Authors response to "Contribution of gravity waves to shear in the extratropical lowermost stratosphere: insights from idealized baroclinic life cycle experiments."

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We thank the referee for the careful reading and constructive criticism on our manuscript. The suggestions helped improve the manuscript, particularly by prompting a more thorough discussion of uncertainties. Below, we address each comment in detail and outline the corresponding revisions made to the manuscript (Reviewers comments in italics and corresponding revised text passages highlighted in italic blue).

Central changes in the revised manuscript

– Motivated by a comment from one of the referees, we have replaced our gravity wave filtering method by incorporating an additional dynamical constraint: the extraction of the divergent wind components using the Helmholtz decomposition technique. In the updated manuscript we derive the small scale effects, i.e., the prime quantities, in the following way:
1) extraction of the divergent wind components using the Helmholtz decomposition
2) spectral decomposition in which the filter is applied in spectral space using a one-dimensional zonal FFT over the Northern Hemisphere and removes the contributions from wavenumber 0 to 8,
3) In the subsequent step, we applied an additional Gaussian low-pass filter to the spectral decomposition in the computation of small-scale momentum flux products (i.e., $\overline{u'w'}$ and $\overline{v'w'}$), following the approach of Kruse and Smith (2015).
This further smoothing better suppresses residual large-scale contributions that may not be fully removed by the initial spectral filtering, thereby improving the separation of small-scale gravity wave effects. This approach affects of prime quantities and thus the figures which show such variables. In detail, we revised Figures 6, 7, 8, 9, 10, 11 and 12 of the original manuscript. We note here that especially the occurrence frequencies of $S^{2'}$ show slightly reduced values but we emphasize that the interpretation of the results and thus the major conclusions drawn from the analysis do not change.

– To address the reviewers concern regarding the realism of moist processes, we included an analysis of 850 hPa relative humidity to show the distribution of the moisture during the baroclinic life cycle. A new appendix figure illustrates its spatial distribution in the MOIST simulations.

- We have added further details on the selection criteria for S^2 and Ri thresholds to clarify the characterization of dynamic instability and shear occurrences across the different experiments.

Response to Reviewer 2

Major comments:

1. Idealized Setup – Scope and Limitations

Comment: *While idealized experiments offer a controlled framework to isolate and study specific processes, the limitations of such an approach should be acknowledged in the introduction. I encourage the authors to elaborate on the potential constraints of transferring these results and conclusions to real-case simulations.*

Reply to comment: We thank the reviewer for this important comment and agree that the limitations of our idealized framework should be stated clearly. We now have included proposed changes in the revised manuscript.
We have added a note acknowledging the limitations of the idealized framework and the need for caution when transferring results to real cases. We note that in a follow-up study we also analyse real case situations for which we have high horizontal resolution of $\Delta x \approx 5$ km, where identical contribution of gravity waves to generation of enhanced vertical wind shear and potential turbulence were found. These findings further support the relevance of our results and will be detailed in a forthcoming study (manuscript in preparation).

L113: Idealized numerical experiments of baroclinic waves, and in particular the representation of gravity waves in such simulations, have certain limitations. While the appearance of gravity waves and their effect on shear are tightly coupled to the model resolution, the findings of this study might be regarded as a lower estimate of the effect of gravity waves on shear generation.... Nonetheless, our idealized setup allows a controlled investigation of the effect of gravity waves emitted within baroclinic disturbance while isolating their role from other processes.

2. Resolution and Model Configuration

Comment: *The chosen in the study domain with hight of 35 km may be sufficient for UTLS-focused analyses. However, the interactions of GWs with large-scale circulation higher in the stratosphere are likely underrepresented. Additionally, it should be noted that GW and convection parameterizations are not included in the coarse resolution runs (e.g., Δx = 80 km), where such processes are arguably unresolved but highly relevant. Therefore, in my opinion, conclusions drawn from these runs—particularly concerning the absence of GW activity—is to be expected.*
I suggest the authors explicitly discuss these limitations and, if possible, include sensitivity tests with GW and convection parameterizations enabled as well as one test case with higher model top. Such inclusion could refine the understanding of the processes of relevance such as vertical momentum transport, and resulting shear evolution in coarser resolutions.

Reply to comment: We thank the reviewer for the valuable feedback. While we acknowledge the importance of including gravity wave and convection parameterizations in coarse-resolution experiments, we would like to clarify that the use of a coarse grid spacing (Δx = 80 km, corresponding to R02B05) was deliberately used for sensitivity purpose within our idealized framework.

