Reviewer 2

This article proposes a method to study the variability of gravity wave spectra from data of a high-resolution global model. The method is introduced and some spectra are calculated, and the potential use of the method in gravity wave parametrizations is pointed out.

The topic is certainly relevant, and the treatment seems to be correct, but what I think is missing a bit in this paper is a clear scientific problem to address. For this reason, it is my opinion that the paper needs substantial revisions to become a stronger piece of research.

We would like to thank the reviewer for their helpful review and thoughtful comments that helped us to improve the manuscript. We think we were able to successfully resolve all comments mentioned by the reviewer. Answers to the individual comments can be found below.

General comment

The emphasis of the paper is on use of a particular method for obtaining spectra of gravity waves in the atmosphere, but there is no well-defined scientific problem to address, apart from finding the characteristics of those spectra. The authors make some vague allusions to the use of the results in gravity wave parametrizations for lower-resolution meteorological models, but this is not really pursued to any significant extent, so the reader gets the impression that the methodology is being presented for the sake of it. It would be good if the method had a more direct immediate scientific objective, linked with understanding some relevant phenomenon, even if its use in improving gravity wave parametrization is relegated to a follow-up publication.

Thank you for raising the issue of the insufficient motivation of the work. Since GW parametrisations generally study effects of prescribed spectra of GWs, analysis of the resolved spectra should indeed help to improve the parametrisations by describing the sources more realistically. For example, for non-orographic non-convective GW sources, this is currently done by assuming these waves follow a universal Desaubies spectrum (Bölöni et al., 2021). In our work, we show by the principal component analysis that the gravity wave spectra are quite variable, and an inclusion of this variability into the parametrisations would be beneficial, which is why we suggest the machine learning approach to predict the shape of the spectrum used. Although we are currently working on such a technique, we think it is better to keep the space in this manuscript to be able to carefully introduce the novel methodology used and show the variability of the spectra.

We modified the introduction section to be clearer about it. Please check the revised manuscript to see the new version.

Bölöni, G., Kim, Y. H., Borchert, S., & Achatz, U. (2021). Toward transient subgrid-scale gravity wave representation in atmospheric models. Part I: Propagation model including nondissipative wave–mean-flow interactions. *Journal of the Atmospheric Sciences*, *78*(4), 1317-1338.

Specific comments

Line 19: "enhance their resolution". Strange phrasing. I suggest replacing this by "have had enhancements to their resolution", or something similar.

Thanks for the comment, reformulated to "the resolution of current simulations of the weather and climate system has been improving".

Line 27: "One dimensional spectra of atmospheric quantities...". It is important to note that the focus in this study is on waves, not turbulence, as spectra are often referred to in the context of turbulence. I suggest adding an allusion to waves in this sentence, to clarify this.

Thanks for pointing out the potential misleading. Reformulated to "One-dimensional spectra describing atmospheric waves using temperature or wind velocities"

Line 33: "localized". It is unclear what this means exactly. Please briefly explain.

Modified to "to be evaluated at local domains for visualizing the wave field at specific locations".

Line 37: "the triangular subdomains". This may be familiar to users of ICON, but not to readers in general. Please explain why this is the natural shape of domains in ICON.

Reformulated from "To this end, we introduce a novel methodology that uses data restricted to the triangular subdomains and projects the spectra to GW modes using linear GW theory."

to

"Since the applied ICON model uses a triangular grid to improve numerical stability around the poles, we adapt our procedure to fit the grid. Therefore, we introduce a novel methodology that uses data restricted to the triangular subdomains and projects the spectra to GW modes using linear GW theory."

Line 42: "results are discussed and concluded". Again, strange phrasing. I suggest replacing by "results are discussed and conclusions presented".

Thanks for the suggestion, the text was modified accordingly.

Line 57: "local spectra". Explain in more detail what "local" means here.

Changed to “spectra at the subdomains” to link more smoothly to the previous paragraph.

Line 67: "deplaned and tapered". Whereas it is explained in adequate detail what "tapered" means, this needs to be done in more detail for "deplaned".

Thank you for pointing out the missing explanation. The following sentence was added:

“The deplaning is done by subtracting plane fitted to the data in the triangle, using the linear approximation approach introduced in Powell (1994).”

Lines 73-74: "defined as the zonal and meridional sides of the smallest rectangle in which the subdomain can be inscribed". As a non-expert in this area, since this procedure (inscribing the triangle in the rectangle where the spectrum is calculated requires a substantial amount of padding, I wonder if it would not make sense to do the opposite, i.e., inscribe the rectangle where the spectra are calculated in the triangle. In that case, there would be non-zero data everywhere. Would this place undue limitations on the maximum length scales that could be included in the spectrum? Please discuss this.

