

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 (Umbarkar et al., 2026) 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; Umbarkar et al., 2026). 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 1

Major comments:

Comment: C1. The manuscript frequently highlights the "importance" of GWs in TSL formation. To make this argument more compelling, it is necessary to explore the specific physical processes involved. For instance, are these TSLs a result of GW breaking and subsequent momentum deposition, or are they the result of the superposition of large-amplitude waves? Additionally, providing information on the characteristics of the waves (e.g., their sources and properties) would greatly enhance the scientific depth of the study. What kind of GWs are responsible for the formation of TSLs?

Reply: This is an important issue which has at least partly been addressed in previous studies (Umbarkar and Kunkel, 2025; Umbarkar et al., 2026). To summarize, the situation is often such that vertical shear is generated by the background flow and additionally superimposed by shear from GWs. This can then lead to such strong shear events that locally and intermittently shear induced turbulence occurs, which is indicated by low Richardson numbers. The GWs do not necessarily break down but parts of the wave packets or additional wave components can continue to propagate vertically and transport momentum (e.g., Fritts and Alexander, 2003; Tsuda, 2014; Kunkel et al., 2014; Achatz et al., 2024).

Regarding the GW characteristics including sources and properties, we have discussed this in short in section 4.1 where we give some examples of different GW-shear events for each season. This information is included in the manuscript. We also note that the sources or source mechanisms and characteristics of the GW are not the primary focus of our analysis.

Comment: C2. While ERA5 offers hourly resolution, the current analysis relies on once-daily data. Since both gravity waves and turbulence are transient and evolve on much shorter timescales, I encourage the authors to utilize the high-frequency (hourly) data. This would allow for a more rigorous assessment of the temporal evolution of these phenomena and provide more robust evidence for the study's conclusions.

Reply: During the revision we altered our data basis so that we now use 6 hourly data instead of 24 hourly data and extended our analysis for over 10 years from January 2016 to December 2025. From previous analyses, we were aware that the relation

between shear, absolute momentum flux and turbulence indices is not too sensitive when this is done over an area as we used it here in our study. This is why we initially decided to use a one time step per day analysis here. We want to note here that, the shear layer occurrence frequency for 6-hourly annual data is not very sensitive in the tropopause region to the inclusion of daily input time steps as long as we stay on the monthly analysis basis (as is evident from Fig. 2). Of course, 6 hourly data comes with some benefits in terms of improved statistical basis as well as better temporal resolution for transient features, while retaining the relation we intended to describe. We therefore included 6-hourly data in the revised version.

Comment: C3. *At present, some of the conclusions regarding the "role" of GWs appear to precede the physical evidence. I suggest re-evaluating the causal links to ensure that the data clearly supports each step of the argument. Framing the results more cautiously or providing the missing physical links will help avoid the impression of a circular reasoning.*

Reply: We have re-evaluated the causal links between GWs, shear and turbulence processes in the revised manuscript and clarified our conclusions based on the results presented therein. In our revised version, we will take this comment serious and put more emphasis on the role of GW in the formation of the shear layers and potential consequent emergence of turbulence. We hope that the revised manuscript satisfactorily address issues raised by reviewer.

Specific Points:

Comment: l. 29: *Since the topic shifts after the phrase "On both hemispheres," I suggest starting a new paragraph here.*

Reply: Done!

Comment: l. 101: *Is there a specific reason for defining the dynamical tropopause at 3.5 PVU instead of the more conventional 2.0 PVU (e.g., Hoskins et al., 1985; Kaluza et al., 2021)?*

Reply: We use a slightly more stratospheric PV value to identify the tropopause for two reasons. First of all, we used this threshold for the tropopause in our previous studies. From experience, the results are not very sensitive to the choice of this PV value. But second, the 3.5 PVU is more conservative in the region of the jetstream (see Weyland et al., 2025; Turhal et al., 2024) and provides slightly better results in this region.

Comment: ll. 137-138: *It appears there is a grammatical error; the sentence should likely read: "The shear perturbations are calculated..."*

Reply: Right. The text has been modified to

The shear perturbations are calculated using...

Comment: e.g., l. 173: It is difficult to characterize a monthly variation observed only in a single year (2017) as an “annual cycle,” as such a variation likely includes both the mean annual cycle and interannual variability. I recommend using a more appropriate term to describe this specific variation.

Reply: Thank you for this comment. In the revised manuscript, we include 10 year reanalysis data, that allow us to characterize the climatological annual cycle more robustly. The corresponding text has been updated to reflect this change.

