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 3

Major concerns

***Comment** 1. The link between gravity waves and the occurrence of turbulence is expected from dynamical reasons, with the GW fluctuations enhancing the shear and modifying stability. However, the choice to use the Absolute Momentum Flux (AMF) to quantify GW when relating turbulence to gravity waves (l140-141). There is not necessarily a better choice for quantitatively relating the two. Nonetheless, the choice to use AMF should be better explained. What is the theoretical justification to use absolute momentum fluxes (AMF) in order to investigate "vertical transport due to small-scale processes"? For conservative waves (i.e. in the absence of dissipation), it is possible to have large fluxes associated to displacements which are reversible, and hence do not lead to net transport. AMF is a quadratic quantity, and it depends on the frequency of the waves, with the low-frequency waves carrying less momentum than high-frequency waves. The impacts of waves on stratification and on shear will also involve both their amplitudes, and their frequency.*

» One of the key properties of GWs is their ability to transport horizontal momentum vertically, then depositing it into the mean flow upon breaking. The use of absolute momentum flux in this study 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; Rapp et al., 2004).* This has been included in the revised manuscript.

***Comment** 2. About the discussion and conclusions: the authors benefit from a case which has been documented by airborne observations. Regarding the results that concern turbulence and mixing, how much of the conclusions are supported by evidence from the measurements? It is not sufficiently clear, in the concluding section, which statements are based on inferences based on modelled diagnostics (Richardson number, Turbulence Indices...) and which statements (regarding mixing and turbulence) are based on the measurements.*

» In the manuscript, the airborne observations are primarily used to confirm the presence of GWs and turbulence, as well as to identify GW-induced mixing event over the Iceland. In contrast, the model-based analysis is performed to investigate their spatial extent, evolution and their influence on shear and potential turbulence occurrence. We clarify that the first point in the conclusions is directly supported by the observational evidence (including ERA5 data interpolated along the flight track), whereas the subsequent conclusions are based on model-derived diagnostics such as GW momentum flux, shear, Richardson number and turbulence indices. This is clarified in the revised manuscript and results from the observations are more directly discernible from the model results.

Minor concerns

Comment *l16-18: 'Large amounts of momentum and energy can be propagated and transferred..' The sentence should be rephrased. There are two concerns: one should not overstate the role of gravity waves in the circulation, and the formulation is clumsy.*
Gravity waves matter for the general circulation. Their vertical propagation induces momentum fluxes that contribute to driving the mean circulation in the middle atmosphere (stratosphere and mesosphere). In the mid-latitudes, this contribution can be important but is secondary relative to that of Rossby waves, in particular planetary waves. In the mesosphere, the contribution of gravity waves is essential. The reference cited rather emphasizes the contribution of large-scale, balanced waves.

» Rephrased as:

GWs redistribute momentum and energy through vertical propagation, which affects the general circulation...

Comment *l32-34: moist processes turn out to play an important role in the generation of GW from mid-latitude and high-latitude weather systems (e.g. Wei and Zhang 2014, Plougonven et al 2015, Holt et al 2017)*

» Thanks for pointing this. It is now included in the text.

Moreover, tropospheric moist processes turn out to play an important role in the generation of GW from mid-latitude and high-latitude weather systems (e.g. Wei and Zhang, 2014; Plougonven et al., 2015; Holt et al., 2017).

Comment *36-38: The sentence on baroclinic life cycles is ambiguous: is this a statement on the contribution of weather systems (i.e. synoptic activity)? While weather systems are essential to the tropospheric circulation and zonally-averaged zonal flow, their influence does not necessarily penetrate very far into the stratosphere; the contribution from larger-scale Rossby waves is more important for the Brewer-Dobson Circulation (line 37). However, it seems that the topic of these two sentences should rather be 'Gravity waves excited from tropospheric weather systems (or synoptic eddies)...' given that the references given on line 38 are reviews on gravity waves. With gravity waves in mind, one should more clearly separate the different impacts:*
1. the vertical transfer of momentum; regarding this, what is in fact essential is the ultimate dissipation of the momentum fluxes associated to gravity waves, leading to forcing of the background flow at upper levels. Models to not take into account

a 'depletion' of momentum at the launch level of GW parameterization. In this sense, what matters really is the forcing of the gravity waves in the middle atmosphere, not a 'transfer of momentum', since the source region is not affected. This forcing of the middle atmosphere's circulation is the main motivation and main purpose of parameterizations.
2. there are local impacts due to gravity waves, such as enhancement of local shear, and onset of turbulence where the waves break. This has implications for vertical mixing. These effects are known to exist, but are not as high on the list as the forcing of the circulation in the stratosphere and mesosphere. This should be reflected in the way these implications are put forward in the introduction.

