Response to Reviewer 1

We wish to thank the reviewer for the careful assessment of our manuscript and for the constructive and insightful feedback. The suggested references were very relevant and helped us better position our work within the existing literature, in particular by bringing to our attention a few recent studies that we had not previously considered. Our point-by-point responses are provided below in blue.

Title

The formulation "... large-scale trapping ..." is somewhat misleading, as the paper does not provide direct evidence of trapping at large spatial scales beyond the visual similarity seen in the satellite imagery. On the other hand, "large-scale" could also be understood as referring to the observed wave packet itself being large in terms of its horizontal extent. The title could be revised to better reflect the content of the paper.

Response: We agree that the in situ measurements characterize a short wave train consisting of a half-dozen oscillations with horizontal wavelengths of about 6 km. To avoid confusion, we have revised the abstract to make the distinction between the short wave train sampled in situ and the broader spatial extent of the trapping-favourable conditions more explicit. However, our interpretation of large-scale trapping does not rely on satellite observations alone, but is further supported by the spatial extent of environmental conditions favourable to the formation of a free-tropospheric waveguide (Figs. 5, S6 and S7) and by the TraLI analysis, which constitutes the second part of the paper and quantifies the occurrence of such conditions over a $10° \times 10°$ domain. Applied to 15 August 2024, the index indeed identifies favourable conditions for wave trapping over an extended region. We believe that, taken together, these complementary lines of evidence are consistent with our interpretation of a large-scale trapping event.

Abstract

"Gravity waves are a key dynamical feature of atmospheric flows, yet remain difficult to simulate and observe. However, on 15 August 2024, widespread wave signatures were observed over a broad region of the northeastern tropical Atlantic, with remarkable spatio-temporal coherence." This is too general, and there are numerous papers on numerical simulations (from linear to non-linear ones) and many many observations of gravity waves. Specify the first sentence. Also: Why do you use "However, ..." in the following sentence?

Response: We agree that the original sentence was too general and have revised it by removing the reference to simulations, which are not the focus of this study, and by specifying that observations of GWs are particularly challenging in the troposphere, where GW signatures can be masked by larger wind and temperature fluctuations associated with, e.g., convection (Guest et al., 2000). This is also consistent with the point already made in the conclusions (L529-532 in the former version of the manuscript). We have also removed "However" from the abstract. Although it was intended to contrast the observational challenges with the widespread wave signatures documented in our case, we acknowledge that the transition was not particularly clear or useful.

"One such packet was sampled in situ by the research aircraft operated within the MAESTRO field campaign, providing an ideal case study." This sentence raises two issues. First, it is unclear why a single, spatially limited in situ sample should constitute an "ideal" case study of a phenomenon. Second, the reference point for the phrase "provides an ideal case study" is unclear: Since it appears immediately after "MAESTRO field campaign," it is not clear whether it is the measured wave packet, the measurement itself, or the campaign that "provides" the case study.

Response: By "ideal case study" we meant the rare combination of high-resolution in situ measurements, airborne remote-sensing observations, radiosonde profiles and satellite imagery available for this event, which we consider particularly valuable for the comprehensive characterization of the wave train. We agree that the original wording was ambiguous, while the description of the observational approach was already described in the following sentence ("We perform a comprehensive characterization of the wave packet through a combination of in situ, remote sensing and radiosonde observations..."). We have therefore removed the phrase "providing an ideal case study" from the abstract.

The last sentence: the reference to "local climatologies" and "previous studies" is vague. Specify which climatologies and which previous studies are meant.

Response: We have revised the last sentence of the abstract to specify that the main variations in TraLI are consistent with the expected seasonal evolution within the West African Monsoon system and with previous findings on gravity wave trapping in sheared flows (e.g., Stephan, 2020; Vicari et al., 2024). We would prefer not to add specific references to the abstract. The relevant literature was already cited in the main text (L404, L511 in the previous version of the manuscript).

