Authors response to egusphere-2025-5142
"Evidence of gravity wave contribution to vertical shear and mixing in the lower stratosphere: a WISE case study"

Umbarkar et al.

We thank the referees and the editor for their comprehensive comments on the manuscript. Minor comments provided by reviewer/editor are addressed in the revised version of the manuscript.
Reviewer comments are given in *italic*, replies in standard and corresponding revised text passages in *blue*.

Response to Reviewer 1

Comment: *The authors have satisfactorily addressed my previous comments, and I do not have further major concerns regarding the scientific content.*
However, the manuscript is very long and, in its current form, is at times difficult to follow due to an overly verbose and occasionally meandering presentation. Key ideas are repeated across sections, and several explanations could be substantially streamlined without loss of scientific content.
I strongly encourage the authors to undertake a thorough revision aimed at improving conciseness. In particular, the manuscript would benefit from:
- Reducing repetition, especially in the Introduction and Results sections. For example, the Introduction provides a very broad overview of gravity waves and related processes, much of which goes beyond what is needed for the present study. I suggest focusing more directly on the specific mechanisms relevant to GW/shear/turbulence interactions in the UTLS, and referring the reader to existing review papers for more general background.
- Shortening lengthy descriptive passages. Key ideas, such as the role of gravity waves in enhancing shear and triggering turbulence, are stated multiple times in the Introduction and reiterated again in the Results. This repetition could be reduced, with each concept introduced once and then referenced more concisely thereafter.
- Improving paragraph structure. Some paragraphs are quite long and contain multiple ideas, which makes the narrative harder to follow. Breaking these into shorter, more focused paragraphs would improve readability and help highlight the main points.
- Reducing overall length. The manuscript could likely be shortened substantially without loss of scientific content by removing redundancy and tightening the language throughout.
Given the availability of modern language-editing tools, achieving a clearer and more concise presentation should be readily feasible and would greatly strengthen the paper.

» Thank you for the suggestions. To enhance the clarity of the manuscript and avoid the repetition, we have revised the wording throughout the manuscript. We hope that these changes address the concerns raised.

Comment: *There are also a few minor typos and points for clarification:*
- L32: 'GW' -> 'GWs'

» Corrected!

Comment: *- L362: 'this wave' -> 'these waves'*

» Corrected!

Comment: *- L362: 'similar to wavelengths of inertia GWs': Is this interpretation appropriate? Earlier the discussion suggests orographic (likely stationary) waves. Also, wavelengths of 100 km are close to the effective resolution limit of ERA5 (x 20 km, implying around 4 grid points per wavelength), so smaller-scale waves would not be well represented.*

» Right. Here we meant

Horizontal wavelength of these waves is on the order of a few hundred kilometers, similar to typical wavelengths of inertia GWs.

Comment: *- L407: 'influenced' -> 'influence'*

» Corrected!

Comment: *- L504-506: 'This indicates that GW characteristics, especially their horizontal details, are strongly dependent on the spatial resolution. Overall, these differences suggest that the nominal resolution largely controls the magnitude of the GW fluctuations in both reanalyses and forecasts.' It would be useful to clarify that this dependence is not only due to grid spacing, but also to the representation of orography. At higher horizontal resolution, the resolved orographic spectrum is better captured, which can lead to the generation of smaller-scale orographic waves.*

» Thanks for pointing it out. We have clarified this in more details and added the following text.

This indicates that GW characteristics, especially their horizontal structure, are strongly dependent on spatial resolution. This dependence arises not only from the changed grid spacing but also to the representation of orography, as higher horizontal resolution better resolves terrain features and can therefore generate smaller-scale orographic GWs. Overall, these results suggest that the horizontal resolution largely controls the magnitude of the GW fluctuations in both reanalyses and forecasts.

Response to Reviewer 2

Comment: *The authors have addressed all the concerns of my initial review of the manuscript. They explain there adaptions clearly in the reply document and, I think, the manuscript has significantly improved in structure and clarity. Still, there are a few comments that I would invite the authors to consider to increase clarity further:*

1. Given that the paper is rather long (35 pages, excluding references and appendices), the abstract remains rather short and also somewhat vague- It would be nice if the statements become somewhat concrete, and thus also reflect the (many) conclusions listed in Section 7.