» L36-38 "Baroclinic life cycles, as persistent large–scale midlatitude wave patterns, play a pivotal role in modulating large–scale circulations. They influence the Brewer-Dobson circulation by vertically transporting momentum and energy, influencing jet dynamics, and affecting the potential vorticity distribution in the UTLS (Alexander et al., 2010; Achatz et al., 2024, and references therein)." modified to

Gravity waves excited from tropospheric weather systems (or synoptic eddies) can propagate into the UTLS and middle atmosphere. Upon dissipation, these waves deposit momentum and energy, leading to forcing of the background flow at upper levels and thereby contribute to large-scale dynamics such as the BrewerDobson circulation (Alexander et al., 2010; Achatz et al., 2024, and references therein).

***Comment** l48-49: the effect of GWs on the thermodynamic structure near the tropopause should be made more precise. GWs that do not dissipate induce fluctuations, which are reversible. Hence, although they modify locally the shear and stratification, this is averages out in a time average, a priori. The situation is different when dissipation is present: mixing due to breaking induces irreversible transport. Dissipation of momentum or heat fluxes induce a forcing of the background flow. What the authors have in mind here should be better explained.*

» We have modified the text for clarity. This reads

GWs likewise play a key role in shaping the thermodynamic structure of the UTLS by either enhancing vertical wind shear or reducing static stability (Kunkel et al., 2016; Kaluza et al., 2021; Dörnbrack, 2024) through reversible fluctuations; however, when they dissipate or break, they can induce irreversible mixing and flux convergence, thereby modifying the background flow on longer time scales.

***Comment** l75-78: There is a problem in the discussion of the motivations of this study. The different processes and different scales through which gravity wave may have impacts are stated in a way that includes repetition and which could be better organized, here and in other parts of the introduction. For example, in lines 75-78, the first sentence of the paragraph highlights the impact of gravity wave breaking for "vertical mixing of key tracers". The third sentence begins by "Beyond its relevance to aviation", although aviation has not been highlighted in the previous sentences, and "cross-tropopause transport" is highlighted - although this is very related to "vertical mixing of key tracers"...*

» We have modified the entire paragraph for better readability.

***Comment** l86-87: the status of the idealized baroclinic lifecycles is ambiguous from the phrasing: it may be understood that the impacts of gravity waves will be investigated in idealized baroclinic lifecycles. The next sentence clearly indicates that observations and a real case study will be considered, in apparent contradiction.*

» Corrected accordingly.

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 and shear 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** l91: "whether ERA5 is good enough resolved to study GWs, shear and mixing in the UTLS": to be rephrased, e.g. "which aspects of the gravity wave field can be reliably investigated in ERA5 fields, given their resolution."*

» Rephrased as:

The last point is related to the question which aspects of the GW field can be reliably investigated in ERA5 fields, given their resolution, and whether resolved spectrum of GWs is enough to study their impact on shear and mixing in the UTLS in case studies, and thus potentially may be suited for climatological analyses on this topic.

***Comment** l139: "ourself" -> "ourselves"?*

» Done!

***Comment** l139-141: not sure to understand the logic of restraining to the consideration of PDFs; could the authors explain?*

» It was a writing issue. We wanted to mention that we analyse GW momentum flux using PDFs, as the goal is to analyse the **occurrences of GWs** and not the characteristics. Rephrased for clarity.

***Comment** l155-161: while the methodology appears reasonable, and indeed consistent with what has been done in other studies, it does mix different methods (spectral approach to obtain the perturbation quantities, then lowpass Gaussian filter to average over several wavelengths and obtain a smooth estimate of the fluxes.*

» Right. The point is to mention that we consider **hybrid approach** following Wei et al. (2022) but not the same methods. However, it is mentioned in the steps to smoothen the averaged fields either by applying Gaussian lowpass filters or or areal smoothing (Kruse and Smith, 2015). This has been clarified in the manuscript.

***Comment** l176: "to account [FOR] this limitation"?*

» Corrected.

***Comment** l178-179: the structure of the sentence is not clear, to be rewritten*

» Rewritten as:

The Ellord-Knapp Turbulent Index (TI1) has previously been shown to be capable of detecting 70%-84% of CAT occurrences (e.g., Ellrod and Knapp, 1992; Sharman and Pearson, 2017; Kim et al., 2018; Gultepe et al., 2019; Thompson and Schultz, 2021).

***Comment** l209: word missing? "between two equatorward [?] reaching..." or placed later in the sentence ("streamers")?*

» Nope. It reads as "two equatorward reaching stratospheric streamers"

***Comment** l229: "ascend" -> "ascent"?*

» Corrected.