Introduction

Observation of extended trains of short gravity waves in the free atmosphere is not new per se: see, e.g., Smith et al. (2008, J. Atmos. Sci., "Mountain Waves Entering the Stratosphere", T-REX, https://doi.org/10.1175/2007JAS2598.1) and, especially, Smith et al. (2016, J. Atmos. Sci., "Stratospheric Gravity Wave Fluxes and Scales during DEEPWAVE", https://doi.org/10.1175/JAS-D-15-0324.1, their Fig. 9). It must be acknowledged that the term "wave trapping" was not explicitly mentioned in these studies apart from the considerable horizontal extent of the wave trains.

Response: We have added the references to Smith et al. (2008, 2016) to the discussion of elevated waveguides in the introduction (as well as later in the text when talking about the estimation of vertical displacement based on the kinematic relation suggested by the reviewer). Smith et al. (2008) indeed found evidence of short trapped waves "riding on the tropopause inversion" during T-REX, while Smith et al. (2016) explicitly acknowledge that "One possibility is that the tropopause inversion layer acts as a wave duct" in their discussion of observations from the DEEPWAVE campaign.

Regarding the overview of recent wave trapping studies (ll. 45–56), the authors are encouraged to consider the closely related study by Dörnbrack (2024, J. Atmos. Sci., 'Transient Tropopause Waves'). Physically, contrasting its TIL-centered duct (likely triggered by stratospheric wave breaking) with the present study's free-tropospheric, non-orographic duct would provide an insightful comparison. Methodologically, the authors' strategy of identifying an 'extensive wave structure over a wide area [...] with remarkable spatiotemporal coherence' (ll. 63–64) via satellite data and verifying it in situ perfectly aligns with Dörnbrack (2024), who documented a similarly large-scale, coherent trapped wave event, additionally supported by downward-looking lidar measurements. In connection with the same discussion (ll. 45–56), the authors might also want to consider referencing the work by Eckermann et al. (2026, J. Atmos. Sci., 'Ground-to-Stratopause Trapped Lee Waves Observed Downstream of the Kerguelen and Heard Islands'). Including this recent paper would provide another excellent example of the growing interest in trapped wave dynamics and further strengthen the literature review.

Response: We appreciate the reviewer's suggestion and have added the references to Dörnbrack (2024) and Eckermann et al. (2026) to the relevant part of the introduction (L51-53 in the revised version of the manuscript). Although Eckermann et al. (2026) investigate a ground-to-stratopause waveguide trapping lee waves, which falls somewhat outside the focus of our discussion on elevated, non-orographic, tropospheric waveguides, we agree that their study is a valuable example of the renewed interest in trapped GWs and have included it accordingly. We have also added a comment to the final discussion (L565-566) highlighting the contrast between our case and that of Dörnbrack (2024), together with a related reference (Woods and Smith, 2010) that we consider relevant to this comparison.

The historical account of the Scorer-parameter and lee-wave-trapping literature (ll. 34–44) cites the two founding references (Queney, 1948; Scorer, 1949) before jumping directly to recent tropospheric-waveguide studies, skipping the substantial body of intervening work that operationalized and extended Scorer's framework (e.g., Corby and Wallington, 1956; Corby and Sawyer, 1958; Vosper, 2004). This gap is particularly relevant for the TraLI introduced later in the manuscript (Sect. 4): Coney et al. (2025, QJRMS) recently published a regional climatology of lee-wave trapping conditions over Britain and Ireland, also derived from the Scorer parameter, albeit using a deep-learning classifier rather than a simplified threshold. Acknowledging and positioning TraLI relative to this very similar approach would strengthen the originality argument of Sect. 4.

Response: We agree that the suggested literature provides useful context for our work, in particular Coney et al. (2025), which offers a relevant point of comparison for TraLI. We have therefore expanded the introduction to Section 5 (Section 4 in the former version of the manuscript) with references to previous climatological studies of trapping conditions using different approaches; in particular, we acknowledge the climatological analysis of lee-wave trapping by Coney et al. (2025) and clarify how our approach complements it (L439-445).

Line 21: What is a " ... vertical view of atmospheric gravity waves ... "? The phrase is unclear and should be clarified.

Response: We have revised the sentence to clarify that "vertical view" refers to a perspective that focuses on the vertical GW propagation and its associated effects, in contrast to the horizontal propagation discussed later when introducing wave trapping.

