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 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 exists 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 1

Major comments:

1. Introduction / representation of turbulence:

Comment: *It should be stated more clearly that Kelvin-Helmholtz instability and turbulence are not explicitly resolved in ICON, IFS or ERA5 even if the gravity waves themselves are; instead, these processes are represented through subgrid-scale parameterizations. It would therefore be useful to analyse the contributions from these subgrid-scale processes explicitly. This distinction should be discussed, and where possible the relevant tendencies should be diagnosed. For example, in ERA5 these can be obtained from MARS using short forecasts on model levels, with fields such as the time-mean eastward and northward wind tendencies due to parameterizations.*

» Thank you for pointing this out. The discussion on representation of turbulence is now added to section 2.5. In this study, our analysis focuses on large-scale diagnostics of turbulence (Ri, shear) to assess the dynamical conditions conducive to turbulence and its relation to gravity wave activity. We state now more clearly that Kelvin-Helmholtz instability and turbulence are subgrid scale processes in the models used in this study and are only indirectly diagnosed via the large-scale diagnostics. We appreciate the suggestion to analyse additional parameters (tendencies) from the ERA5 forecast. We will keep this suggestion in mind for a future analysis. However, for this study we decided to discuss the turbulence diagnostics based on the resolved wind field to be able to determine the differences between the models, since the tendencies are neither available for the IFS, nor for ICON. We include in the manuscript:

It should be noted that Kelvin-Helmholtz instability and turbulence are not explicitly resolved in the used reanalyses as well as forecast even if the gravity waves themselves are. Instead, these processes are represented through subgrid-scale parameterizations. Our analysis is based on diagnostics of the resolved fields in ERA5, IFS and ICON. These diagnostics highlight *regions which have conditions favourable for the occurrence of turbulence. We therefore regard these regions as regions of potential turbulence occurrence based on the resolved fields of the respective model.*

2. Origin of the gravity waves:

Comment: *The source of the turbulence-inducing GWs is unclear from the analysis and discussion. Do the authors attribute these waves primarily to baroclinic instability, or to flow over orography? From the figures, flow over orography appears at least as plausible as baroclinic instability, particularly given that the GW signal occurs near the transition between ocean and Icelandic topography. The presence of a front does not necessarily imply GW generation via baroclinic instability; instead, the front may simply provide favourable background wind conditions allowing surface-generated GWs to propagate into the UTLS. It would therefore be useful to extend Fig. 6df down to the surface and to explicitly mark the underlying topography.*

» We agree. We extended Fig. 6 in the original manuscript to show the entire vertical range down to the surface. From this it seems that the flow associated with the baroclinic wave over the topography of Iceland is the source of the GW. This information is added to the manuscript, but we note that the source of the GW is not the primary focus of our analysis. The modified figure is shown below.

The gravity wave 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 gravity wave. 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.

3. Comparison of ICON with IFS and ERA5:

Comment: *The ICON forecasts analysed here are at lead times of 3-4 days, whereas the IFS forecasts are at lead times of 0-1 day and therefore much closer to the analysis/ERA5. This difference should be explicitly acknowledged when comparing the two models. A major reason for the discrepancies between ICON and IFS is likely the different background flows, as ICON will have drifted further from the analysis. This is already evident in the large-scale fields shown in Figs. 1, 6, 7, 9, 12, and 13, and the discussion of these figures should reflect this. For a more direct one-to-one comparison, the two models should ideally be compared at the same forecast lead time.*

» We thank the reviewer for noticing this important point. We have now explicitly clarified the difference in forecast lead times between ERA5/IFS and ICON and revised the manuscript accordingly, separating the analysis of ECMWF products and ICON output. These changes are detailed in central changes.

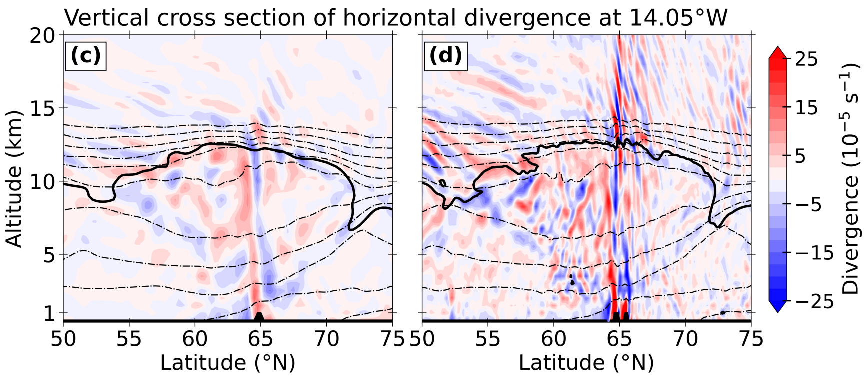

Figure 1. Vertical cross sections of horizontal divergence at 14.05° W is for c) ERA5 and c) IFS datasets. The solid black line represent the 3.5 PVU as a dynamical tropopause whereas dashed lines in lower panel represent the potential temperature starting from 280 K (bottom) to 380 K (top) with 10 K increments. (Figure 6 (lower panel) in original manuscript).