Thank you for the comment. We have selected to take L_x and L_y as the sizes of the “larger” rectangle, as the longest waves that might be represented by the data will be determined rather by the size of the triangle, including the peripheral regions. Taking a smaller rectangle would be possible, it would however only limit the range of the *k* and *l* values in the resulting spectrum.

Lines 85-86: "F is defined by Eq. (3) as a matrix containing terms like ...". This is not very clear from Eq. (3). I suggest mathematically defining and presenting both F and hat(f) separately.

Thank you for pointing out the unclear definition. We have separated the original operator form of the definition (better understandable) from a more technical one (sines and cosines; used in the algorithm). The text around the equations is reformulated.

Line 87: "lambda || \hat(f) ||". Briefly explain the importance and meaning of the term involving lambda.

The following sentence was added: The regularizing term $\lambda \left|\left|\hat{f}\right|\right|^2$ is necessary to avoid overfitting.

Line 88: "this corresponds to solving the equation...". It is not clear how Eqs. (4) and (5) are linked. Please provide some intermediate steps that show this.

We corrected a typo in Eq. (4) and reformulated the sentence to

“The minimiser can be obtained by evaluating the Gateaux derivative of the functional *J* and setting it to zero, which leads to the equation...”

Lines 90-91: "Eq. (3) describes the real Fourier transform and its equivalence to the formula for the standard Fourier transform". It this really necessary? I would think that it is clear enough to present either Eq. (3) or Eq. (6).

Thank you for the comment, we removed Eq. 6.

Lines 98-99: "Note the negative sign in the definition of omega that allows us to write...". Is it really important that omega is negative, or just a convention, which relies on how the Fourier series and the Fourier coefficients are defined?

In this case, we think that in this case, the sign of the Fourier is important, as it influences the signs of omega further in the results, and thus determines, e.g., if we are describing upward or downward propagating waves. Therefore, we would prefer to keep this comment in the text.

Line 105: Eq. (10). The notation f_xyt and f_klw differs from that introduced previously, where x,y,t and k,l,omega appear inside brackets, instead of as subscript indices. Please make this notation uniform throughout the paper.

Thanks for noticing this. We corrected the notation in Eqs. 10 and 11.

Line 111: "the scaling factor alpha_u". Is this scaling factor uniformly applied across scales? If yes, please mention it explicitly.

It is. We modified the sentence:

To solve this issue, we introduce a scaling factor for each of the variable modifying its spectral amplitude based on the Parseval theorem, which is applied on the spectra uniformly across scales.

Line 114: "The scaling factors are computed using a single time step only". Eq. (11) suggests that the scaling factor is only computed based on the spatial Fourier transform, and not the temporal one. Is this so? If yes, explain why.

Thanks for the comment. We added the sentence:

The factor is applied during the computation of the horizontal spectra and the results obtained from them (i.e., the 3D spectra and their projection to GW modes) are therefore modified as well.

Line 121: "for both gravity waves and sound waves". Actually, waves which are affected by the Coriolis parameter are usually called inertia-gravity waves. Eq. (12) expresses the dispersion relation for waves which are affected by gravity, Earth's rotation and the compressibility of air. Should they be called "gravity waves and sound waves", or rather "inertia-gravity-acoustic waves" or something similar?

In our opinion, there are different conventions for this. Although some studies indeed use the term inertia-gravity waves, there are other studies where the authors stay with a general term “gravity waves” (e.g., Kirsch et al., 2020). Since the subdomains used in our study are relatively small and the Coriolis effect is stronger for larger wavelengths, we think that mentioning the inertia waves in the study would be confusing and we would rather stay with the current notation. However, we have changed the “gravity waves and sound waves” to “acoustic-gravity waves” when mentioning the general dispersion relation, thanks for the suggestion.

Krisch, I., Ern, M., Hoffmann, L., Preusse, P., Strube, C., Ungermann, J., ... & Riese, M. (2020). Superposition of gravity waves with different propagation characteristics observed by airborne and space-borne infrared sounders. *Atmospheric Chemistry and Physics*, *20*(19), 11469-11490.

Line 139: "the vector describing the polarisation relation above". It is not totally clear what this vector is. I suggest presenting the definition of the mentioned vector explicitly.

We mentioned the form of the vector.

Line 159: Eq. (21). It is worth noting here that the last term in this equation corresponds to potential energy (unlike the preceding terms, which are kinetic energy).

Thanks for the comment. We reformulated it to:

The total energy of the projected gravity wave modes can be defined as (...), where the first three summands correspond to the kinetic energy and the last one to the potential energy.