Moreover, implementing a convection parameterization lies outside the scope of this study. Our focus is to resolve the effects of GW within baroclinic life cycles with a particular focus on dry dynamics. Accounting for the effects of convectively generated GWs requires a more comprehensive setup: convection, parameterized or resolved, requires other processes like radiation, surface processes and of course moisture to be included. First of all, this would increase the complexity of our model setup substantially. Second, the interpretation would be much more complicated. We have chosen this setup to be able to determine which gravity waves emerge during a baroclinic life cycle in a dry setup depending on the resolution. Additionally, we investigate how does this occurrence of GW change with minimal adjustments in our setup, i.e., the inclusion of moisture and the inclusion of the turbulence parameterization. This gives a baseline from which a consequent study could then address the next question: what the additional effect of convectively generated GW are. Besides this, we have verified that increasing the model top from 35 km to 45 km does not significantly affect the GW activity in the lowermost stratosphere, nor does it change the results in terms of GW contribution to shear in the LMS.
As mentioned earlier, regarding GW drag and convection parameterization, we have experiments performed in real case as a separate publication involving understanding GWs in convection permitting simulations to more closely examine the GW and convection contribution to tropopause wind shear layer formation.

L114: Also note that this study specifically focuses on gravity waves generated by jet–front systems during baroclinic wave evolution. Gravity waves from other sources, such as orography and convection, are not considered, as their adequate representation would require different model configuration and substantially higher resolution which are beyond the scope of this study.

3. Figure Clarity and Presentation

Comment: *Figure 1: Consider splitting into two panels—one showing θ and the other q fields.*

Reply to comment: Thank you for suggestion. The colors and scale of variables in Figure 1 has been updated instead, as suggested by Reviewer 1.

Comment: *Figure 2: Maybe it worth to add marked zones for a particular feature on the figure and refer to them in the description for the better understanding*

Reply to comment: Thank you for the suggestion. Instead surface pressure have been added to Figure 2 for better clarity.

Comment: *Figures 3–4: Unify altitude labeling and fix overlapping x-axis ticks.*

Reply to comment: Thank you. Altitude labels and x-axis ticks in Figures 3-4 have been corrected.

Comment: *Figure 6: The occurrence of shear is not visually clear. Consider using colored lines (e.g., red) for better contrast and increasing font/line size*

Reply to comment: Thank you. Figure 6 has been updated with red colored lines and increased line thickness to improve visual clarity.

Comment: *Figure 7: Consider moving 7a to the appendix.*

Reply to comment: We have moved Figure 7a to the appendix in the revised manuscript.

Comment: *Figure 10: Note the change in y-axis scale for different cases (dry vs. moist).*

Reply to comment: We understand the reviewers concern. However, the $\mathrm{REF_{wind}}$ TURB simulation exhibits lower shear values and behaves similarly to the dry reference case. For consistency and clear comparison, we have retained the current y-axis scaling.

Comment: *Figure 13: a/b panels depict different altitudes. If the discussion centers on the UTLS, consider extending the altitude to 22 km. It seems that figures for model time of 288 and 312 h would exhibit some features above the 18-20 km, which might be of relevance. The figures might also benefit from cropping Ox to 15–75° longitude.*

Reply to comment: We thank the reviewer for the suggestion. Figure 13 has been updated to extend the altitude range to 22 km and cropped to 15–75° latitude to better capture relevant UTLS features. We assume the reviewer intended to refer to latitude rather than longitude. We also want to mention that our focus is on the extratropical lowermost stratosphere, which is the region between the extratropical and tropical tropopause.

Comment: *Figure 14: Remove the 264 h panel from 14e for consistency.*

Reply to comment: Done!

Minor comments:

Comment: *Lines 37–52: The introduction could benefit from a more quantitative framing (e.g., order-of-magnitude estimates of GW-induced shear/turbulence) and acknowledgment of uncertainties or active debates (e.g., resolution sensitivity or parameterization limitations).*

Reply to comment: We include the following statements in the updated manuscript:

L43: Observational studies suggest that GW-induced enhancements of S^2 can reach values on the order of 10^{-2}–10^{-3} s^{-1} (e.g., Lane et al., 2004; Kaluza et al., 2021), particularly in dynamically active regions such as baroclinic instability, jet streaks, and upper-level frontal zones (Koch et al., 2005; Wang and Zhang, 2007).