Comment: l. 175: This sentence (“This finding...”) is somewhat abrupt and lacks clarity. Please specify which large-scale dynamics are being referred to and what roles they play. Furthermore, there appears to be a logical leap regarding the definition of the Richardson number. It would be necessary to present the shear (or Ri) associated solely with the large-scale wind to demonstrate the contribution of large-scale processes to TSL formation.

Reply: With ‘large-scale dynamics’ we are referring to ‘large-scale processes associated with baroclinic life cycle viz. jet-front system, upper tropospheric fronts, jet dynamics’. We also clarify that the Richardson number discussed here is based on the total vertical wind shear and stratification, and therefore, does not isolate the contribution of small-scale wind shear alone. On the other hand, the shear associated solely with the large-scale wind is beyond the current scope of the manuscript and is currently being investigated by a colleague. We added following text in the manuscript.

L175: This finding supports the view that large-scale dynamics associated with baroclinic life cycle, viz. jet-front system, upper tropospheric fronts, jet dynamics, play an important role.

L143–147: Ri can be defined as the ratio of static stability (N^2) to vertical wind shear (S^2). It is commonly assumed that a Richardson number below a critical value is required for a Kelvin-Helmholtz instability to occur. The critical value for linear shear instability of an idealized stratified flow is often determined to be 0.25; however, for model based analyses values on the order of 1 are often assumed to characterize regions susceptible to Kelvin-Helmholtz instabilities (e.g., Kunkel et al., 2019). Note that the total vertical wind shear is used to compute Ri in order to initially evaluate regions susceptible to turbulence within the baroclinic flow.

Comment: Fig. 3: The magenta shading is difficult to distinguish from the shading used for the 200 hPa zonal wind.

Reply: The figure has been updated in the revised manuscript.

Comment: l. 184: “A intensity” is grammatically incorrect. Did the authors mean “intensification of meridional baroclinicity” or “intense meridional baroclinicity”?

Reply: Thank you for pointing out this grammatical error. We intended to refer to an “intensification of meridional baroclinicity,” and the sentence has been corrected accordingly.

The seasonal displacement is accompanied by an intensification of meridional baroclinicity and upper-level jet stream strength.

Comment: l. 188: The phrase “A planetary circulation feature” is too vague. Could the authors briefly elaborate on the discussion in Kaluza et al. (2021) to provide better context?

Reply: We have replaced the vague phrase “planetary circulation feature” with a more specific description referring to the subtropical jet and the associated baroclinic frontal zone discussed in Kaluza et al. (2021).

Throughout the year, strong wind shear (magenta) occurs predominantly near the jet core, where each region can be attributed to a planetary circulation feature (see Kaluza et al., 2021, for discussion).

Comment: ll. 222-224: The interpretation of Fig. 5 is not entirely clear at this point of the manuscript. Why do the authors examine GWMF specifically in the vicinity of high-frequency strong shear events rather than showing the overall GWMF? Also, please clarify what is meant by “tropopause-relative,” given that the tropopause height itself varies in the figure.

Reply: We know that upward-propagating GWs having short-horizontal-wavelength can carry substantial vertical momentum fluxes. However, to address the question whether GWs contribute to the formation of the extratropical tropopause shear layer and to CAT hotspots, we examine GWMF in the vicinity of strong, turbulent prone shear, rather than overall vertical distribution in the UTLS. With this analysis, we anticipated that ‘if GW contribute to high shear and turbulent events, then their signal should become evident in regions of high shear and/or turbulence.’ Nonetheless, in Umbarkar et al. (2026) we also observed that the largest AMF magnitudes are concentrated in the upper troposphere and extend into the lower stratosphere.

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 and is now added to the revised version as

L133–135: In a tropopause-relative coordinate system, grid volumes (e.g., satisfying $S^2 \geq S_t^2$) 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: l. 223: For clarity, please consider changing “shorter GWs” to “GWs with shorter horizontal wavelengths.”

Reply: We have changed ‘shorter GWs’ to ‘GWs with shorter horizontal wavelength’ wherever appropriate. The text now reads *GWs with shorter horizontal wavelength which propagate more vertically carry a larger absolute momentum flux.*

Comment: ll. 227-230: There is an inconsistency in the list format: point (i) is a full sentence, while (ii) and (iii) are noun phrases. Additionally, the latter half of point (i) lacks a verb (perhaps the authors meant “could also be related to”?). Regarding the interpretation, enhanced absolute GWMF “indicates” or “shows” intense upward propagation rather than just “hinting” at it. Furthermore, I am not convinced that convection directly contributes to the MF above the tropopause in the manner described, even if MF is shown as an absolute value.