Line 28: There is a dot behind Lane in the citation, which should be removed. Additionally, there is a nice case study by Heller, et al: Mountain waves modulate the water vapor distribution in the UTLS, Atmos. Chem. Phys., 17, 14853–14869, https://doi.org/10.5194/acp-17-14853-2017, 2017. which is an observational study of tracer transport.

Response: We have corrected the typo in the citation and added the suggested reference.

Line 54: "The occurrence of trapped waves appears more likely in the presence of significant shear in the upper troposphere..." It might be nice to also acknowledge the classic works of Kuettner (1971), Clark, Hauf and Kuettner (1986), and Kuettner, Hildebrand and Clark (1987) here. The presence of shear as a key condition enabling coherent, far-reaching propagation of gravity waves above convectively ABLs was indeed one of the central results of these foundational studies, and a brief nod to them would nicely connect this statement to this established body of work. By the way, every glider pilot knows the relationship between shear and wave propagation, as it is a key factor in the formation of wave lift above the convective boundary layer.

Response: We have added references to Clark et al. (1986) and Kuettner et al. (1987) in the introduction, immediately after mentioning the role of vertical shear for GW trapping (L37-38 in the revised version of the manuscript).

Section 3

Figure 1: Could you draw lines from the red boxes in panel a to the respective panels b–f?

Response: We have added connecting lines from the red boxes in panel (a) to the corresponding panels (b–f), which are now outlined in red to make the correspondence clearer. To minimize line crossings and improve the overall readability of the figure, panels (b–f) have been arranged according to their spatial proximity to the boxes in panel (a), rather than in chronological order.

L. 112: delete "s" after the commata.

Response: Done.

L. 116: a reader would benefit if you explicitly state that these regions occur at different times and places.

Response: We have added this clarification immediately after the first reference to Figure 1 in the manuscript.

Ll. 133/134: "must have been": can you provide evidence or it is just a speculation?

Response: We have toned down the statement and, also in response to a comment from Reviewer 3, added a new Figure S1 and new Section 1 in the Supplement, where we present and discuss representative examples of GOES-16 water-vapour imagery. These observations support our interpretation that the wave signatures also extend into clear-sky regions.

Eq. 1 could be nicely combined with the direct computation of the vertical displacement via the purely kinematic relation used by Smith et al. (2008, 2016; see also the remark on Smith et al. above):

$$\zeta(x) = \int_0^x \frac{w'(x')}{U(x')} dx',$$

where w' is the perturbation vertical velocity and U is the along-track wind speed (Smith et al., 2016, their Eq. 12, following Smith et al., 2008). This would provide a useful, independent cross-check of $|\tilde{\zeta}|$ – obtained here from the polarization relation (Eq. 6) – without requiring prior knowledge of N or $\hat{\omega}$.

Response: We have performed the computation of ζ using the kinematic relation suggested by the reviewer. The results are shown in the following Figure R1 for the same segment considered in Figure 3 (17:16-17:23 UTC). The resulting displacement signal exhibits a maximum half peak-to-peak amplitude of approximately 125 m (when individual oscillations are considered) and a root mean square (RMS) amplitude of 70 m at flight level. The former is broadly consistent with our original visual estimate of 150 ± 60 m based on the lidar backscatter data and we note that, throughout the manuscript, wave amplitudes have been estimated from the most prominent oscillations rather than using an RMS measure. For instance, for θ we report $|\tilde{\theta}| = 1.2$ K, while the corresponding RMS amplitude would be 0.63 K. This figure and related discussion have been added to the Supplement (new Section 2.2), with a reference to this result also included in the main text.

