direction or phase speed. Please state the restrictions under which the exclusion in lines 399-400 holds, or describe TraLI as a diagnostic for the chosen wave parameters without this exclusion claim.

Response: In response to the reviewer's comment, the sensitivity of TraLI to the assumptions underlying its definition has been examined and clarified in the revised manuscript. In particular, we tested both the choice of propagation direction and the zero-phase-speed assumption. We would like to remark that the choice to project the background wind along its upper-tropospheric direction is motivated by a strong observational constraint from our case study and is also consistent with previous studies of wave trapping in sheared flows (e.g. Stephan, 2020; Vicari et al., 2024). We now explicitly acknowledge in the manuscript that maximizing the potential for upper-tropospheric reflection does not automatically optimize the full waveguide (L469-476). In this respect, we have been careful throughout the manuscript to distinguish between conditions favourable for upper-level reflection and conditions favourable for trapping, which additionally requires a compatible lower boundary and a propagative layer in-between.
Regarding the sensitivity tests, we have first compared TraLI based on the standard upper-tropospheric projection with an alternative projection chosen to maximize lower-level reflection. As to be expected in cases of strong directional shear, some differences between the two cases are observed, but good agreement is retained for a large fraction of the days under consideration, and the trapping likelihood is generally higher for the upper-tropospheric projection. We also tested the effect of including a non-zero phase speed antiparallel to the upper-tropospheric wind, considering a representative value of $c = -5$ m s^{-1}, whose magnitude exceeds the value estimated from our case study. As expected, TraLI increases for most days, indicating that the $c = 0$ assumption can lead to an underestimation of the trapping likelihood in some cases. However, the relative strength of TraLI across most events is preserved. Overall, these sensitivity tests suggest that the standard definition of TraLI constitutes a simple yet reasonable compromise for the purpose of our climatological analysis. The results and interpretation of the sensitivity tests are discussed in detail in the new Supplementary Section 6, while the limitations of our approach with respect to directional shear and non-zero phase speeds are now explicitly mentioned in the revised manuscript (L473-475).

Major comment 2 and 4: Cavity eigenfunctions and observed polarization

Section 3.5, lines 330-362; Equations 11-13; Figures 6-7
The explanation of the different horizontal- and vertical-velocity peaks is appealing, but appears inconsistent with the stated cavity geometry. Under the symmetric boundary conditions, continuity with the decaying exterior solutions gives

$$\tilde{w}_n(z) \propto \cos\left[m_n(z - z_b) - \frac{\psi}{2}\right], \qquad m_n H = n\pi + \psi.$$

For mode 2, the vertical-velocity antinode and horizontal-velocity node lie at $z_b + H/2 = 6.1$ km for $z_b = 4.5$ km and $H = 3.2$ km. The aircraft flew at approximately 5.8 km. Using the rounded values of N and $\hat{\omega}$ in Tables 1-2 and $\tilde{u} = (i/k)\partial_z \tilde{w}$ gives $|\tilde{u}_2/\tilde{w}_2| \approx 1.9$ there, rather than a horizontal-velocity node. Could the authors reconcile Figure 7 with these boundary conditions and explain whether the predicted component amplitudes at flight level

support the proposed mechanism? Since H was fitted to the spectral peaks, this polarization comparison is particularly useful for establishing what the model explains beyond their positions.

Response: In the previous version of the idealized model, the amplitude ratios $|\tilde{w}_n/\tilde{u}_n|$ could not provide a reliable quantitative assessment of the model predictions. While the model was already able to qualitatively explain the different observed wavelengths, we agree with the reviewer that it is interesting to explore how quantitative its predictions can be. For this purpose, we have developed a methodology to derive the cavity depth H and lower boundary altitude z_b in a more rigorous way from the observed atmospheric profiles, rather than treating these parameters as tuning parameters. While the procedure used to determine the cavity geometry has been revised, the physical assumptions and formalism of the model remain unchanged. The details of this procedure and the updated assessment of the model predictions are presented below.