Line 162: "unless the wave dissipates or generates". Strange phrasing. I suggest replacing it by "unless the wave dissipates or is being generated".

Thanks for the suggestion, the text was modified accordingly.

Line 170: "without a clear orography-based GW activity". I am not sure Fig. 1 shows this. There seems to be a certain correlation between orography elevation and wind speed.

What was meant here is that it is not a typical orographic GW hotspot. We reformulated the text to “without distinct orography that could cause significant GW activity”.

Lines 174-175: "When averaged over the bands of horizontal wavelength size". Is this averaging also performed over the different directions? If so, this should be explicitly mentioned.

Thanks for the comment. We indeed meant averaging over different direction by averaging spectra with all pairs of k and l such that $\sqrt{k^2 + l^2}$ lies within an interval (K_i, K_{i+1}), where K_i and K_{i+1} define a binning of the horizontal wavenumber space.

Reformulated to "When averaged over the bands of horizontal wavelength size across all directions, the slope of the spectrum (...)"

Line 185: "the symmetry of Fig. 1e is no longer present". The authors may mean "the exact symmetry of Fig. 1e", because Fig. 1f is also to a large extent symmetric, but presumably not as much as Fig. 1e.

Thanks, we have made the suggested change.

Line 207: "not as strongly circular". Do the authors mean "not as isotropic" or "more anisotropic"? If so, I think these latter forms are more appropriate.

Thank you for the suggestion, replaced by "more anisotropic".

Line 210: "The more noticeable difference" should be instead "The most noticeable difference".

Thank you, substituted by "The most noticeable difference".

Line 216: "Global distribution gravity waves". There should be the word "of" between "distribution" and "gravity".

The word "of" was added.

Lines 239-240: "The gravity wave energy is higher at the Northern Hemisphere since the wind is stronger in the winter hemisphere". Is this the reason, or is it because there is much more orography in the northern hemisphere, or possibly both reasons? If so, please correct appropriately.

Thank you for the comment. It is true that there are more orographic gravity wave hotspots on the Northern Hemisphere. On the other hand, in northern-hemispheric summer, the gravity wave effects are generally higher in the Southern Hemisphere due to extreme values around the southern Andes and Antarctic Peninsula (e.g., Procházková et al., 2025). The following sentence was added to the text:

“Although the individual hotspots in the Northern Hemisphere produce lower GW energy compared to the southern Andes region, there is more orography in the Northern Hemisphere, increasing the total zonal-averaged energy.”

Procházková, Z., Zajíček, R., & Šácha, P. (2025). Climatology, long-term variability and trend of resolved gravity wave drag in the stratosphere revealed by ERA5. *Weather and Climate Dynamics*, *6*(3), 927-947.

Caption of Fig. 3, line 1: "Global distribution of the GW". Since GW projection was not done yet at this stage, it would better to call this "total energy", avoiding use of "GW", if I am not mistaken.

GW projection was already done in this figure. We added text “as obtained by the GW projection” to the figure caption to make it clearer.

Line 276: "the most comprehensive PCs". It is unclear what "comprehensive" means here: "numerous"? If so, the latter formulation might be more specific and thus preferable.

Modified to “provide PCs with the most comprehensible meaning”.

Line 299-300: "Although the reconstructed spectra for 200 PCs for both groups do not look exactly the same as the original spectra...". 200 PCs sound quite a lot. Is using such a large value of PCs really useful for parametrization? Why do the spectra converge so slowly?

Thank you for the question. As each of the original spectra has about 2600 data points, reduction to 200 still brings in some simplification, although it is not such a low number.

Nevertheless, we have changed the analysis to work with log(N+1) instead of log(N). The advantage is that with log(N+1), we focus on the scales where N is larger, which is for the wavenumbers with “larger amount” of GWs and is thus more physical. The PCA therefore works better for these wavenumbers and the approximation by even 20 components is more reliable (see Fig. 10) in the revised manuscript. On the other hand, the number of components we need to estimate given percentage of variability is still high (even higher than before, Fig. 9), suggesting high variability of the spectra.

Section 3.3 was modified to account for the changes in the plots with the change to log(N+1).

Line 303: "unique methodology". Does this mean "original"? Could the word "unique" be replaced by something more descriptive?

Replaced with “novel”.

Figs. 5, 6 and 8: What are the units of the colour scales on these figures, if any?

These figures are dimensionless, as we are analysing the logarithm of a quantity (wave action density).

Caption of Fig. 6, line 1: "Principal components 0, 1, 2 (columns)". Does this mean each of the PCs in isolation, or 0, 0+1 and 0+1+2 ? Please clarify.