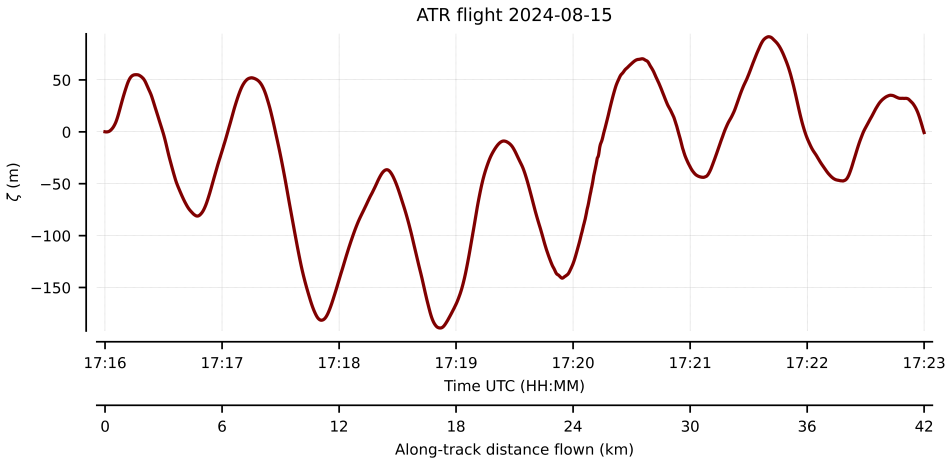

Figure R1: Vertical displacement ζ at flight level, computed using the kinematic relation of Smith et al. (2008, 2016), for the same flight segment shown in Figure 3 (17:16-17:23 UTC).

L. 155: Which formulas are used to compute the ice and water saturation mixing ratios for RH_w and RH_i? It would be helpful to provide the formulas or at least a reference.

Response: The relative humidities with respect to liquid water and ice are computed as

$$\mathrm{RH}_w = \frac{e}{e_{s,w}}, \quad \mathrm{RH}_i = \frac{e}{e_{s,i}},$$

where e is the vapour pressure and $e_{s,w}$ and $e_{s,i}$ denote the saturation vapour pressures with respect to liquid water and ice. For the saturation vapour pressures we use the formulations of Ambaum (2020) (Equations 13 and 17, respectively). The reference has been added to the caption of Figure 3 rather than to the main text, as we felt that adding this detail there could interrupt the flow of the presentation.

L. 156: replace "multiple" by "six"

Response: Done.

L. 156: The term "nearly monochromatic" caught my attention, given that the same signal is referred to as a "wave packet" elsewhere in the manuscript. Strictly speaking, a wave packet implies an amplitude-modulated wave train composed of a range of wavenumbers. If "wave packet" is instead used here in a more generic sense – simply meaning "a short train of waves" – rather than in this stricter technical sense, it would be helpful to clarify this to avoid ambiguity.

Response: We appreciate this helpful terminological clarification and agree that "wave packet" may imply a more specific meaning than intended here, particularly in the context of a nearly monochromatic signal. We have therefore replaced "wave packet" with "wave train" throughout the manuscript to avoid ambiguity.

Figure 3: The plot would benefit from an additional x-axis showing the distance in km.

Response: We have added an extra x-axis showing the along-track distance flown (in km), which helps assess the horizontal wavelength of the observed signal.

In Section 3.3, two different cases are combined to provide evidence that KHI can be ruled out for the case under consideration. The readability of this subsection could be greatly improved by integrating the relevant material from the appendix into the main text and by contrasting the two cases directly. Otherwise, the reader has to move back and forth between Section 3.3 and the appendix. Alternatively, this part (as it is important for the whole paper) could be an own new section.

Response: We have reorganized Section 3.3 to clarify the evidence supporting our interpretation of the 15 August event as an internal gravity wave. The nearly sinusoidal structure of the observed signal, together with Richardson number estimates well above the critical threshold at flight level, already support the conclusion that KHI is unlikely in this case, without the need to reference the event from 25 August 2024. We now present the latter as a complementary comparison in the Appendix, with only one reference to it at the end of the subsection to avoid repeated cross-referencing. While this additional case reinforces our interpretation of the 15 August observations, we felt that incorporating it in detail into the discussion of the main case study would interrupt the narrative flow.

L. 194: What are " The unforced momentum ... equations". I assume, there is still Coriolis force, buoyancy, pressure gradient as forces in the equations. Please, clarify.