***Comment** l276-277: the paragraph discusses the measurements. Suddenly,*

» The sentence related to temporal resolution in models is only to explain the limitations against observations and why we attempted to look for gravity wave signatures in the measurement.

***Comment** l292-293: "which rotates anticyclonically (cyclonically) for an upward (downward) propagating wave" -> "which rotates anticyclonically for an upward propagating wave, and cyclonically for a downward propagating wave."?*

» Done!

***Comment** l334-335: the conclusion that the IFS manages to capture the appearance of inertia-gravity waves excited by jet-front systems, despite a limited resolution, is consistent with findings of previous studies that have investigated gravity waves in ECMWF products and reanalyses in particular (see Jewtooukoff et al, 2O15, and references therein).*

» Thanks for the suggestion. We have now cited Jewtoukoff et al. (2015).

***Comment** l343: the authors emphasize the shear in Fig. 7, "in particular in ICON". This is somewhat at odds with the figure, in which the shear in ICON seems weaker.*

» Right. These differences might be due either to the representation within ICON or to the level we are focusing on. Another possibility is that this reflects a systematic difference between ICON and IFS. As also noted by another reviewer, this discrepancy might be due to the fact that 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. Following this, we have separated discussion of ECMWF products and ICON simulation in the revised manuscript. These modifications are detailed in central changes.

***Comment** l371-391: as far as I understand, the relation between AMF (absolute Momentum Flux) and the shear is somewhat opportunistic, rather than a fundamental relation. Momentum fluxes are used as a proxy for the presence of waves, and these contribute to shear. Figure 8, perhaps because of the logarithmic axis and logarithmic colorbar, does not seem so compelling regarding the correlation between shear and AMF. The text and comments around this figure should clearly recognize that this relation is tentative.*

» While we agree that AMF serves as a proxy for GW activity, the relationship between AMF and shear **perturbations** in our analysis is not purely opportunistic. In addition to the evidence of single event presented here, a consistent relationship between GW activity and enhanced small-scale shear is also found in a analysis of annual cycle (see also, Umbarkar et al., 2026), which supports the robustness of this result. Nevertheless, we acknowledge that the relationship may depend on the underlying flow conditions. The text has been revised to reflect this interpretation.

Comment *l535-537: it is worth noting that there are several different uses that can be made of ERA5 for studies of GWs. It is very challenging and demanding to expect the representation of small-scale shear to be accurate, let alone the representation of the resulting turbulence. Providing insights about regions favorable to the occurrence of gravity waves is more reasonable. In the assessment of the value of ERA5 for GW studies, it should be mentioned that there are different levels of information that may be targeted.*

» We have included the following text:

Nonetheless, ERA5 does capture a large portion of GW spectrum and most of the enhanced shear encounters. As such the reanalysis data provides valuable information on the spectral characteristics of GWs and is therefore suitable for long term analysis of GW-induced shear, at least over the North Atlantic. In this context, different levels of information can be targeted: ERA5 is well suited to characterize the large-scale environment and regions favourable for GW activity, and to some extent their resolved spectral characteristics, whereas its limited effective resolution constrains its ability to accurately represent small-scale shear and turbulence.

Comment *l540: "small-scale gravity waves": it is worthwhile being more precise here. It is possible to have low-frequency waves which are small-scale in the vertical (vertical wavelength of a few hundred meters) yet retain horizontal wavelength on the order of several tens to a hundred of kilometers. It is possible to have higher frequency waves that have horizontal wavelengths only of a few kilometers (and similar vertical wavelengths). The frequency (which can be estimated from the tilt of the phase lines) matters very much in the description of the waves.*

» Right. Text modified to

...to multiscale interactions dominated by upward-propagating large-amplitude GWs with relatively short horizontal but large vertical wavelengths.

Comment *l541-544: the relationship highlighted here is not clear: do the authors highlight GWs producing pockets of turbulence, through local enhancement of shear? Or do they have evidence for secondary generation of smaller-scale gravity waves where a primary wave-packet breaks? The second interpretation does not appear very likely, but the structure of the sentence is ambiguous (∼ "Turbulence (...) correlates with appearance of (...) small-scale gravity waves"..*

» Text modified to

GW–induced enhanced shear, which are conducive to turbulence, highly correlates with appearance of envelopes (or packets) of small–scale GWs of ∼100 km of wavelength in the LMS with the same characteristics.

Comment *l560-561: on the difficulty of convergence with resolution, the study by Kruse et al. (2022) is also relevant.*

» Thank you for the suggestion. We have now cited Kruse et al. (2022).

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