

Authors response to egosphere-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 for their comprehensive comments on the manuscript. We have carefully addressed all points 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 revised the title from "Evidence of gravity wave contribution to vertical shear and mixing in the lower stratosphere: a WISE case study" to "Evidence of gravity wave contribution to vertical shear and mixing in the lower stratosphere".
- 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) can be confirmed in forecast and reanalysis data. A dedicated focus is on the relation between shear and turbulence occurrences in regions of gravity wave activity in the region of a baroclinic wave. We decided to use a case study of a baroclinic wave over the North Atlantic. This case study has several benefits: (i) observations exist which show the existence of a gravity wave related to the baroclinic wave and mixing occurring in the vicinity of the gravity wave; (ii) a comprehensive model forecast dedicated for this case with ICON. In addition to ERA5 reanalysis data and IFS forecast data, the latter allows to study this case also in a comprehensive ICON model forecast which has been dedicated for this case study. It also allows us to uniquely study the relation between gravity waves, shear and turbulence in various model resolutions. We thus have a suite of comprehensive models to test whether our predictions from the idealized world hold true in the "real" world (real in terms of comprehensive models and during a baroclinic life cycle over the North Atlantic).
- Following the suggestions by two referees to enhance the clarity of the manuscript and to separate the analysis between ECMWF and ICON products, we changed the flow of result sections. We start with the observations and present the synoptic situation based on ERA5. We then introduced the gravity wave, turbulence and shear diagnostics first for ERA5. After that we put these into perspective, once compared to IFS data, and then for the ICON simulations. We thus gain insights explicitly how ERA5 sees the situation, then a comparison for an ECMWF product with finer horizontal grid

resolution. Finally, the ICON simulations, despite their different lead time, provide a unique insight into the situation, since we have the same situation in three model domains (with and without convective parameterization) and also in relation to our previous results from idealized ICON model simulations. Comparing ICON and ECMWF also shows whether there are general differences between a model with a non-hydrostatic and a model with a hydrostatic dynamical core and with varying parameterizations.

– **Add-ons:**

- (i) Discussion based on source mechanism of observed gravity waves is now included in the section 4.
- (ii) A brief introduction of turbulence/CAT indices is provided in part of introduction section.
- (iii) We also discuss the applicability and limitations of hybrid approach used to compute GW momentum fluxes and turbulence diagnostics.

Response to Reviewer 2

Major comments:

1. Clarity:

Comment: L139-142: *we restrict ourself here to the presentation of gravity wave vertical fluxes of horizontal momentum in terms of distribution of probability density functions (PDFs). The vertical transport due to small-scale processes in the ExUTLS is investigated via absolute momentum flux (AMF) -> this is a preamble to the following paragraphs, but in terms of distribution of probability density functions (PDFs) remains rather unclear.*

» we meant "in terms of occurrences." The text has been modified to

In order to understand momentum transport due to small scales and occurrence of GWs, we present the gravity wave vertical fluxes of horizontal momentum in form of probability density functions (PDFs).

Comment: L260-261: *All of these findings are potential indications that these air masses have been subject to a process which breaks the relation between CO, N₂O, and Θ; one potential process which can lead these correlations is mixing -> Referring to the relation between the three fields is unclear to me; this relationship should be much more clearly introduced in the preceding text.*

» This relation between CO, N₂O, and Θ has been stated on line 249-251 of the original manuscript. For clarity, we have now included the following statement.

Note that irreversible mixing can be followed from the interrelation of CO, N₂O and potential temperature. Due to the chemical life times of the tracers we can infer that N₂O is almost constant in the troposphere with a sink in the upper stratosphere and thus a minor negative gradient with altitude. The same is true for CO, with the difference that the mixing ratios of CO are

less constant in the troposphere and the gradient in the stratosphere is stronger due to stronger chemical sink. However, their tracer-tracer relation is positive. While both tracers decrease with altitude, potential temperature increases, which in turn gives specific background relation of these three species. A deviation from the stated background condition is then only possible in the presence of non-conservative processes which also leads to mixing. This is the case here, since we see ...

