

Authors response to reviews on egusphere-2026-413

Umbarkar et al.

We thank the referees for their comprehensive comments on the manuscript. We have carefully addressed all points raised and revised the manuscript accordingly. As the manuscript has undergone major revision, we first list the central changes in the revised version of the manuscript to give an overview.

Reviewer comments are given in *italic*, replies in standard and corresponding revised text passages in *blue*.

Central changes in the revised manuscript

- We have modified the title from "Co-occurrence of gravity waves, vertical wind shear and turbulence in the lowermost stratosphere over the North Atlantic" to **"Interannual co-variability of gravity waves, vertical wind shear and turbulence in the lowermost stratosphere over the North Atlantic"**.
- We want to note that the main goal of this study is to determine whether previous findings based on idealized simulations (Umbarkar and Kunkel, 2025) and real-case study (?) can be confirmed in the long term reanalysis data. The first paper on the description of the tropopause following shear layers has been published by Kaluza et al. (2021). There the existence of this frequent occurrences of the enhanced shear just above the tropopause has been described for the first time, essentially with the same data set which we use in this study. The focus of the study by Kaluza et al. (2021) was on the first description of this tropopause shear layer, its hemispheric variability and its link to balanced flow. In consequent studies, the focus has shifted towards the contribution of small-scale (unbalanced) dynamics which are often associated with gravity waves (Umbarkar and Kunkel, 2025; ?). In one of these studies numerical experiments have been used to identify the link between small-scale gravity waves and enhanced shear. This relation was evident in various simulations of baroclinic waves and was quite robust across various initial conditions and also analysis time steps in the later stages of the life cycles of baroclinic waves. In a consequent study, we analysed a case study of baroclinic wave over the North Atlantic to assess whether we find the same relationship. Again we find a strong correlation between gravity wave occurrence and enhanced shear. In the latter study, we also have measurements of temperature, wind, pressure and trace species at hand and provide evidence that irreversible mixing of trace gases took place in the region of enhanced shear and gravity wave occurrence. These findings motivated us to further look into this relation and whether the relation between gravity wave occurrence and enhanced shear shows this relation over the course of 10 years in the North Atlantic.
- Following the suggestions by a referee and the editor to enhance the clarity of the manuscript and to include the data with high available temporal resolution, we revised the result section comprehensively. We present the synoptic situation

based on ERA5 reanalysis in sect. 4.1, where we also introduced the gravity wave, turbulence and shear diagnostics for individual cases. We then use these diagnostics to obtain a long term perspective for a ten year time period based on 6-hourly input. We thus gain insights on explicitly how ERA5 sees the role of GWs to vertical shear and turbulence based on small-scale perspectives with finer temporal resolution. In detail, we revised figures 2–8 and 10–16 of the original manuscript. We note here that based on the increased data input, we obtain a slightly clearer picture with higher occurrence frequencies of enhanced shear above the local dynamical tropopause. This is in line with the overall conclusion of the revision of the manuscript. Although we now have a much more comprehensive data set in terms of temporal resolution and period, our main results and major conclusions do not change to a significant degree.

Response to Reviewer 2

Major comments:

Comment: 1) Writing: *I admire the length to which the authors have gone to clearly and logically establish the connection between gravity waves and the enhanced shear in the lower stratosphere, but the manuscript is unnecessarily longer than it needs to be and can be significantly trimmed (without losing content). Moreover, the manuscript would be much clearer and enjoyable to read if the authors could reframe some sentences in the results section and move some plots to the SI. For instance: - I would suggest moving Figures 6, 8, 10, and 14 to the SI and referring to them briefly in the text. Figure 6 has been discussed briefly. T1 and T2 have similar structures, so one of the two figures can be moved to the SI. Similarly, only one of the VMFC and GW momentum flux maps is needed in the main body. Figure 14 (histogram) is only needed for a quick reference as the crux of your argument is illustrated in Figure 15 (scatterplot). I found having this many plots very distracting. This was aggravated by the unclear writing in multiple places (see next point)*

Reply: Thank you for this suggestions. We have revised and trimmed the lengthy content throughout the manuscript and also moved figures suggestive to the supplementary and referenced them in the manuscript accordingly. We now have 23 pages in the draft format whereas it was 26 pages in the discussion manuscript.

Comment: *- Every subsection begins and ends with redundant text, which in my opinion, disturbs the reader's flow. For example, L395-400 is not needed. Section 4.3.1, the first 4-5 lines are not needed. The final two lines of section 4.3.1 are redundant. Likewise, I would say the first 7-8 lines of section 4.3.2 are not needed. Similarly, L175-180: part of this can be moved to the conclusion section, and the remaining half removed. I found these forced 5-6 line segues, subsection after section quite distracting.*

Reply: Thanks for noting this. We have revised the entire subsections with the appropriate sentences and removed the redundant text as suggested.