L52: However, the exact magnitude and representation of these processes remain sensitive to model resolution and the treatment of subgrid-scale gravity wave processes, particularly in parameterized frameworks. This continues to be an area of active research and debate in both modeling (e.g., Plougonven and Zhang, 2014; Stephan et al., 2019) and observational (e.g., Geller et al., 2013; Jewtoukoff et al., 2015) studies.

***Comment:** Line 23: "ascend" -> ascent*

Reply to comment: Done!

***Comment:** The introduction largely describes the importance of GWs and related phenomena without discussing potential challenges or uncertainties in the field. A brief mention of existing debates or challenges in studying GWs (e.g., model resolution limitations or uncertainties in parameterization) could provide a more critical perspective.*

Reply to comment: Please see our reply to previous comment.

***Comment:** Line 29: Fix spacing in "(UTLS)(e.g., . . .)"*

Reply to comment: Done!

***Comment:** Line 90: Add commas around "which are quasi-periodic"*

Reply to comment: Done!

***Comment:** Line 128: Remove "so-called"; reduce ICON technical details and avoid referencing polar grid issues in idealized simulations*

Reply to comment: Modified as;

L128: ... alleviating numerical stability concerns associated with traditional latitude-longitude grids.

***Comment:** Table 1: When referring to the resolution R03B07, horizontal grid spacing is Δx = 13 km*

Reply to comment: Done!

***Comment:** Line 210 and thought the text: Unify case labels ("Moist" vs "MOIST")*

Reply to comment: Thank you for your suggestion. We changed the wording accordingly.

***Comment:** Line 223: Replace "after the model starts" with after the simulation time*

Reply to comment: Replaced as "after the simulation start"

Comment: *Line 227: Does that mean that the definition of the dynamical tropopause is taken from the cited literature? It would be better to reformulate the sentence for clarity.*

Reply to comment: Done!

Comment: *Line 232: Please consider rephrase it in passive, for instance: Differences are observed in the size of the PV streamer…*

Reply to comment: Done!

Comment: *Line 240: Suggested rewording: "Wave activity was not prominent before the indicated simulation time…"*

Reply to comment: We have reworded the sentence for clarity.

Comment: *Figure 3 caption: Clarify as "model integration time"; please remove hyphen from "11 -km"*

Reply to comment: Done!

Comment: *Line 270: Use the abbreviation UTLS*

Reply to comment: Done!

Comment: *Line 272–276: Clarify whether observations refer to a specific case or general result; re-order figure reference*

Reply to comment: We agree and changed the wording accordingly.

L272–276: In general, the evolution of the baroclinic wave is similar across all dry simulations. While the large-scale structure remains consistent, mesoscale differences and slight variations in the exact location of the trough are evident. These are general features observed across all simulations, independent of the perturbation function. As a result, gravity wave activity tends to emerge in similar regions among all dry simulations. Figure 4 illustrates the distribution of horizontal divergence at 11 km altitude after 288 h for the respective simulations, highlighting these similarities.

Comment: *Line 287: Replace viz. with i.e. or namely for clarity*

Reply to comment: Changed!

Comment: *Line 334: Consider replacing "air parcel" with air masses, since it is a description of a physical process and broader term would be better suited.*

Reply to comment: Done!

Comment: *Line 343–344: Suggested rewording: "We have shown that GWs emerge under various initial states, grid resolutions, and process complexities."*

Reply to comment: We have changed the wording accordingly.

Comment: *Line 352: Avoid starting a sentence with a symbol; rephrase*

Reply to comment: We have rephrase the sentence accordingly.

L352: As all the other prime quantities, $w^{'}$ here represents filtered quantities.

Comment: *Lines 374–375: Combine into one clear sentence (e.g., "We focus our analysis on strong wind shear and static stability (N^2), with emphasis on GW-induced shear and potential turbulence in the LMS.")*

Reply to comment: Done!

Comment: *Line 376: Replace parcels with masses*

Reply to comment: Done!

Comment: *Line 379: Include the formula for the gradient Richardson number alongside the non-dimensional form for clarity*

Reply to comment: We have now included the formula, as suggested.

L379: ... Richardson number, Ri:

$$Ri = \frac{N^2}{S^2} = \frac{\left(\frac{g}{\theta}\frac{d\theta}{dz}\right)}{\left(\left(\frac{\partial u}{\partial z}\right)^2 + \left(\frac{\partial v}{\partial z}\right)^2\right)} \tag{1}$$

which is defined as the ratio of static stability (N^2) to the vertical shear of the horizontal wind (S^2), with the gravitational acceleration g and the zonal and meridional wind components u and v.