The extension to shear in Supplement Section 4.2 raises a related physical question. For fixed ground-relative frequencies, $\hat{\omega}_j(z) = \omega_j - k_j U(z)$. Modes sharing an intrinsic frequency at one level generally acquire different Doppler shifts elsewhere when their horizontal wavenumbers differ. Could the authors clarify the conditions under which one effective stratification profile can represent all modes? A shared vertical deformation does not follow automatically, even for linear shear.

(1) The extent to which the idealized model can represent a sheared wind profile has been clarified in the Supplement Section 4.2, where we have further developed and formalized the methodology outlined in the previous version of the Supplement, relating the cavity depth for arbitrary environmental profiles to that for a step-wise constant stratification. We considered two approaches with increasing levels of physical complexity. In the "thermal-only" approach (vertical variations in $N(z)$ accounted for, shear neglected), a local stretching factor was computed to relate the observed stratification profile to an equivalent idealized waveguide with a stepwise constant $N(z)$, through a vertical coordinate transformation that conserves phase. In the "thermal-Doppler" approach (vertical variations in $N(z)$ and $U(z)$ accounted for), the observed atmospheric profiles were mapped onto an equivalent idealized waveguide with stepwise constant $N(z)$, constant $U(z)$ and constant intrinsic frequency that is shared by all modes. Profiles of this stretching factor were computed from the radiosounding profiles, with the main objective of assessing whether the equivalent cavity parameters could be considered independent of the horizontal wavelength. For each approach, we have computed the depth (H) and lower boundary altitude (z_b) of the equivalent cavity, which become mode-dependent through Doppler shifting in the thermal-Doppler case. In particular, while z_b can be considered approximately constant, H varies significantly with the mode horizontal wavelength and, for the dominant mode observed in w (mode 2), the thermal-Doppler model predicts exactly the value of $H = 3.2$ km used in the previous version of the manuscript (see new Figures 6a and S9). Since the cavity parameters are now derived from the radiosounding profiles rather than tuned to reproduce the observed spectral peaks, the agreement between the predicted and observed mode wavelengths provides an independent quantitative verification of the idealized model. Further details on the calculation of the resonant wavelengths are provided in the new Section 4.1 of the main manuscript as well as in the Supplement.

(2) The vertical structure of the modes has been examined in detail (new Section 4.2 in the main manuscript). We have computed the observed amplitude ratios from Figure 6b (L415-419). Although these ratios do not match the theoretical values, the agreement is improved compared with the previous version of the model. According to the cavity model, the antinodes of u_2 and w_3 are separated vertically by only 50 m. This separation is smaller than in the previous version of the model, in which H was assumed to be constant, thus representing an improved qualitative agreement. Nevertheless, the simplifying assumption of a common $\hat{\omega}$ may affect the predicted amplitude ratios compared to a constant horizontal phase speed hypothesis, for instance. Furthermore, the exact altitudes of nodes and antinodes could be sensitive to even small differences in the background wind profiles. These aspects are discussed in more detail in the main manuscript (L422-425) and in the new Section 4.3 in the Supplement.

Major comment 3: Propagation reference frames and source attribution

Section 3.3, lines 245-254; Figure 4c; Table 2; Section 5.1, lines 484-496
The quadrature argument should distinguish intrinsic from ground-relative phase propagation. The linear ther-

modynamic equation is

$$(\partial_t + \mathbf{U} \cdot \nabla_h)\theta' = -w'\partial_z\overline{\theta}$$

Thus, upward motion determines the temperature tendency following the background flow; it does not alone determine the tendency at a fixed ground location. Table 2 illustrates why this matters: the intrinsic and ground-relative phase speeds along the 125° axis are +5.6 and −2.9 m s^{-1}. Please make the reference frame of Figure 4c and its fixed-location explanation consistent with these opposite signs.