Thank you for pointing out the unclarity. Indeed, the PCs in isolation were meant. Word “Individual” added to the figure caption.

Caption of Fig. 6, line 2: "which is notable". Do you mean "noticeable" instead, or "remarkable"? If the latter, in what sense?

Yes, thanks for the remark, modified to “noticeable”.

Lines 320-321: "since we were analysing data at a single altitude only, we do not have the spectra in the wavenumber klm space". This seems to be contradicted by Figs. 5-6. Probably the authors mean something different. If so, please phrase this passage more clearly.

Rephrased to: “Second, since we were analysing data at a single altitude only, we do not get the spectra in the wavenumber klm space directly from the GW projection as in Borchert et al. (2014), but (...)”

Lines 325-326: "the approach is relatively reliable". What term of comparison (truth) or measure of reliability do you have? If none, then this claim seems unjustified.

The statement refers to the comparison between scale separation and GW projection presented in Fig. 3b (old figure number). That is, for the truth in this statement, we take the GW projection relying on the linear GW theory. Based on your comment, we have reformulated the sentence to:

“We observed that the scale-based filtering results in similar GW signature, with an overestimation of GWs near the subtropical jet streams.”

Caption of Fig. 8, line 1: "first column". Since earlier in the same line you mention "right column", it would be better to call this "left column" instead.

Thank you for the suggestion, it was changed to first, second and third column.

Line 336: "with a novel methodology". If this methodology is indeed novel, that should be emphasized earlier in the paper, say in the introduction or so.

Thanks for pointing out this was not stressed before. Although the methods already existed in some form (e. g., GW projection in Boussinesque approximation and in klm space), it is the first time it has been used in a more general setting and combined with the Fourier transform on triangular grid.

We repeated that the methods are novel in the introduction.

Line 344: "gravity and sound waves". As previously, would not these waves be better described as "inertia-gravity-acoustic waves"?

Modified here and after Eq. A12.

Line 346: Eq. (A1). The appearance of the speed of sound c_s in this equation suggests that this equation set is subject to some kind of scaling, which is omitted. Either this should be described here (an appendix) or referred to a publication where it is explained.

The scaling is described in the book referenced for the equations (Achatz, 2022). We made the reference more specific to make it clearer. Although it would be more transparent if the full derivation of Eqs. A1-A5 was in the paper, it is quite lengthy for the publication.

Line 349: Eq. (A4). This equation expresses adiabatic flow. Yet, I do not think this assumption is mentioned anywhere. Please mention it explicitly.

Thank you for the comment, it is indeed assumed that the GW perturbations are adiabatic. We added the assumption to the beginning of Attachment A.

Line 360: "it holds". Remove these words, as they are unnecessary.

Removed.

Line 365: Eq. (A12). I think k_h needs to be defined here, for clarity.

Thanks, definition was added.

Line 379: "For sufficiently large ..., this translates to ...". Does this approximation automatically exclude sound waves? If so, explain why/how.

The sound waves are excluded by selecting the negative sign in Eq. B1. The approximation is only necessary for using Taylor expansion on the square-root in the equation to arrive at the simplified form (would be necessary even for derivation of simplified dispersion relation for sound waves). We reformulated the text to mention the selected sign of the square-root term:

“For sufficiently large ..., the previous equation with the negative sign before the square root translates to the standard dispersion relation for GWs ... Equation (B1) with the positive sign describes the sound waves.”

Line 383: "minimal intrinsic frequencies can be obtained by maximizing the vertical wavenumber". It is not obvious to me why. Please explain how it works.

This follows from the fact that the intrinsic frequency decreases with increasing m^2 (for $k_h^2 + m^2 + 1/4H^2$ large enough, it can be obtained by differentiation of Eq. B2). Its minimum is thus for the largest m^2. The text was modified to clarify this by adding "Since the intrinsic frequency decreases with the increasing size of vertical wavenumber, the minimum can be easily obtained ..."

Line 386: "the maximum can be reached by taking the limit m^2 -> 0". Again, it is not obvious why. More details are necessary.

As in the previous answer, this follows from the dependency between the frequency and vertical wavenumber (although we are taking small m^2, it can still be assumed for this dependency that $k_h^2 + m^2 + 1/4H^2$ is large compared to f^2/c_s^2). The maximal intrinsic frequency is thus obtained by taking minimal m^2, as it is a decreasing function.

Line 433: The page number range of Fritts and Alexander (2003) seems to be missing.

Article ID within the volume was added.

Line 477: The page number range of Stephan et al. (2022) seems to be missing.

Page number was added.