Comment: *Line 384: Phrase "on the resolution used in this study" seems misplaced—restructure for coherence*

Reply to comment: We modify the sentence to make the statement more clear.

L384: However, in studies using output from numerical models with comparable spatial resolution, higher Richardson number thresholds are commonly used to identify regions prone to dynamic instabilities.

Comment: *From Figure 6 it seems to me, that the GWMF is not overlapping with the shear occurrence in "stream" case. Is there an explanation for such behavior?*

Reply to comment: We thank the reviewer for this observation. While the overlap between GWMF and shear occurrence in the "stream" case may appear less pronounced at first glance, this is largely due to the scale used for GWMF. Upon closer inspection, there is indeed spatial co-location between regions of elevated GWMF and vertical shear. We have clarify this point in the figure caption and adjust the scaling for improved visual consistency.

Comment: *Line 392: Define S' (small-scale shear) again, as the earlier mention is far removed*

Reply to comment: Changed!

L392: ... where $S^{2'}$ represents small-scale shear defined as the deviation from the background vertical shear, in the LMS. We focus

Comment: *Figure 8: Consider separating panels into different figures or moving higher-res versions to the appendix*

Reply to comment: We appreciate the suggestion. However, we prefer to keep Figure 8 in its current form to maintain the consistent structure of the manuscript and allow direct comparison of similar diagnostics across the dry and moist sensitivity experiments.

Comment: *Line 395: Rephrase/remove "right side of the distribution" for clarity.*

Reply to comment: We have revised the sentence accordingly.

L395: ... Figure 7a-d) shows temporal variation in the positive tail of the distribution.

Comment: *Line 396: Reword for clarity: what is the comparison baseline for "larger S^2 values"? I am struggling to understand the discussion, therefore clarifying the paragraph is required.*

Reply to comment: Thank you for the comment. The sentence has been revised to clarify that the increase in "larger S^2 values" refers to the maximum value of S^2 at later simulation times as seen in the positive tail of the distribution in Fig.7b (now Fig. 7a-d)

L396: There is an increase in the occurrence of S^2 maxima with time, particularly during the strong GW activity.

Comment: *Lines 401–410: Add explicit figure references to help the reader follow the analysis.*

Reply to comment: Thank you for the suggestion. The paragraph have been modified in the revised manuscript.

Comment: *Line 411: "On smaller scales and in instantaneous considerations" does it refers to the instantaneous outputs?*

Reply to comment: Yes, "instantaneous considerations" refers to individual model output time steps.

Comment: *Line 415: Clarify whether turbulence rarity refers to model output or general atmospheric behavior. Cite observational studies.*

Reply to comment: Done!

L415: Turbulence is a rare event under general atmospheric conditions (Sharman et al., 2012; Dörnbrack et al., 2022)

Comment: *Figure 10: Is there any explanation on higher S'2 in the case TURB Moist for S^2 close to 0 (Figure10e lower panel)? Could you please elaborate, whether it is connected to the gap in high S2 occurrence with small N^2 (Figure10e upper panel)?*

Reply to comment: We thank the reviewer for careful observation. The unexpected behavior in Figure 10e (lower panel) was due to a technical error on our side. The updated figure now accurately reflects the relationship between $S^{2'}$ and N^2 in the TURB Moist case.

Comment: *Line 420: Suggested rewording: "Dynamic instability becomes likely when Ri $\leq$ 5."*

Reply to comment: We have reworded the sentence as:

L420: For Ri $\leq$ 5, an indication of potential for the occurrence of dynamic instability is given.

Comment: *Line 440: The values of shear were also shown on Figs. 7,8. Indicating same situation, lower values for dry cases and 10 times larger S2 values for moist cases. Please, refer back to previous figures (7, 8) where similar shear behavior is present but not discussed.*

Reply to comment: Thank you for the suggestion. Text has been modified accordingly.

L440: The shear values reach up to ten times higher than in the dry cases, as also evident in Figures 7 and 8, which show consistently lower shear values in the dry simulations compared to the moist cases.

Comment: *Lines 483, 485: Replace "exceedance" with "exceeding a defined threshold"*

Reply to comment: Changed!

Comment: *Line 511: Clarify whether "TSL occurrence across sensitivity experiments" refers to figures or broader results*

Reply to comment: The mentioned text refers to the broader results, specifically those illustrated in Figure 14. We have clarified this in the manuscript by referring to the relevant figures.

Comment: *Line 550: Use plural: GWs*

Reply to comment: Done!

References

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