Response: This set of equations retains the Coriolis, buoyancy and pressure-gradient terms. We have revised the text to refer explicitly to Eqs. (7)-(11) of Fritts and Alexander (2003), which are the linearized forms of the momentum, mass and energy conservation equations derived from their Eqs. (1)-(5) with the forcing terms removed, as described by the authors.

Table 2: The vertical wavelength is rather small. How can this value be explained? By the thickness of the inversion layer or the depth the wave duct?

Response: The small vertical wavelength is explained through the idealized cavity model presented later in the manuscript. We have added a cross-reference to the cavity-model section in the relevant discussion (L275 in the revised manuscript) and included a comparison between the model results and the observations at the end of that section (L404-407).

Ll. 280/281: The statement "Above this layer, dry static stability increases before decreasing again above roughly 10 km" oversimplifies the N^2 profile shown in Figure 5c, which also exhibits a secondary, weaker stratified layer between 11–13 km (as the authors themselves note a few sentences later). Consider rephrasing for consistency with this more detailed description.

Response: In fact the original description was somewhat simplified as it was intended to capture the main features of the vertical stratification. We acknowledge that a more detailed description is needed, particularly to ensure consistency with the subsequent discussion, and have revised the text accordingly.

Ll. 285–286: The statement that the stratification "was already evident in the radiosonde profiles from August 14 . . . [and] that the well-mixed layer had decreased significantly by August 16" is not supported by Figure 5, which, according to the caption, shows only profiles from August 15, 2024. Either reference the correct supporting evidence (e.g. an additional panel or supplementary figure) or remove/qualify this comparison.

Response: We have added to the Supplement a new Figure S7 showing the vertical profiles of θ from all radiosonde ascents at Sal Island on 13 August (panel a), 14 (panel b) and 16 August (panel c). For reference, the profile from the 15 August radiosonde ascent at 17:24 UTC is also included, represented as a dashed orange line.

Figure 5: Could you add ticks at the x-axis for N^2?

Response: In response to this comment, we have added one major tick with its label and two minor ticks to the x-axis in panel c.

Section 3.5: Although this subsection is remarkably clearly formulated, its scope warrants a separate and dedicated section. Furthermore, the manuscript would benefit from the inclusion of historical references to the theory of multilayer linear gravity wave theory, such as those found in the textbook by Gossard and Hooke (1975). Explicitly highlighting the incremental progress or distinguishing this work from these pioneers would significantly strengthen its positioning within the field. It might be also worthwhile to check the literature of trapped waves in the mesosphere and stratosphere. I vaguely remember papers seen at conferences and in the J. Geop. Res.

Response: We have dedicated a separate, full section to the cavity model, in particular as it was substantially expanded following the other reviewer's comments. We have also added references to relevant earlier work on linear GW propagation and trapping in idealized, multilayered atmospheric structures to better contextualize our approach within the existing literature (L346-349).

Figure 6 reminds me of the figures 9-12 of Smith and Kruse (https://doi.org/10.1175/JAS-D-16-0297.1): w-power at small scales, u-power at large scales. A hint on this similarity in airborne observations in the free atmosphere close to inversion and shear layers (Smith: tropopause; here: ABL inversion layer) would be nice.

Response: We do not think that the present observations allow us to draw conclusions on the relative powers of u and w. In addition, this is a direct blind spot of our idealized model (discussion added in the new Section 4.3 in the Supplement) in which the unconstrained emission spectrum could strongly influence the amplitude ratio across scales. We believe that such an investigation would be of interest, but lies outside the scope of the present study.

Either Section 3.1 or 3.4: Interested in this case study, I briefly analyzed the synoptic situation on the afternoon of August 15, 2024 (see figure above). The reason for doing so was simply to determine what caused the curved phase lines in the gravity wave field. This question was neither raised nor answered in the manuscript. First, the event was located north of the ITCZ (TCWV < 48 kg m^{-2}). Second the observations were conducted in a region where a low pressure system was located to the southeast. At its northwestern edge strong easterly winds with cyclonic curvature dominated. The curvature of the streamlines nicely corresponds to some of the gravity wave phase lines seen in the satellite imagery. A short discussion of this flow pattern in the manuscript would be great to place the observations in a meteorological context.

