First, we would like to thank both the referees and the editor Prof. John Plane for facilitating the review process. The answers to the comments of both referees, together with the listed changes of the manuscript, can be found below.

Reviewer 1

The paper is a very thorough introduction into the application of spectral analysis to the icosahedric grid of the ICON model avoiding spurious effects necessarily generated by interpolating the data on a rectangular grid first. The data gives some convincing examples of the application. I feel, however, that the paper should be strengthened in the intuitive physical interpretation. Also, when it comes to discussion of global distributions there is a wealth of previous work from both models and satellite observations which you can compare against. This shouldn't be lengthy, but also not completely ignorant. I am wondering that you are not considering phase speed spectra and would suggest this. The paper is generally well written and of high presentation quality, thus strongly recommended for publication as soon as these points raised have been taken into account.

We would like to thank the reviewer very much for their constructive review and helpful suggestions. Please find the answers to individual comments below.

General comment:

The aim of the paper is, I think, to motivate other scientists working with ICON to use your tool to analyze ICON data. This means, you are aiming to a large fraction to GW physicists and people interpreting models. I think a few minor modifications (as indicated below) would help them to better understand what you propose. The paper contains no introduction to the code. I hope the tool is reasonably self explaining, otherwise I am voting for an appendix C with ashort technical introduction.

We agree with the reviewer that some documentation to the code can be useful and we will post it in the same repository where the code will be uploaded. However, since that is very technical, we would prefer not to include it in the paper.

Specific comments:

L51 With a horizontal resolution of 2.5km you are expected to reasonably represent wavelengths of 25km and longer, which would correspond to GWs emitted from a single convective tower (though that would of course not be really resolved). You would then expect 20min the dominant period (works of Fovell and Lane), i.e. you are just at the Nyquist limit -> direction flips to be expected. For introducing a new method, that's all fine, but from a physical point of view I think one should do 5min time step.

Thank you for the reminder, we agree that shorter timestep would be better. Unfortunately, having the data from global high-resolution model at 5-minute interval would be very demanding on the disk space and it was not possible for the current simulation. We added following sentences to the discussion to acknowledge this:

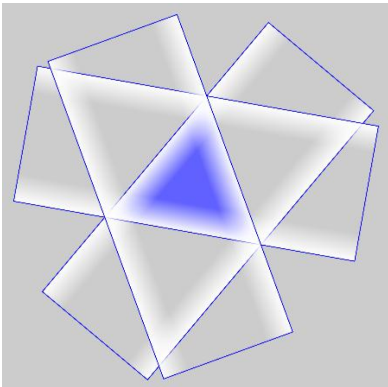
“A limitation of the procedure is the necessity for using data with a short output timestep, increasing the disk-space requirements. The 10-minute output frequency applied in our case is at the limit of what is needed to capture the dominant period of convective GWs (Lane et al., 2001).”

L55 Only a comment, no action needed: Just to remember that this is only an estimate, since the interaction of scales would be missing in a 160km run.

Thank you, this is indeed true. Not to omit this information in the manuscript, “excluding the interaction of scales” was added to the text.

L67 A picture illustrating this would be really nice. If you really care it takes minutes to understand these three senetences.

The following figure was added to the manuscript, thank you for the suggestion.



L90 Perhaps one sentence why, in contrast to a FFT on a regular rectangular grid you need a regularization?

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

L104 Why do you want a symmetric range? At some point you can resolve ambiguities by GW polarization relations. If that is the motivation, include here? For instance: The symmetric range is later used in Section … to …

Added “to simplify interpretation of the results”.

L107 equidistant in a regular x-y grid   I think you need to say this, because the sides of the triangles are of equal length -> equidistant

Thanks, modified according to your suggestion. The sides of the triangles are nevertheless not completely the same – in ICON, an algorithm modifying the precise location of the vertices to improve numerical properties is run after constructing a first triangular mesh.

L116 For comparison with other methods and observations it would be interesting to do temp as well, in particular as b includes a vertical derivative which introduces new sources of uncertainty

In our method, buoyancy is computed for the GW projection only. After the projection, we can get back to temperature using GW polarization relations – in any case, only the spectrum of zonal velocity is stored after the projection. We added text “which will be needed for the next steps” after listing the computed variables.

Regarding the comparison of the global distributions with satellite observation, this is quite problematic since we have data from the altitude of 15 km only, where the satellite observations are not so reliable. We are unaware of data that could be used for comparison with the global distributions at this height. However, we included a study of the slopes of the spectra, which is something that can be easily compared with the literature, in a new Appendix C.

L121 Do you see infrasound waves in ICON? If yes then the 10min sampling become really questionable

Sound waves are dampened by ICON and should not be present in the data. We are, however, taking a more general dispersion relation first to keep it as general as possible. With the 10-minute sampling, we are indeed not able to see any sound waves (i.e., the dispersion relation for the sound waves does not have a real solution for vertical wavenumber with our range of horizontal wavenumbers and frequency).