Comment: *In Sect. 2.4, please also discuss the vertical resolution of ICON in the analysed region, as this is important for comparing vertical wavelengths with IFS (L331-332).*

» We now added vertical resolution of ICON in the analysed region in the manuscript section 2.4.

In the vertical the ICON simulations have 90 levels from the surface up to 23 km altitude with a vertical grid spacing of roughly 250-300 m in the UTLS.

Comment: *Regarding Fig. 6, it is notable that ICON and IFS show very similar GW amplitudes, even though ICONs horizontal resolution in this region is about three times finer than that of IFS (3.3 km versus 9 km). One might expect ICON to produce larger-amplitude resolved GWs, consistent with the statement in L323 that nominal resolution controls resolved GW amplitude. This does not appear to be the case in Fig. 6. This could again be related to differences in the background flow. However, later in Figs. 8 and 11, ICON does show larger GW momentum fluxes than IFS, as expected given its higher resolution. Please comment on this apparent inconsistency. I am also somewhat confused by the ICON figures in the supplement and the discussion on L409-410: It was my understanding that you are analysing the nested simulations for ICON throughout the manuscript with the region of interest having 3km horizontal resolution. Is this not the case? If not you need to clarify section 2.4.*

» The reviewer raises an important point regarding the comparison between ICON and IFS GW amplitudes. We note that the ICON results analysed (in the original manuscript) correspond to the ∼13 km horizontal resolution simulation, rather than the ∼3 km horizontal resolution simulation. Because the analysed ∼13 km ICON simulation has a horizontal resolution closer to that of IFS, the similar GW amplitudes shown in Fig. 6 were therefore consistent. This may have caused confusion in the original manuscript. **We have now included analysis of all three domains of ICON simulation in the revised manuscript.** For more details, see response in central changes.

4. Scale separation in the UTLS (L159-161):

Comment: *The authors state that their scale-separation approach follows commonly used GW separation methods and cite several previous studies. While this is indeed true in the stratosphere, where there is a clear scale separation between planetary waves and gravity waves (and where studies such as Gupta et al. and Stephan et al. apply these methods), the situation is less clear in the UTLS. In this region, the separation between GWs and other mesoscale structures is more ambiguous. Please comment on the applicability and limitations of this approach in the UTLS.*

» We assume the reviewer is referring to the wavenumber-based scale separation, which is consistent with Gupta et al. (2024); Stephan et al. (2019). We agree that the separation between GWs and other mesoscale structures was noted to be ambiguous in the UTLS. However, Wei et al. (2022) suggested that the using divergent wind as a proxy provides more reliable estimates of GW momentum flux, particularly in the upper troposphere. Also, recent work by Zhang et al. (2025) has evaluated similar scale-separation approaches at 200 hPa, i.e., at comparable altitudes to those considered here. Moreover, our analysis primarily focuses on the lowermost stratosphere where scale separation is comparatively expected to be more robust. We have clarified these applicability and limitations in the revised manuscript.

Recent study by Zhang et al. (2025) suggest that such hybrid scale-separation approaches can be applied at lowermost stratosphere levels (e.g., around 200 hPa). Moreover, Wei et al. (2022); Zhang et al. (2025) further suggest that the use of divergent wind as a proxy for separating GWs provides more reliable estimates of GW momentum flux, particularly in the upper troposphere. In this study, the focus is primarily on the lowermost stratosphere, where the scale separation is comparatively more robust, and the limitations of the method are therefore reduced.

5. Figure 1 (PV field):

Comment: *Are regions of negative potential vorticity present, as suggested by the colour-bar range? If so, could these be highlighted using a distinct colour rather than shades of blue? Negative PV is dynamically significant and would indicate regions of instability.*

» Yes, regions of negative potential vorticity are now highlighted in magenta color, e.g. in figure 2. Also negative PV values are often related to cloud processes.

6. Wavelength estimates (L302-303):

Comment: *Please state the estimated vertical and horizontal wavelengths explicitly here, so that they can be clearly compared with model-resolved scales later in the paper.*

» In this study, hodographs are drawn from the flight data using profiles at multiple times at one flight altitude, 12.5 km. The limited altitude region here means that only one or two vertical wavelengths are sampled. Under the assumption of quasi-stationary wave propagation, this approach can provide indirect information on wave characteristics in horizontal, yet the estimation of vertical wavelength adds some uncertainty to the analysis.