Reply: Thank you for pointing this out. The paragraph has been modified as suggested in the revised manuscript. The text now reads

In particular, the vertical distribution of MF reveals three interesting regions: (i) in the upper troposphere between 5-10 km altitude, enhanced MF indicates intense GW activity with upward propagating waves; (ii) around the tropopause, a first strong vertical gradient in MF is evident; and (iii) in the upper branch of the LMS, a second strong vertical gradient in MF is apparent. These patterns demonstrate that shear hotspots systematically co-locate with regions of strong GW activity, with substantial MF transport both across the tropopause and within the LMS.

Comment: ll. 239-240: A potential reason for the strong MF in shear regions is that GWs may be generated by, or have longer vertical wavelengths near, the jet stream where strong shear exists. This indirect relationship, which is a correlation rather than a causal “role” of GWs, should be explicitly noted.

Reply: We agree that, on larger scales, enhanced GW activity may be indirectly associated with strong shear through GW generation and propagation in the vicinity of the jet. However, the analysis presented here specifically isolates the small-scale ($k \geq 21$) perturbations used as a proxy for resolved GWs. Moreover, we explicitly investigate the role of small-scale GWs in shear generation in Sect. 4.3.2. We have therefore revised the sentence to avoid overstating the results prior to their interpretation.

Comment: l. 255: The term “smaller hotspots” is unclear. Furthermore, attributing them to “specific dynamical events” without further evidence feels speculative.

Reply: Right! We agree and have revised the text by replacing the ambiguous term “smaller hotspots” with “localized regions of enhanced occurrence frequency” and removing the speculative attribution to specific dynamical events. The sentence now reads as

The highest frequencies occur from November to March, while the summer months (June to September) feature rather small hotspots of enhanced potential turbulence occurrence. These hotspots might be related to distinct dynamic features, e.g., convection.

Comment: ll. 263-265: The last sentence does not make lots of sense. The vertical shear term $S2$ in the definition of the Richardson number $Ri \equiv N^2/S^2$ already includes contribution from shear associated with GWs. Therefore, the difference between the

distribution of S^2 and Ri is solely attributable to N^2 . While GWs may contribute to the distribution of N^2 , this difference alone does not provide sufficient evidence that GWs play an important role.

Reply: We thank the reviewer for this clarification. We agree that S^2 includes shear associated with GWs and that differences between the vertical distributions of S^2 and Ri therefore could reflect the contribution of N^2 . Ri , on the other side, directly shows turbulence occurrence and that large-scale flow contributes. This has already been discussed in Kaluza et al. (2021). We further discussed this in section 4.3.1 which reflects both large and small-scale contributions. Section 4.3.2, however, focus on understanding the small-scale shear and GW contribution to turbulence generation in the lowermost stratosphere. We have modified the sentence accordingly and removed the inference that the difference itself provides evidence for GW contribution.

Comment: ll. 288-289: *Could the authors clarify which specific results support the argument made in the sentence starting with “In fact, ... GW activity”?*

Reply: What we meant here is that, 'according to the thermal wind relation, in our analysis, processes like the flow deformation, convergence and differential temperature advection can result in frontal zones with the associated wind shear. As the analysis is performed on the sub-synoptic scale, we argue that these aforementioned processes arises from the small-scale GWs. The corresponding text now simplified to

L269–272: We note that neither Ri nor $TI1$ are expected to follow the distribution of S^2 . Both quantities also rely on other factors and as such on other processes with some of which occurring above and some below the synoptic scales. The $TI1$ which we use is calculated based on the perturbation quantities and as such highlights the contributions of processes on the mesoscale and below. As such, we assume that the potential turbulence occurrences are tightly linked to GW activity.

Comment: ll. 318-319: *The statement “Nevertheless, ...” is a fair acknowledgment, and it highlights the very question I had throughout this section. I recommend that the authors avoid overstating the causal link before this point is reached.*

Reply: Right. We have clarified and revised the entire section 3.3 accordingly.

Comment: ll. 549-558: *The final paragraph of the Summary seems slightly disconnected from the main discussion of the paper.*

Reply: We have modified the text accordingly in the revised manuscript.

References

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