L139 I think at this point you need to explain a bit more. In which way are the polarization relations a vector? And what are you gaining by the projection in a physical sense. A few additional senetnces should help. Please keep in mind that you need to keep the average atmospheric scientist in the boat, if you want your method to be applied.

Thanks for pointing out the unclear paragraph. We added a mathematical definition of the vector, expressed the decomposition in terms of it and slightly expanded the comment around it.

L146 O.k., here is the why. Still I don't see how the projection does it.

We hope this will be more understandable with the text changes around the projection.

L151 in Appendix B from the extremal vertical wavenumber values m->inf and m=0, respectively.

Thank you for the suggestion, the text was modified accordingly.

L154 I don't think the model can really simulate waves at its vertical Nyquist. Should be a safeguard only?

Yes, this is to ensure that "waves" that cannot be represented by the model are not admitted for the analysis, as the methodology can in principle result also in such vertical wavenumbers. To make it clearer, we reformulated the text to:

This ensures that we do not include in the analysis wavelengths that cannot be resolved by the model, although they can be obtained from the dispersion relation.

L163 the wave … generates -> is generated   also not fully satisfactory

Modified to "is being generated".

L168 arbitrary, exemplarily ? Or did you really use a (computer) dice? And why not use something where other studies exist?

As for the Kanin peninsula subdomain, the selection was done by selecting a triangle index (without a dice, but also without knowing the location and the spectra in the triangle beforehand) and checking that it is indeed a subdomain without significant orography. As for the Scandinavia subdomain, we have selected the mountain range to show a subdomain where there are clearly orographic gravity waves but it is still not a relatively rare hotspot with extremely strong waves (such as Southern Andes or Himalayas). In any case, we replaced the word "randomly" by "arbitrarily", thanks for the suggestion.

F1 Lucid writing rules: same colors for same things, different colors for different things. There are more color scales than viridis. Confirming is a bit strong.

We have changed the colormap of Figs. 1c and 2c to distinguish between the horizontal and 3D spectra. As for Figs. e and f, we think keeping the same colormap is necessary so that they can be easily compared. The word "confirming" was replaced by "indicating".

In the explanation you discuss the limits by f, N creeps in via changes in color between e and f (correct?), the extent of the k scale is Nyquist. In principle k=0 is part of the Fourier grid, but you cut to the domain size, so the white stripe in the middle (?). That this is naturally not very populated (e) is from the tapering? An additional background removal? Because b is at a mean value of 11m/s.

The change of color is caused by the GW projection. The limit by f is visualised by the missing values between the two slanted lines, the upper limit on intrinsic frequency is considered mostly for safety reasons but it is mostly out of the range of our data (it is not visible in the cases shown). The white stripe at k=0 follows from the GW dispersion relation: When $k_h^2 = 0$ is substituted into the dispersion relation (Eq. 11 in the revised manuscript), the radicand becomes negative, resulting in no solution for the GW vertical wavenumber, which means that there are no GWs in this region. The low values in the middle stripe of fig. (e) are caused by the deplaning procedure applied before the spectrum computation.

We made the following changes in text:

- Added sentence "The low values for $\mathrm{k} = \mathrm{l} = 0$ are caused by the deplaning of the data." to the discussion of Fig. 2e.
- As an introduction to Fig. 2f, we added the text: "The 3D spectrum is then projected to GW modes using the projection described in Eq. (17) and limited to the range of intrinsic frequencies described by Eqs. (18) and (19)."
- We linked to Eq. (18) when describing the limit by f.
- Explanation for white vertical band in Fig. 2f expanded to "Another region of missing values in the projected spectrum is the column of $\mathrm{k} = \mathrm{l} = 0$, which also does not describe GWs, as the dispersion relation for these wavenumbers does not result in a real vertical wavenumber."

L187 This irritates me slightly. Physically, time is the dimension which runs only in one direction, so you should break symmetry here (same as nature). m>0 should be downward propagating, m<0 upward propagating waves and it should make sense to compare both of them in order to search for reflections and middle atmosphere sources. Mathematically, you can break of course as you like, but I am nota able to send my sensor backward in time. At least mention that you have both possibilities and do a choice here which makes it easier to discuss some features.

Thank you for the comment. We understand the reasoning for positive frequency. The relationship to the direction of propagation would be however more complicated due to the difference between the intrinsic and extrinsic frequency. If we took extrinsic frequency >0, which is the natural situation in physics, we would still need to divide the spectrum to parts with intrinsic frequency >0 and <0 to differentiate between upward and downward propagating waves for both m>0 and m<0. We would thus prefer keeping the original notation. Nevertheless, as you suggest, we added the following text after the discussion of the symmetry of the spectra in Section 3.1:

The representation by positive m and both signs of frequency and horizontal wavenumber is chosen to simplify the discussion of the results. Other representations, such as using positive frequency and both signs of the wavenumbers, carry the same information and it is possible to switch to them using the symmetry of the spectra.