» Thank you for the suggestion. The abstract has been extended further by adding

In summary, this study extends the findings from idealized experiments as shown in Umbarkar and Kunkel (2025) to the 'real' world and provide physical and chemical evidence of the role of small-scale processes in the dynamics and composition of the tropopause region.

Comment: *2. As a general comment: The structure, e.g., separating observation, ERA5, IFS and ICON, has become much clearer, and the authors have also improved considerably in guiding the reader through the manuscript, by building bridging and introductory statements and paragraphs to connect the different parts. At several places, however, I still had the impression that conciseness could be improved. There are repetitive statements in the text that 'destroy' to some degree the reading flow. I know that this is a rather general comment, but it would be great if the authors looks for such repetitive parts. Some examples will be listed below.*

» We thank the reviewer for this suggestion. We have revised the manuscript accordingly.

Comment: *3. L28: Here it is written that baroclinic waves represent an important source of gravity waves (GW). This is okay, but – in my opinion – also somewhat 'misleading' because of the scale gap between gravity and baroclinic waves. I think it would be better to emphasize that baroclinic waves exhibit finer-scale substructures (upper- and lo-level fronts, convective ascent, jetstreak and flow imbalances,..) that are susceptible for GW excitation.*

» This is an important point, and we thank you for pushing for greater clarity on it. The text has been modified to

In the extratropics, baroclinic life cycles represent an important, albeit less well understood, source of GWs. Baroclinic waves exhibit finer-scale substructures, such as upper- and lower-level fronts, convective ascent, jet streaks and flow imbalances, that are susceptible for GW excitation, primarily through spontaneous imbalance, whereby deviations from balanced flow trigger wave emission (Plougonven and Zhang, 2014; Zhang et al., 2015).

Comment: *4. L77: This paragraph starts with GW and then rather abruptly changes topic to CAT. I would appreciate a somewhat smoother transition of topic.*

» Right. The entire paragraph have been modified accordingly.

Comment: *5. In L80-81 it is written that CAT play a significant role in modulating the large-scale atmospheric circulation. Given the very local scale and transient nature of CAT events, I am not sure whether this statement is not somewhat too strong. I fully agree that turbulence strongly influences the atmospheric flow, but why this focus on CAT (and not on turbulence in general). At least, some references to support the statement would be necessary.*

» Thank your for this comment. Our intention was to emphasize that the CAT-associated turbulent mixing can influence tracer distributions and chemical composition in the UTLS through its cumulative effects. We have therefore revised the text and added appropriate references.

The identification of this clear air turbulence (CAT) is crucial for safety aspects in commercial aviation as well as for mixing of trace species and as such the chemical composition in the UTLS (Chau et al., 2025, 2026).

Comment: *6. L91: "More so, it is important to analyse the sources of this turbulence and GW and shear are important larger scale phenomenon for this" -> please correct sentence and rephrase in clearer way.*

» Rephrased to

More so, it is important to analyse the sources of this turbulence, as GW and shear are important factors that can promote its development.

Comment: *7. At the end of the introduction, and in agreement with the authors' reply, the study strongly links to Umbarkar and Kunkel (2025). I wonder whether this link could also already be mentioned in the abstract, i.e., by explicitly stating there that the study extends the findings from idealized experiments to the 'real' world.*

» Thanks for this suggestion. We have now mentioned Umbarkar and Kunkel (2025) in the abstract.

Comment: *8. L163: What is the unit of the wavenumber? And to which (approximate) wavelength does wavenumber 20 correspond?*

» It is the number of wave cycles around circumference at a given latitude. So the wavenumber 20 corresponds to $\frac{20}{(2\pi R_E \cos\phi)}$, i.e., wavelength of $\sim$ 1000 km in the midlatitudes.

Comment: *9 L186: Maybe, explain already here in 1-2 sentences how dynamic and kinematic turbulence generation differ? Or what exactly this means?*

» The following line added to the manuscript.

Here, dynamical diagnostics characterize the stratification of the flow and its susceptibility to instabilities, whereas kinematic diagnostics describe flow deformation and shear that can promote turbulence development.

Comment: *10. L298: "Overall, the w and spectra follows the slope close to k = 5/3, can be related to three dimensional isotropic turbulence." -> Please correct and clarify sentence.*

» Rephrased to

Overall, the w and Θ spectra follow the slope close to $k = -5/3$ and can be related to three dimensional isotropic turbulence, which is an indication of dynamic processes on or below the mesoscale affecting the flow.