Comment: L269-271: *Particularly, the small transition between slopes observed in N₂O spectra around 10–2 Hz (green and blue lines), where the slopes of w indicates turbulent energy source, and here, the N₂O hints the turbulent behavior of small scales, e.g., those related to GWs, might be substantial to explain the dynamics in the lower stratosphere. -> What small transitions? How does the N₂O hints to the turbulent behavior of the small scales? Why do the slopes of w indicate a turbulent energy source? This is all difficult to understand. Please rephrase in easier way, and add some explanations.*

» We have clarified this in more details and added the following text.

The w spectra shows a slope of -3 for the smaller wavelengths (i.e., for frequency <0.8 Hz), in agreement with Lachnitt et al. (2023). For higher frequencies (shorter wavelengths), the increase of power spectra to 0.5 Hz could be indication of turbulent processes below the synoptic scale. Overall, the w and Θ spectra follows the slope close to $k = -5/3$, can be related to three dimensional isotropic turbulence. This is an indication of dynamic processes on or below the mesoscale affecting the flow. Moreover, N₂O and CO also have a slope close to -5/3 for intermediate frequencies i.e., for smaller wavelengths. From 10⁻² Hz onward to larger scales, the N₂O spectra follow a -5/3 slope which suggests that small-scale processes, potentially those related to GWs, contribute to turbulent mixing in the lower stratosphere.

Comment: L289-290: *Due to distinct sampling frequencies and noise characteristics, separate filtering methods were applied to flight and model datasets to optimally isolate GW related perturbations -> This remains rather vague? Please be more specific.*

» Modified to

Note that, due to distinct sampling frequency and noise characteristics in the measurements, the flight data is filtered using a 4th order Butterworth bandpass filter (Butterworth, 1930), while the model data is processed using the Helmholtz decomposition approach described in Sect. 2.5, in order to isolate GW related perturbations.

Comment: L336-337: *Moreover, the large amplitude wave signatures pointing towards the strong upward motions across the tropopause, that could influence the vertical shear, and in turn turbulence generation in this region -> Please rephrase in clearer way. Do you, essentially, want to say that GW of large amplitude are discernible at and across the tropopause and that they locally enlarge vertical wind shear, which in turn increases the probability of turbulence?*

» We meant

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. This, in turn, leads to higher shear values and lower Richardson numbers, that increases the probability of turbulence.

Comment: L370-372: *In continuation to turbulence analysis, this section addresses the occurrence of dynamic instability associated with GWs, particularly in the LMS, as well as the relation between the occurrences of GWs and shear perturbations in terms of 2D histograms. -> Mentioning in terms of 2D histograms confuses more than it helps in this preamble to the section.*

» Removed in the revised text.

Comment: L396-400: *Here, suddenly inertial instability and their association to GW is introduced. So far, inertial instability was not a key aspect of the study, and I was astonished them to appear here out of the blue. Possibly, this link between GW and inertial instability is important, but then I would expect it to be introduced and discussed already in the introduction.*

» Right. Thanks for pointing it out. Here, we refer more generally to dynamical instability. A detailed investigation of inertial instability is rather beyond the scope of this study. We replaced it with "dynamical instability" in the revised text.

Comment: L433-434: *which further induce strong potential instability and ultimately result in the development of higher risk CAT -> What do you exactly mean with potential instability? And what is higher risk CAT? Please rephrase in clearer way.*

» the word "potential" throughout the manuscript is referred to "potential for turbulence occurrence or the potential for dynamic instability to occur" as $Ri_c \leq 1$ is considered. From these we can only infer that the background conditions are conducive for turbulence. this is revised in the manuscript (L199). We erased "higher risk". This was placed incorrectly in this sentence.