L191 This is a bit simpler than the limit you have stated above (Equ 20). At one point you should include, how the two relate to each other. I would suggest after equ 20 something for GWs with such and such properties these become the well known limits …

We removed the upper limit at this line as it is in any case irrelevant to the statement and the following text was added to the end of Appendix B:

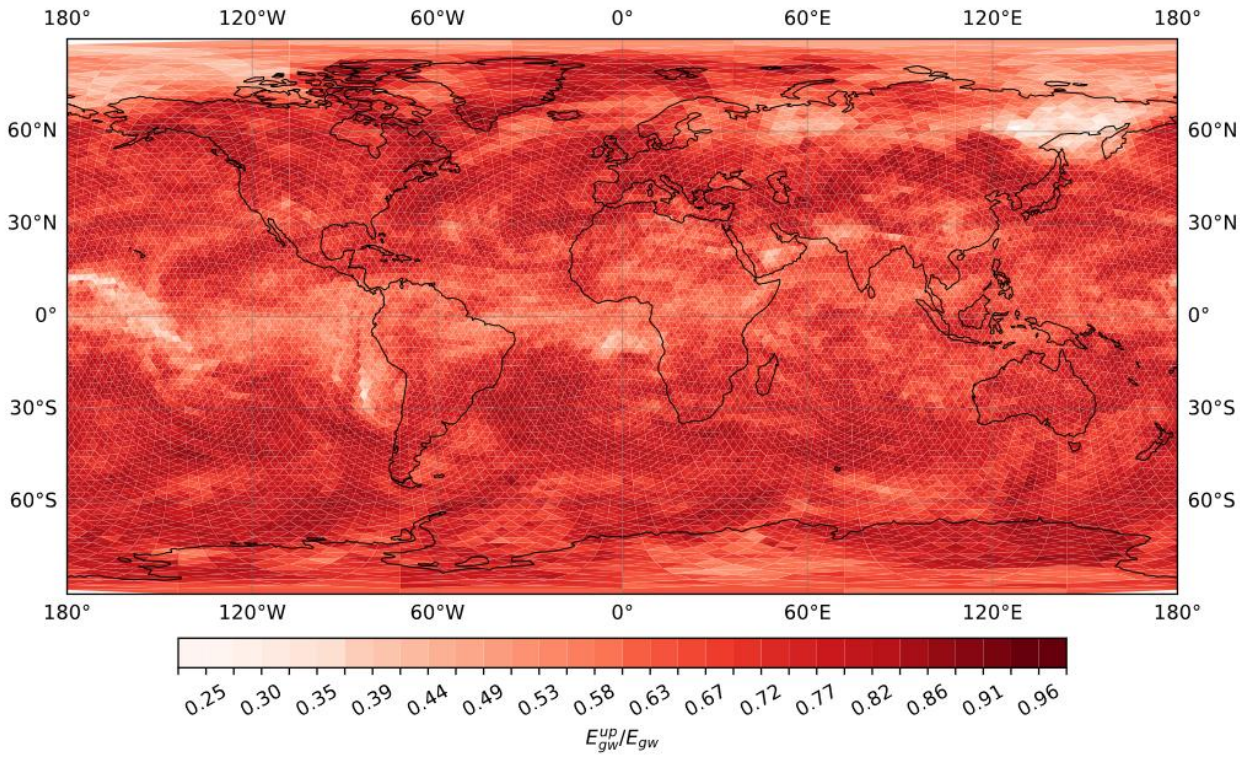
"(…), with the upper limit simplifying to N^2 for non-inertia GWs with large horizontal wavenumbers."

L197 58% is surprisingly symmetric.

Thank you for the comment. The 58 % is indeed quite symmetric. It is nevertheless a value for a specific subdomain. In general, most subdomains at the studied level are dominated by upward propagating waves. To clarify this, we stressed in the case-study Section 3.1 that it is a local value and added a figure showing the percentage of upward propagating waves to Section 3.2 (figure is also shown below). The following text was added to describe the figure:

As shown in Section 3.1, the GW projection method also allows us to distinguish between upward and downward propagating waves. For the day shown in Fig. 4a, the fraction of total energy of the upward propagating waves is visualized in Fig. 5. At most places, 70 – 80% of the wave energy propagates upward, highlighting the importance of tropospheric wave sources. A lower percentage is present in two regions, where the values decrease even below 50%. First, the fraction is generally lower above the tropical Pacific and Atlantic ocean. This is likely connected to the fact that the tropopause is in the tropics higher than the studied altitude and the spectrum can therefore be dominated by convective GWs coming from above. Second, the figure shows significant downward propagating waves at the northern-hemispheric polar regions, especially above the Chukotka region, possibly originating at the edge of the polar vortex (Vadas et al., 2023) in the absence of strong tropospheric sources.