Comment: *I appreciate the effort that went into writing this, but I would recommend the authors to revise the text. It is easily possible to cut 2-3 pages off the text (apart from moving above said figures to SI) while improving readability.*

Reply: Done. Changes can be seen in the revised manuscript.

Minor Comments:

Comment: *- The introduction can benefit from a revision. For instance, L54-73 read mixed up, with a couple of sentences on dynamics, followed by a couple of sentences on applications, followed by a couple of sentences on data, followed again by a couple of sentences on dynamics. This is easily seen on lines 63-74.*

Reply: Thanks for pointing this out. We have revised the introduction as suggested. We now start with dynamics (L55–60 in revised manuscript) followed by the implications of turbulence for atmospheric mixing and aviation (L60–65 in revised manuscript). We then discuss the specific motivation for focusing on the North Atlantic and state the objective of the present study (L66–72 in revised manuscript).

Comment: *- L89: Are sponge layers and hyperdiffusion applied on all levels?*

Reply: The ERA5 reanalysis data incorporates sponge layer and hyperdiffusion to attenuate artificial wave reflections and stabilize the model numerically. The IFS model, which is used to produce ERA5, applies a sponger layer near the model top to prevent spurious wave reflections. Since our analysis focuses on the UTLS, well below the model-top sponge layer, its direct influence on our results is not expected. On the other hand, hyperdiffusion (horizontally) can be applied on all levels but would not affect our analysis since the scales we are looking at are usually not damped by the hyperdiffusion scheme.

Comment: *- L113: Asian continent? Please specify longitudes?*

Reply: Done! the sentence now reads as

Over the Asian continent extending into the western Pacific (around 40 °E to 160 °E), the PV structure exhibits a sharp meridional gradient that coincides with a coherent subtropical jet.

Comment: *- L115: Rossby or gravity? Which one is it?*

Reply: We meant 'gravity wave'. Corrected in the revised text.

Comment: *- L145: provide citation?*

Reply: Done! Ellrod and Knapp (1992) has been cited.

Comment: - L174: Where exactly is this shown?

Reply: Horizontal wind vectors shown in Fig. 3j,k indicates the jet axis is displaced farthest south.

Comment: - L180-185: Do the authors have an intuition on why March (springtime) is stronger than February? Such anomalies are also on L188 (May is stronger than Feb). Similarly, Figure 12 and the discussion around L414-419.

Reply: We thank the reviewer for pointing this out. The stronger activity in March compared to February, and similar month-to-month variations such as the enhanced activity in May, reflect seasonal variability rather than a strictly monotonic seasonal cycle. Importantly, we observe similar features in our 10-year climatology, indicating that these variations are not unique to the particular year analyzed here. We have clarified this point in the revised manuscript.

Comment: L186: I wouldn't use the word "band" for this, since the structure is localized for some months and quite patchy. Don't you agree? Possibly, multi-year averages produce a band?

Reply: Agree. Now that the study is performed for over the 10 years, it is seen as a 'band'. The revised text now reads

Throughout the year, strong wind shear (magenta) occurs predominantly near the jet core, where each region can be attributed to a planetary circulation feature...

Comment: Figure 2: If is possible to superimpose the zonal mean zonal winds in Figure 2 itself? Then the reader can see the connection in the shear and the mean wind from the get-go

Reply: We appreciate this suggestion. We have chosen not to superimpose the zonal-mean zonal wind on Fig. 2, as the primary purpose of this figure is to highlight the seasonal and tropopause-relative distribution of strong shear occurrence. The connection between the background jet and strong shear is examined explicitly in Fig. 3. We therefore retain Fig. 2 in its current form to avoid overcrowding the figure and to keep the two analyses distinct.

Comment: Figure 3: Possible to adjust the pink shading? Difficult to assess the zonal wind map for some months due to the overpowering pink above it.

Reply: We agree. We have modified the figure accordingly.

Comment: L231: You mean GW dissipation? Because GW activity is high even down below in regions with high Mfs

Reply: Not exactly. There happens to have a GW propagation of waves with medium frequency that can also deposit momentum therein. But dissipation can also be reasonable.

Comment: L231: If the gradients are high, does it mean limited transport (and not substantial transport)?

Reply: The strong gradient in the tropopause relative vertical coordinate usually indicates a sharp transition across the tropopause. In fact, if a strong gradient is maintained, it can indicate limited mixing/transport across that layer.