Comment: *11. L313-314: "Although, hodographs can be drawn from vertical profiles at a time, given that limited altitude level data, in our analysis a single hodograph is drawn using profiles at multiple times at one flight altitude (e.g., as in Gomes et al., 2025" -> I think that I get the meaning, but rephrase in clearer way.*

» Modified to

These hodographs are typically drawn from vertical profiles at a given time. However, due to the limited number of altitude levels available in our dataset, we instead construct a single hodograph using profiles at multiple times at one flight altitude (e.g., as in Gomes et al., 2025).

Comment: *12. L323-324: This looks rather repetitive to what is written in L313-314. Please try to be more concise and avoid repetitive statements.*

» The entire paragraph have been modified to avoid repetitions.

Comment: *13. L360: Here, some wave-induced features are mentioned, but it remains rather vague what these features are. Please be more precise.*

» Right. We meant 'associated wave-induced features such as fluctuations in PV field at later times'. The sentence has been modified to

These signatures of GWs are persistent over the following hours around Iceland (Fig. 6b, c) and are also evident in the PV field.

Comment: *14. L364: As a further source of GW, imbalances of the synoptic flow is mentioned. I agree that flow imbalances could play an important role in triggering CAT, but the statement at this places remains rather vague. Is there a way how to make the statement stronger? Quantification of flow imbalance by some metrics? References?*

» Thank you for pointing this out. Since the observed GWs (based on divergence field) are primarily attributed to flow over Icelandic topography, we have removed the statement. The flow imbalance is discussed next in the shear interpretation.

Comment: *15. L380-381: " Horizontally propagating GWs with large horizontal wavelengths tend to dominate the horizontal derivatives of momentum fluxes, whereas, upward-propagating large-amplitude GWs (i.e., with large vertical wavelength)*

carry large momentum, which then contribute to the vertical derivatives of the wind components across the tropopause" -> This sounds as if large-amplitude GW are at the same time the ones with large vertical wavelength. Is this true? Further, essentially the statement is that GW can enhance vertical wind shear across the tropopause, right? If so, the sentence could be rephrased in easier way?!

» The text has been modified as

Horizontally propagating GWs with large horizontal wavelengths tend to dominate the horizontal derivatives of momentum fluxes, whereas, upward-propagating large-amplitude GWs can carry substantial momentum vertically across the tropopause. This further modulates the vertical gradients of the wind components across the tropopause, which in turn, leads to higher shear values and with this turbulence occurrence in this region.

Comment: *16. L400-404: Here, first it is stated at the end of the paragraph that GW and vertical wind shear co-occur; the next paragraph then starts with the question whether GW are of relevance for shear generation across the tropopause. To me, this is somewhat confusing. It would be good to rephrase in clearer way.*

» Rephrased as

While the co-occurrence of GWs and enhanced vertical wind shear suggests a possible link, the extent to which small-scale GWs contribute to shear modification across the tropopause remains to be assessed.

Comment: *17. L441: "One previously suggested impact from dynamic instability is that any associated inertia GWs could lead to CAT when these waves breaks (Knox, 1997)." -> Rephrase in clearer way!*

» Rephrased to

One previously suggested consequence of dynamic instability is that associated inertia GWs may generate CAT when they break (Knox, 1997).

Comment: *18. Given that the manuscript is rather long, I wonder whether Section 6 about the zonal framework is really needed.*

» Thank you for your comment. The idea was to show the co-occurrence of absolute momentum flux with turbulence-prone regions identified by **total** shear, and CAT indices. The zonal occurrence frequency distribution of AMF with respect to these diagnostics therefore captures the GWs momentum flux deposition during such turbulent episodes. We hope the reviewer agrees to keep this section in the manuscript.

Response to Editor

Comment: *All referees agree that the paper is improved and close to being suitable for publication. Each has provided further comments. Please can you consider these carefully and provide a revised version of the paper together with responses to the referees. In particular you will see comments on the length and clarity of the paper – I encourage you to make an effort to address these comments to make it easier for readers to appreciate your results and conclusions.*

» As detailed above, we have carefully addressed all reviewer comments and revised the manuscript accordingly, including improvements to the clarity and presentation of the results and conclusions. We hope that the revised manuscript satisfactorily addresses all the points raised.

References

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