Comment: L444-445: *One plausible explanation is that local flow deformation in ICON exhibit enhanced values during turbulent events in the LMS, partly compensating for weaker resolved shear, thereby causing higher abundance of TII in the vicinity of GWs. -> difficult to understand!*

» This part is fully modified in the revised manuscript. See central changes.

A possible explanation is that, in ICON, enhanced local flow deformation during turbulent events in the LMS partly compensates for weaker resolved shear, but leading to a higher occurrence of TII in regions influenced by GWs.

Comment: L448-451: *Overall, following the positive relation between small-scale shear and AMF in the LMS above the North Atlantic as discussed in Sect. 4.3, we can go even one step further. The relationship observed between the turbulence indices, TII and TI2, and GWs, leads us to conclude that GW play a role for the occurrence of CAT in our analysis region, as evidenced by the positive correlation between turbulence diagnostics and the AMF -> The statement starts with positive relation between*

small-scale shear and AMF, and it ends with a positive correlation between turbulence diagnostics and AMF. This is confusing, at least to me, and I struggle to get the key message from the statement.

» Modified to

Overall, following the positive relation between small-scale shear and AMF in the LMS above the North Atlantic (Sect. 4.3), we can go even one step further. The co-occurrence of enhanced AMF with both increased shear and elevated turbulence indices, T11 and T12, leads us to the conclusion that GW play a role for the occurrence of CAT in our analysis region.

Comment: L468-479: *Switching back and forth between T11 and T12 is quite confusing, and at the end hindered me to get the key message of the paragraph. For example, first is mentioned that T11 and T12 exhibit identical patterns, but a few lines below it is with wider spread out in T12...*

» The entire section 4.5 has been modified in the revised manuscript. See central changes for more details.

Comment: L510-511: *We note that this is key for the potential occurrence of clear air turbulence, as highly transient yet frequent mixing processes in the extratropical lowermost stratosphere -> Is this sentence complete?*

» Modified to

We note that this is key for the potential occurrence of clear air turbulence, which manifests as highly transient yet frequent mixing processes in the extratropical lowermost stratosphere.

2. ERA5/IFS/ICON:

Comment: *Three models are included in the analysis, with different spatial and temporal resolutions, but also differing in the way how turbulence is handled. I am not completely convinced that keeping all three models in the study helps the storyline, or whether it is more confusing. Some points to consider: (i) The difference in the models is at several places in the manuscript highlighted, but it should more systematically be discussed in the methodology section; (ii) Why is only ERA5 discussed in section 3.2, and the other, higher resolved IFS and ICON are not? (iii) At some places it is unclear whether the study is about a model intercomparison, or whether the physical processes are the focus.*

Some of the conclusions remains somewhat vague. As an example, it is written in L387-389: Note here that the ERA5 and IFS shows quite similar relation when looked at the occurrences at the range of 103 to 102 (see the green colors), while the difference in ICON likely arise mainly from the occurrence of low Ri values (see Sect. 4.2). Nonetheless, ERA5 shows fewer occurrences of maximum shear than IFS. This could be due to its coarser horizontal resolution, which might lead to the limited representation of (partially resolved) GW spectrum in the UTLS. -> This statement seems plausible, but remains also speculative

I think it is worthwhile to study the case in different models, however, I would appreciate if the key process analysis is based

on one model, and that all the sensitivity (model dependence) is handles in a separate section that is only dedicated to this model comparison. In this way, process study could more clearly be separated from intercomparison, and so the storyline would become easier to follow for the reader.

Finally, note also that some of the differences in GW/turbulence between ERA5, IFS and ICON might originate from differences in the large-scale evolution of the models. In particular, the evolution of ICON (in Figure 1 but also in Figure 7 & 9) seems to be somewhat different compared to ERA5 and IFS. This aspect should be discussed when comparing the GW and the turbulence indicators in the three models.

» Thank you for the suggestions. This is also pointed out by another referee. We have revised the manuscript accordingly by presenting the analysis of the key processes based on ERA5, and all the sensitivity (model dependence) handled in a separate section. In this context, we separated the discussion of ECMWF products and ICON simulations throughout the manuscript. We hope that this modifications addresses the issues raised. Please see central changes (and revised manuscript) for more details.