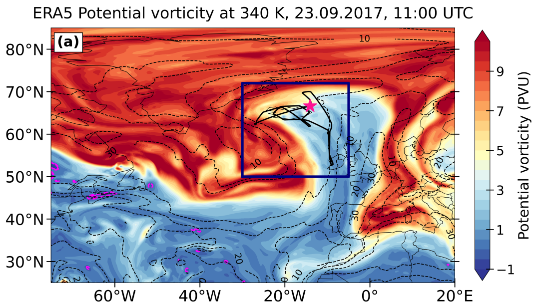

Figure 2. Potential vorticity distribution (in PVU) at the 340 K isentropic surface on 23.09.2017 at 11:00 UTC for ERA5 reanalysis. The dashed lines show the horizontal wind (in m s^{-1}) on 340 K. The thick black line shows the flight path of RF05 and the star marks the aircraft position at 11 UTC. Magenta color shows the negative PV values.

The text "Moreover, as we will show later in the analysis of the model data, these profiles give an estimates of horizontal and vertical wavelength that are similar to those determined from the models" is modified to

Moreover, as we will show later in the analysis of the model data, these profiles give an estimates of at least horizontal wavelength that are similar to those determined from the models. The range of horizontal wavelength estimated is roughly 200-250 km.

7. Interpretation of Fig. 6d-f and GW breaking:

Comment: *If irreversible mixing is attributed to GW-induced KH instability, why does the GW appear to continue propagating beyond the UTLS in Fig. 6d-f? This would suggest that the GW has not undergone saturation or breaking, which would normally be required for GW-induced CAT as argued in Sect. 4.4. Alternatively, this may not be the intended interpretation. The discussion in L361-368 is somewhat unclear and appears to suggest that only reversible GW propagation contributes to shear generation. Please clarify this point.*

» The superposition of GW-induced shear with the background flow can locally enhance the total vertical shear, thereby reducing the Richardson number and favoring the onset of Kelvin-Helmholtz instability. In such cases, wave breaking and turbulence occur intermittently and locally, while other parts of the wave packet 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). Note that the KHI is based on the ERA5 field along the flight path. The low values of Ri suggest that KHI might occur and serve as the process which leads to the irreversible mixing. The irreversible mixing is diagnosed from the observed quantities and chances are high that the gravity wave is related to this mixing. Locally, not only the tracer show this sign for mixing but also state variables highlight that turbulence occurs (see Fig. 4). As noted by the reviewer, this does not lead to the total breakdown of the GW in the models but to a further propagation into the stratosphere. This is an interesting fact and the question remains whether this is the lack of the models to fully represent this process or whether as stated before that parts of the wave packet or additional wave components continue their propagation.

8. Figure 7 and GW contribution to shear:

Comment: *Since the authors argue that the GW contribution to shear is important for mixing, it would be very useful to show an analogue of Fig. 7 for S', i.e. the component of the shear associated with gravity waves. This would help to quantify how much GWs actually contribute to the shear and would also allow one to verify that this contribution is confined to regions where GWs can be clearly identified.*

» Done! We included the distribution of $S^{2'}$ to appendix.

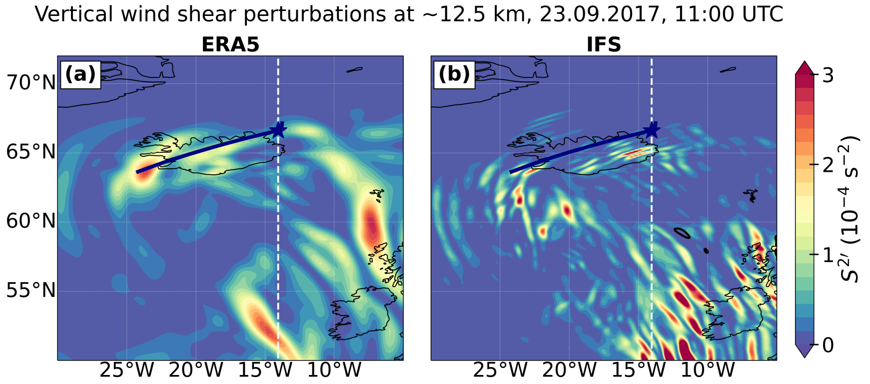

Figure 3. Distribution of vertical wind shear perturbations $S^{2'}$ at 12.5 km altitude for (a) ERA5 and (b) IFS.