Comment: Figure 6: Isn't the climatological shear layer above the region of potential instability? So, is the argument that mixing due to GW-induced turbulence in the region below can create a highly stable region above it (as shown in Figure 2).

Reply: Right. This has been explicitly stated in the revised manuscript.

Comment: L265: Can you clarify what “tropopause-based coordinates” are? Are you simply referring to the dynamic tropopause and the 380 K isentrope? This phrase has been used repeatedly. Sorry if I missed the definition.

Reply: Throughout the manuscript, the “tropopause-based coordinates” are referred to **rearranging the grid volumes in a tropopause-relative vertical coordinate system depending on their vertical distance from the local tropopause**. In detail, in the tropopause-relative coordinate system, we first sort the grid volumes (e.g., those satisfying $S^2 \geq S_t^2$) according to their vertical distance dz from the instantaneous local dynamical tropopause (TP). We then rearrange these grid volumes using the tropopause-relative averaging approach, in which their absolute altitude is reconstructed as $\overline{z(\text{TP})} + dz$ from TP, where $\overline{z(\text{TP})}$ denotes the mean altitude of the local tropopause and dz denotes the vertical distance from the instantaneous local tropopause. The resulting distribution therefore represents the grid volumes satisfying $S^2 \geq S_t^2$ in a tropopause-relative framework, while retaining a corresponding altitude coordinate (e.g., Birner et al., 2002; Hoor et al., 2004; Kaluza et al., 2021). We realized that this needs to be mentioned in the manuscript more clearly and is now added to the revised version on Line 133-135 as

In a tropopause-relative coordinate system, grid volumes are first sorted according to their vertical distance dz from the instantaneous local dynamical tropopause (TP). They are then rearranged relative to the mean tropopause altitude as $\overline{z(\text{TP})} + dz$ from TP, yielding a tropopause-relative distribution while retaining the corresponding altitude coordinate.

Comment: L288: Analysis? You mean this is your hypothesis?

Reply: Not exactly. This statement is based on our findings for small-scale GWs regime considered in this study.

Comment: L305: Maybe I missed it somehow, but isn't the vertical extent of TI2 broader than TI1, rather than more sharply confined as mentioned?

Reply: Right! That can be said in other words. The has been revised accordingly.

Comment: L314: suggests the substantial $\rightarrow$ suggests the possibility of substantial.

Reply: Done!

Comment: L316-319: Maybe move this to the conclusion section?

Reply: Done!

Comment: L331: 25 $\rightarrow$ 30N?

Reply: Corrected!

Comment: *L428: turbulence is a rare event: I am not sure I understand this statement. Are you referring to wave-induced turbulence? Otherwise, the atmosphere is highly turbulent with a high Reynolds number and ample perturbations. In fact, the prevailing turbulence all the way to the Rhines scale maintains the midlatitude jets. Isn't it?*

Reply: We are referring to wave-induced turbulence. This has been clarified in the revised manuscript.

Comment: *Figure 15: Interesting scatterplot. Does this mean gravity waves need to carry MF above a threshold to be able to induce inversion and turbulence in the UTLS? Which means even low-amplitude waves can still manage to induce inversions provided their vertical wavelengths are large enough and/or their horizontal wavelengths are small enough?*

Reply: We thank the reviewer for this interesting interpretation. Our analysis does not identify a distinct MF threshold required for the occurrence of strong shear/turbulence. Rather, the scatterplot indicates that enhanced shear can occur over a range of MF values. This is consistent with the fact that wave-induced shear depends not only on wave amplitude (and hence MF), but also on the wave scales, particularly the vertical wavelength. Thus, relatively low-amplitude GWs may still produce substantial vertical shear when their vertical wavelength is sufficiently short. However, a quantitative assessment of the dependence on GW wavelength is beyond the scope of the present analysis.

Besides this, the physical mechanism behind GWs contribution to shear is discussed in Umbarkar and Kunkel (2025), where they consider the vertical propagation behavior of GWs in a strongly stratified environment. Specifically, as small-scale GWs cross the tropopause, the strong vertical gradient in buoyancy frequency could lead to a shortening the vertical wavelength of GWs. This, in turn, increases the vertical gradients of horizontal wind perturbations and thereby enhances local vertical wind shear. This interpretation is consistent with the theoretical framework described in Wei and Zhang (2015); Kunkel et al. (2019). The enhanced shear then become responsible for the generation of potential turbulence as it precedes the static stability. We hope that this explanation answers the point raised by reviewer.

References

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