3. GW source and evolution:

Comment: GW is the key topic of the study. Figure 6 nicely shows that a GW indeed is present and propagates from the mid-tropospheric levels to the tropopause, and from there further propagates with a different angle into the stratosphere. Given that this GW is key for the study, it would be good to know what the origin of the waves actually is. Since the WISE flight is over Greenland, I assume that the GW source are topographic flows?! In short, I would suggest to more clearly investigate the source of the wave. Furthermore, it would also be interesting to see, based on the models, how spatially and temporally persistent the wave is. What is its lifetime? Does it change amplitude, propagation angle over time?

Additionally, the link between GW and vertical wind shear is highlighted several times in the manuscript. I fully agree that GW locally modify the background flow, and so can lead to enhanced vertical wind shear, with possible triggering of Kelvin-Helmholtz instability. However, vertical wind shear is only one part in the Richardson number, the other one being vertical stratification, which has a stabilizing effect on the flow. Hence, I wonder whether the modification of the stratification by GW activity must also be considered. So, why not more systematically consider Richardson number (with wind shear and stratification included), instead of only wind shear.

Next, I do not clearly understand how the absolute momentum flux (AMF) is used in the line of arguments. I see that it is a metric to quantify the momentum transport due to gravity waves. What its specific role in the discussion remains somewhat unclear. I see that there is a gravity wave associated to the WISE flight, that it can be characterized with respect to the propagation direction (as in Figure 5), and that the gravity waves leads to regions of enhanced vertical wind shear, which in turn lead to turbulence due to KHI. But why is the AMF really needed in the overall storyline? It is stated that in L390-391: Overall, these results pinpoints the crucial role of GW-induced shear to the potential turbulence occurrence, and subsequently representing the important role of GWs to transporting the momentum across the tropopause. But why this is important for the storyline,

remains unclear to me. In this sense, I also do not clearly understand what we can learn from the AMF-S2 plots in figure 8 and, later, in figure 11 from the AMF-TI1/2 plots. Especially, in figure 11 the distributions of the two turbulence indicators (TI1 and TI2) look very similar to me.

» We thank the reviewer for this suggestion. To address the first part of comment, we initially show the evolution of GW fields and the key processes using ERA5 data in the revised manuscript. Regarding the origin of GWs, this issue was also raised by other referee. Upon detailed investigation, we attribute the source of GWs to be flow over orography. These GWs further altered in shape at the tropopause due to the background conditions. We extended Fig. 6 to the surface and included following text in the manuscript.

The GW field is primary noticeable over the Icelandic highlands. The flow associated with the baroclinic system over the topography of Iceland leads to the emission of the GWs. Its signal is already evident in the troposphere and changes its appearance in term of wavelength over the tropopause. This change is expected due to changes in stability across the tropopause. The flow over orography appears to be source mechanism for the generation of GWs, particularly given that the GW signal occurs near the transition between ocean and Icelandic topography.

Moreover, there is presence of upward propagating wave signatures with short horizontal wavelength that are excited in the upper troposphere above the location of troposphere jet. This indicates that the GWs that are originated from surface orography further emitted away from their source region and propagate upward toward the lowermost stratosphere.

» Regarding the second part of comment, we agree that the modification of vertical stratification by GWs is possible. The role of GWs in modification of stratification has been discussed in previous studies (e.g., Kunkel et al., 2014; Kaluza et al., 2019, 2021). Here, our intention is to clearly address the role of GWs to generation of enhanced shear (which further result in the formation of tropopause shear layer). We also note that analysis of Richardson number with both wind shear and stratification is already included in section 4.2 and 4.3 (Fig. 7 and Fig. 8). The analysis of potential turbulence provided in order to understand how the GWs and GW-induced shear results in turbulence occurrence. Following the reviewer's suggestion, we now included a brief discussion on this point in relevant section.