Response: The reviewer is right in noting that the phase relationship between w' and θ' is described by the linearized thermodynamic equation in terms of the intrinsic, rather than ground-relative, frequency and we agree that the previous explanation, particularly the wording "fixed locations" in the caption of Figure 4, could be interpreted as describing phase propagation in a ground-relative frame. We have revised the argument used to resolve the 180° ambiguity, which relies on the phase convention defined in Eq. 3 and the observed spatial ordering of the maxima in w' and θ' along the aircraft track. As a result, the illustration previously shown in Fig. 4c is no longer necessary and has been removed. The text has been clarified accordingly (L260-268 in the revised version of the manuscript). This clarification also allows us to distinguish consistently between the intrinsic and ground-relative phase speeds reported in Table 2 (new text added at L273-274 in the revised manuscript, and in the table caption).

The exclusion of convection southeast of the packet also needs reconsideration: a source-to-packet connection concerns ground-relative group propagation, including background advection, rather than intrinsic phase direction alone.

Response: We agree that the source-to-packet connection should be assessed using the ground-relative group velocity. Considering a geographic horizontal coordinate system with x and y directed to the E and to the N, respectively, we can compute the ground-relative group velocity components as

$$c_{gx} = \overline{u} + k\frac{N^2 - \hat{\omega}^2}{\hat{\omega}(k^2 + l^2 + m^2)} = -7.3 \text{ m s}^{-1}, \; c_{gy} = \overline{v} + l\frac{N^2 - \hat{\omega}^2}{\hat{\omega}(k^2 + l^2 + m^2)} = -3.8 \text{ m s}^{-1}.$$

Thus, the ground-relative group propagation is towards the SW, hence it would be consistent with a source towards the NE. The convective region to the SE considered as a possible source does not lie along this propagation direction, implying that a direct source-to-packet connection with this region is not supported by the local ground-relative group velocity analysis. The text has been modified accordingly at L572-574 in the revised version of the manuscript.

An unidentified source additionally leaves its contribution to the observed spectrum unresolved. Environmental filtering remains a plausible influence, but its dominance over the forcing is not established by the absence of source identification.

Response: The reviewer is right in noting that the absence of an identified source does not by itself establish the dominance of environmetnal filtering. However, we still believe that the properties of the observed wave fields, which are similar across the synoptic-scale region of interest and throughout the day (Figure 1; also stated at L484-485 in the former version of the manuscript), may be strongly influenced by the propagation through the large-scale environment. The importance of propagation and selective environmental filtering is well established and is not a new idea (see, e.g., the already cited Lindzen and Tung, 1976; Monserrat and Thorpe, 1996; Román-Cascón et al., 2015), but in our opinion this aspect is often overlooked when interpreting observed gravity wave packets. In any case, we have tried to tone down some of our statements, in particular revising the wording to avoid implying that the source is unimportant, while retaining our main point that the wave characteristics may be largely controlled by the propagation environment.

Major comment 4: Common frequency and packet identification

Section 3.5, lines 364-370; Supplement, Sections 4.1-4.2.

A shared propagation direction does not establish a common intrinsic frequency or source; even one source can generate a broad frequency spectrum. Section 4.1 of the Supplement also identifies a differently oriented component, particularly for the first mode. Please distinguish the modes attributed to the main packet and present the common frequency as a modelling assumption unless it is independently constrained.

Response: The reviewer is right in noting that we should distinguish the modeling assumptions from what is independently constrained by the observations. The hypothesis of a common intrinsic frequency is now explicitly presented as a fundamental modeling assumption (L350), and its implications and potential alternatives are discussed in a new Section 4.3 in the Supplement. Following the model modifications described in our responses above, we show in the Supplement (Section 4.1) that, at flight altitude, between 17:18-17:23 UTC, the observed mode-specific hodographs exhibit weak oscillations, with the polarization ellipses aligned with the propagation direction of the main wave packet analyzed in this study. This packet displays its most conspicuous activity at the horizontal wavelength associated with mode 3. We also explicitly distinguish this wave activity from that observed between 17:16-17:18 UTC, which is associated with a secondary wave train propagating in a nearly perpendicular direction. Finally, the manuscript has been revised to remove claims about the source that were not sufficiently supported by the observations.

Minor comments

1. **Amplitudes at different altitudes.** Lines 217-222 combine lidar displacement below flight level with vertical velocity at flight level through $\tilde{w} = -i\hat{\omega}\tilde{\zeta}$. This relation applies locally. Please explain how the different measurement heights are reconciled with the vertically varying eigenfunctions invoked later (manuscript, p. 10).