9. Section 4.5 and momentum-flux deposition:

Comment: *To complement Figs. 12 - 13, it would be useful to show the vertical gradient of the resolved GW momentum flux without any additional modulation. One would expect momentum-flux deposition in the region where GW-induced KH instability, CAT, and turbulence occur. Is this observed?*

» To further assess momentum deposition, we now examined the vertical structure of the GW absolute momentum flux. In Figure 4, the largest magnitudes of AMF appear to be concentrated in the upper troposphere. ERA5 exhibit a pattern similar to IFS, whereas, ICON exhibits more localized but intense AMF that extend upward into the lower stratosphere (Fig. 4c). The AMF exhibits high magnitudes and sharp gradients in the vertical in regions of strong shear $S^2 \geq S_t^2$ and potential turbulence, particularly in the lowermost stratosphere. This figure has been added to the appendix.

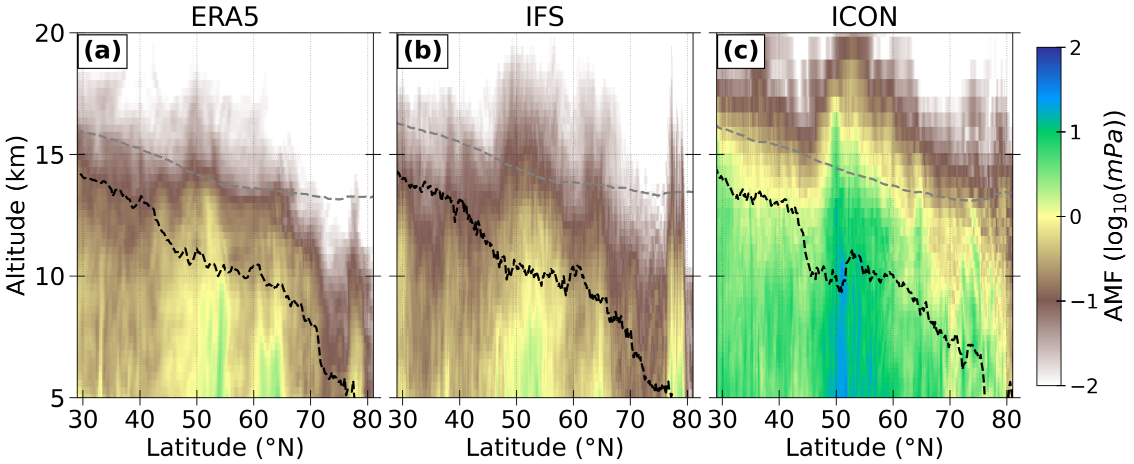

Figure 4. Zonal mean resolved GW absolute momentum flux (AMF, $log_{10}(mPa)$) for (a) ERA5, (b) IFS and (c) ICON.

Minor comments from the marked-up PDF:

Comment: *L120: Could you please clarify what you mean by 'derived'? Do you mean the forecasts have been initialized from the operational analysis? Or does this refer to the 137 levels, in which case these are not really derived from operational analysis data, but are the same vertical resolution used in analysis and in forecasts.*

» We intended to refer to the standard model configuration, where the 137 vertical levels are used consistently in both the operational analysis and forecast. The sentence modified to

... and incorporates 137 vertical levels which are the same vertical resolution used in analysis and in forecasts.

Comment: *"Particularly, the small transition between slopes observed in* N_2O *spectra around* 10^{-2} *Hz (green and blue lines), where the slopes of w indicates turbulent energy source, and here, the* N_2O *hints the turbulent behavior of small scales, e.g., those related to GWs, might be substantial to explain the dynamics in the lower stratosphere. This sentence is not at all clear. What do the authors want to say here?*

» Modified as:

Moreover, N_2O *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* N_2O *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: *What is the correlation in these? Could you provide some numbers please.*

» The shown Fig. 8 (in the original manuscript) is not targetted for the correlation, instead, it is the colocation or co-occurrences of GWs and shear perturbations under the potential turbulent episodes.

Comment: *Do you mean shorter vertical or horizontal wavelength GWs? Usually very short vertical wavelength gravity waves carry less momentum than long vertical wave length gravity waves because momentum flux goes like k/m (see eqn 7 in Ern et al: https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2004JD004752)*

» Right. We meant GWs with relatively short horizontal but large vertical wavelengths. 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.

Comment: *What is the relative contribution of this GW-induced shear to the total shear? This is not really discussed anywhere and would be an important to assess.*

» The central focus of the study is to understand the role of small-scale processes in shear generation. While a direct quantification of the relative contribution of GW-induced shear to the total shear is not explicitly performed, its influence is indirectly assessed through the shear perturbation field ($S^{2'}$, see also Fig. 3) and analysis of momentum flux filtered for regions with high shear occurrences (Fig. 13 in the original manuscript). A more detailed quantitative decomposition is beyond the scope of the present study but represents an important direction for future work.

Comment: *L563 "flow imbalance" This has not been made fully clear in this paper. Are the authors very sure the waves are not of topographic origin?*

» Please see our response to comment 2. The sentence now read as

...inertia GWs associated with the flow over orography...

References

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