Response: We wish to thank the reviewer for this interesting analysis. Based on our inspection of the satellite imagery, we interpret the phase lines as predominantly straight rather than curved (cf. Fig. 1e,f). Moreover, regarding the proposed link between the curvature of the streamlines and that of the phase lines, we note that a similarity in orientation at the aircraft sampling location is apparent at 600 hPa, but not at 500 or 450 hPa. The aircraft was flying at a pressure level of approximately 506 hPa, while the cloud bands revealing the wave crests in satellite imagery were located about 1 km above, likely between $400 - 450$ hPa. We therefore consider the correspondence between streamlines and phase lines too limited to draw robust conclusions. We nevertheless agree that a brief description of the synoptic situation helps place the observations within their broader meteorological context and have therefore added some details to the case study introduction (L121-123).

Section 4

L. 381: As far as I understand, your search for a "suitable approximation model" for S2 ends here with the Scorer parameter, which essentially applies to atmospheric flow over mountains in the direction of the mean wind (Equation 14). Well, this can be done as a first step. Better would be to use a plausible range of phase speeds and directions. I agree, such an approach would be challenging but modern computer architectures should enable such a study. Moreover, the use of pressure levels instead of higher resolved model levels is critical as you compute vertical derivatives. In the text, most of the limitations are mentioned, so I see no need to change the text principally. However, I would like to see a more detailed mentioning of the limitations of the TraLI index in the conclusions, not just the rather qualitative and vague formulation " ... purposefully defines a conservative criterion ...".

Response: We agree that a comprehensive computation of the simplified Scorer parameter for a range of plausible directions and phase speeds would be ideal. In response to this and a related comment from Reviewer 3, we have extended our analysis to examine how the definition of TraLI depends on the specific choice of the directional projection (considering the lower-tropospheric wind direction) and on the inclusion of a representative phase speed (5 m s^{-1}, antiparallel to the upper-tropospheric wind). This analysis was conducted for a two-month subset of the original period and is now discussed in detail in a new Section 6 of the Supplement. This additional analysis provides insight into the sensitivity of TraLI to these assumptions and supports the use of its original definition as a reasonable compromise between ease of computation and physical consistency for extending the case study to a regional climatological perspective. Following the reviewer's suggestion, we have also revised the Conclusions to state more explicitly the assumptions and limitations of TraLI (L595-599 and L607-609 in the revised manuscript).

Section 5

I agree that the identification of the gravity wave sources is of secondary importance for the present study. What is unclear to me is the role of the Saharan dust layer in the dynamics of this event. There is the study by Chouza et al. (2016, Atmos. Chem. Phys., 16, 4675–4692, "Vertical wind determined using airborne lidar and analysis of island-induced gravity waves in combination with numerical models and in-situ particle measurements," https://doi.org/10.5194/acp-16-4675-2016). It seems that more than just the geographical proximity of the observation of trapped (resonant) waves serves as a further point of commonality. These airborne coherent Doppler wind lidar measurements plus the in situ particle measurements and numerical modelling characterize gravity waves in the Saharan dust layer. This is a methodologically close approach to the combination of RASTA/LNG lidar, in situ microphysics, and model comparison employed here. Both the geographical overlap and the methodological similarity could also support the argument regarding regional representativeness that underlies the TraLI climatology discussed in Section 4.

Response: The work by Chouza et al. (2016) provides an interesting regional and methodological comparison with our study, given the geographical overlap and the combination of in situ and remote-sensing airborne observations. The documented occurrence of trapped gravity waves in the presence of the SAL, together with the use of a simplified version of the Scorer parameter, makes this study particularly relevant to the discussion

of TraLI variability in our study region. We have therefore added a reference to Chouza et al. (2016) at L549-551 in the revised manuscript. We note, however, that the waves investigated in that study are orographically generated and, in the vicinity of Cape Verde, extend up to the bottom of the SAL, while in our case the wave train is located above it. In our case, the well-mixed structure of the SAL and the stable stratification above might be relevant to GW generation. Nonetheless, since we consider the large-scale environment to be more relevant than the specific characteristics of a source, we have chosen not to elaborate further on possible wave generation mechanisms related to the SAL.

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