   Response: In the revised manuscript, we have estimated the amplitude of vertical displacement at flight level following a kinematic argument suggested by Reviewer 1. This constitutes an independent verification and the resulting estimate is in good agreement with that obtained from the lidar measurements below the aircraft. The relevant discussion has been added as a new Section 2.2 in the Supplement. Furthermore, we have expanded the discussion on the vertical structure of the modes as predicted by the idealized model and its comparison with the displacement field inferred from lidar measurements in a new dedicated Section in the main manuscript (Section 4.3). In particular, the model predicts the first antinode of w_2 (above the lower cavity boundary) to be located close to flight altitude and that the wave becomes evanescent below, consistent with the observed exponential decay of in-phase signal below the aircraft.

2. **Direction of the dominant component.** *Lines 223-243; Supplement Section 4.1.* The hodograph in Figure 4b uses band-pass periods of $20 - 40$ s in the aircraft record, whereas the dominant w' and θ' signal has a crossing period of about one minute. Please make the connection between their propagation directions explicit, referring to the mode-specific hodographs in the Supplement where appropriate.

   Response: The hodograph in Figure 4b was constructed using a band-pass range centered around the dominant period observed in the (u', v') fields, which differs from that in (w', θ'). This difference is later explained by the cavity model, which interprets the two dominant wavelengths as distinct modes of the same wave train. This interpretation is justified by the common direction of propagation in the mode-specific hodographs, and their co-location. The text has been clarified at L255-258 in the revised manuscript, where the connection to the mode-specific hodographs is made more explicit.

3. **Water vapour observations.** *Section 2.2; lines 129-134 and 432-434.* Please identify the water-vapour channels used to support wave activity in clear sky and describe their effective resolution, vertical sensitivity and relevant processing. A representative image would help clarify how these observations support the inferred regional extent and relate to the proposed waveguide.

   Response: We have expanded the description of GOES-16 observations, including vapour-sensitive products, in Section 2.2, specifying the channels used, their horizontal and temporal resolutions and commenting

on their effective vertical sensitivity (L115-118 in the revised version of the manuscript). We have also added a new Section 1 and new Figure S1 in the Supplement, where we present and discuss representative images from channels 9 and 10 for 15 August, the day of our case study, and 12 April 2024, a day characterized by high upper- and lower-TraLI values. In spite of their coarser horizontal resolution, wave activity is still apparent in these images.

4. **Finite barriers.** The leakage estimate in Supplement Section 4.2 uses the neutral-exterior decay scale $1/k_h$. In a general evanescent region, the local decay parameter is $\kappa(z) = \sqrt{k_h^2 - S^2(z)}$. Please distinguish the idealized estimate from a conclusion about leakage through the observed thermal-wind barriers; their geometrical thickness alone does not determine attenuation.

   Response: The Supplement was revised to explicitly recall the assumptions of the idealized model (i.e., neutral exterior layers) under which our estimate is valid and to make the conclusion regarding leakage in the observed case more cautious. In particular, the unobserved mode 1 could be a consequence of tunnel effect, as its λ_x approaches the theoretical critical value (L426-428).

Technical corrections

Equations 12-13. Please use a quadrant-aware definition of the reflection phase, for example with atan2. This does not affect the central frequency ratio of the case studied.
Figure 6. Please specify the Fourier normalization and explain the "m" unit on the amplitude axis.
Page 4, line 112. Remove the stray "s" in "Yet, s a more detailed examination. . . " and change "examination . . . reveal" to "examination . . . reveals".
Page 22, line 469. Replace "key takeouts" with "key findings".

Response: The manuscript was revised in response to all these comments. Regarding the technical correction on Figure 6, the manuscript indeed presented a mistake in the units reported on the y-axis. Figure 6b presents the spectral density of the velocity perturbations as a function of wavelength, derived from the frequency spectrum using the aircraft speed (units: $\mathrm{m^2\ s^{-2}\ m^{-1}}$). This has been corrected and clarified in the manuscript.

References

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