» Concerning the third part of your comment, the use of absolute momentum flux is a pragmatic choice for a simple reason that *one would expect momentum-flux deposition by GWs in the region where GW-induced instability and turbulence occur (Fritts and Alexander, 2003)*. We now included this line in the relevant section.

Figure 8 (AMF-S² pair plot) and, figure 11 (AMF-TI1/2 pair plots) are implied to represent the relation between GWs and shear perturbations during the potential turbulent event. The examination of AMF-S2 pairs simply show that large AMF values, and hence GWs, correlate with large shear perturbation values. This tells that GW induced shear is an important driver that lead to turbulence generation in the subsequent region. Next, AMF-TI1/2 pairs show the relation between GWs and CAT indices, where there is role of GWs to the development of CAT. It turns out that, GWs on smaller scale that induces shear can play as a key driver of the onset of CAT locally. We believe that this is an important supportive implication by not only showing

the dynamical and physical evidence of co-location of GWs, shear and turbulence, but also provide statistical evidence for the formation and **co-occurrences** of the shear and potential turbulence that arises in association with GWs.

4. T11 vs T12 & introduction:

Comment: *The turbulence indicators T11 and T12 are compared at several places. In L191-194 it is written: The inclusion of CVG in T12 offers an advantage over T11 by capturing the small-scale flow features such as those associated with GWs or upper level frontal structures. These region often exhibit enhanced shear and convergence making T12 more sensitive to turbulence driven by mixing processes. Is there a reference that supports this statement about T11 and T12? I am not completely convinced that the difference between T11 and T12 should easily be used to separate between more GW-related turbulence and less GW-related. At least, I would like to see this aspects to be discussed in the introduction.*

This may be lead to a deficiency of the introduction. Turbulence indicators (T11 and T12), vertical wind shear, absolute momentum fluxes (AMF) could also be discussed in the introduction. So, for example, in L176 it is written: While Ri captures dynamic instability, it may not fully represent turbulence under complex flow conditions. To account this limitation, we also employ the Ellord-Knapp turbulence indices, T11 and T12, which are empirical diagnostics specifically developed to identify CAT. Hence, since all these indicators are extensively used in the manuscript, a careful introduction to them from the beginning would be appropriate.

» This is an important point, and we thank you for pushing for greater clarity on it. In our analysis, the inclusion of the convergence (CVG) term in T12 provides additional sensitivity to local flow deformation and convergence associated with small-scale perturbations. Since turbulence diagnostics are computed from **small-scale perturbation** fields, we expect T12 to be more responsive to features such as GW-induced convergence, localized deformation and shear. However, we acknowledge that this does not uniquely isolate GW-related turbulence, and other processes (e.g., frontal dynamics) may also contribute. We have therefore revised the text to clarify this interpretation and included a brief discussion in the introduction. This now reads:

In addition to vertical shear-based diagnostics (e.g. gradient Richardson number), empirical turbulence indices such as T11 and T12 are widely used in CAT identification (Ellrod and Knapp, 1992; Sharman et al., 2006; Jaeger and Sprenger, 2007). T11 is a semi-empirical diagnostic; although it neglects the influence of the stratification, it has been shown to perform well within the hierarchy of turbulence predictors (Sharman and Pearson, 2017). Both Ellord indices combine vertical wind shear and horizontal deformation, while T12 additionally includes a convergence term, enhancing its sensitivity to localized flow features associated with small-scale perturbations.

5. Relevance of synoptic situation / target region:

Comment: *The study first uses the WISE flight data to identify turbulence segments along the flight track and then to see whether GW are associated (or leading) to this turbulence. The discussion of the WISE measurements is mostly confined to sections 3.2 and 3.3, whereas the later sections use the NWP data to get a more comprehensive picture of the GW activity along the WISE flight. In this sense, the vertical cross-sections in Figure 6 are very helpful, and also the figures 7 and 9 are important*

to get a more comprehensive figure. I wonder, however, whether all figures should focus (zoom in) more to the region explicitly around the WISE flight. At the moment, part of the discussion deals with features (e.g., tropopause fold) that are farther away from the WISE flight path. Possibly, the study gets some more focus if it restricted more strongly at and around the flight.

To me, it is also not so clear why the turbulence/GW analysis is performed within a baroclinic system. How relevant is it really for the analysis that the flight and GW encounter happened within a ridge (baroclinic system). Maybe, the motivation is that these regions are often sources of GW? This is hinted to in the introduction: (L30-33): *In the extratropics, baroclinic waves represent an important, albeit less well understood source of GWs. Surface fronts and upper level jet streams associated with baroclinic wave development generate GWs, primarily through spontaneous imbalance, i.e., deviations from balanced flow trigger wave emission (Plougonven and Zhang, 2014; Zhang et al., 2015b). Regions of baroclinic instability, particularly along jet streaks and frontal zones, are hotspots for non-orographic GW activity. If so, I would expect that the origin of the GWs in this study are more clearly related to these baroclinic processes? For example, the origin of the GW remains rather unclear. It seems to originate from lower-to-mid tropospheric levels, i.e., possibly of orographic origin and not directly related to a baroclinic wave.*

» As mentioned in central changes, the primary aim of this study is to determine whether the findings from the idealized cases (Umbarkar and Kunkel, 2025) are also evident under more realistic conditions. Note again that the WISE flight took place in a baroclinic system which is our system of reference. In this context, the flight data is used to confirm the presence of GWs and to identify GW-induced mixing over the Iceland. Meanwhile the broader model analysis allows us to investigate their spatial extent, evolution and their influence on shear and potential turbulence occurrence. Therefore, we intentionally retain a larger spatial extent in the figures to capture the full dynamical environment (e.g., tropopause structures) in which these processes occur.

» Regarding second part of the comment, please see our response to comment 3. This information is added to the manuscript. Note that we are not only interested in the GW itself but also in the larger scale structure around the GWs.

Specific comments:

Comment: L11: Further analysis of turbulence diagnostics suggests -> Please be more specific!

» Modified to

Further analysis of turbulence diagnostics including Richardson number on large-scale and turbulence indices (TI1, TI2) on small-scale suggests that GWs produce shear which leads to the occurrence of clear air turbulence.

Comment: L85-87: This sentence tries to summarize in one statement the goal of the study, which leads to a complicated sentence structure. Please rephrase in easier way.

» Modified!

Our primary goal of this study is based on recent revelations from idealized simulations of baroclinic life cycles (Umbarkar and Kunkel, 2025). In these simulations, a clear relation between GW occurrence, enhanced shear and turbulence occurrence in the lower stratosphere was found. The question is now whether this relation is also evident within an extratropical life cycle over the North Atlantic.

Comment: L132: A warm-conveyor belt is mentioned here. Okay, but the WCB is mentioned for the first time and comes here somewhat out of the blue.

» Warm-conveyor belt is not important here, hence removed.

The simulation was initialized with the operational ICON global analysis at 00:00 UTC on 20 September 2017, shortly before the cyclone developed, and ran for 96 hours, ending at 00:00 UTC on 24 September 2017, when the life cycle of cyclone was in its final stage (Schwenk and Miltenberger, 2024).

Comment: L143: Note that,.... scale GW -> Incomplete sentence?

» Rephrased as:

Note that the momentum flux at sub-synoptic scales is used here as a first proxy for the momentum flux due to small-scale GWs.

Comment: In Figure 4a, the w spectra flatten out towards low frequencies, while this is not the case for potential temperature and the two tracers. Why does w behave differently compared to potential temperature?

» Please see our response to comment 1 part 3.

Comment: L342: Minor detail, but you are referring to upper and lower panels *before* referring to the figure

» Corrected!

Comment: L358: Why in principle?

» Removed!

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