Vadas, S. L., Becker, E., Bossert, K., Baumgarten, G., Hoffmann, L., & Harvey, V. L. (2023). Secondary gravity waves from the stratospheric polar vortex over ALOMAR observatory on 12–14 January 2016: Observations and modeling. *Journal of Geophysical Research: Atmospheres*, *128*(2), e2022JD036985.



L203 For me I would again discuss along the physical case. Scandinavia is known to excite mountain waves. (I find the suitable… a bit awkward). Accordingly, we find a predominance of waves with ground-based frequencies close to zero. There obviously is a stronger dominance of upward propagating waves with a well defined source below. That the ridge is N-S and thus most power is in k you don't discuss in the spectra, maybe omit? What you mean by circular I don't understand. Explain or omit.

The introductory sentence was reformulated to "This subdomain lies on the Scandinavian Peninsula at 62°N and 7°E, which is known for exciting orographic GWs and thus provides a potentially more complex wave field."

The sentence specifying the exact direction of the ridge was removed, although we keep the more general sentence "As in the previous case, the direction of the perturbations is visible in the directionality of the horizontal spectrum in Fig. 3c."

The wording “not as strongly circular” changed to “more anisotropic”.

L217 Why "Although"? Mission A accomplished now comes mission B. We now extent our analysis to the global scale … or something like that

Thank you for the suggestion, modified to:

“The GW spectra presented in the previous section are informative about the wave type in individual subdomains. Now, we will extend our analysis to the global imprint of GWs.”

L220 e.g. Andes or Himalayas -> These are the two you would not necessarily expect and which will disappear when you move a little higher in analysis altitude. (Andes -> summer, Himalaya a general wind shear). Not against quoting them, but I would do a few more e.g. Delany, Rocky Mountains, Greenland, Iceland, Mongolia, Catabatic winds at Antarctica. Interestingly, at this particular day, not so much Scandinavia. In addition to this you see the subtropical convection on the SH, without pointing at special locations, should be mentioned.

The list of the orographic regions was extended and the subtropical region added, thank you for the suggestion. The modified sentence is now:

The most significant areas with high energy are the orographic regions (e.g., Andes, Rocky Mountains, Alaska Range, Greenland, Island or Himalayas) and the regions of the Southern Hemisphere connected to the subtropical convection.

F3 Please do not use judgemental language: simple -> only How good or bad the removal works will depend a lot on the details of the removal.

Thanks for the comment. Our aim here was to contrast our specific method that uses the scale filtering (due to the size of the subdomains) and an additional filtering (GW projection) with the results we could get by using *just* the scale filtering. We removed the word “simple”, since it seems to be rather confusing in this context.

L235 What is a non-linear GW? Do they really exist as a pure form or are GWs rather usually well described but may deviate somewhat from linearity? In particular, you would then expect deviations from the quadrature phase relations between temperatures and winds. which indead would reduce the projected energy in the cospectra. Please rethink the formulation.

“nonlinear GWs” changed to “nonlinearities”

L238 Why Finally ?

The word “Finally” was removed.

L242 ITCZ -> this would be the small peak around 10S on top of a wider GW maimum form 10N to 25S. Convection about subtropical sources is also known.

Thank you for the correction, text modified to ". The local maximum of the GW energy is therefore most likely linked to the convectively generated GWs above the subtropics and near the Intertropical Convergence Zone (ITCZ)."

L248 … are chosen narrower … Or did you really make them conatining equla amounts of cells?

The bands are indeed chosen to contain equal amounts of cells. To avoid confusion, we have changed word "domains" in the sentence "the domains closer to the equator are narrower compared to the domains closer to the poles" to "groups" to refer to the 10 groups containing 2048 subdomains mentioned in the previous sentence.

3.3 in general:

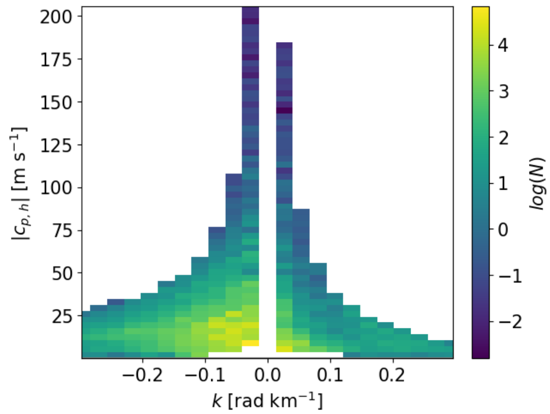
For me it would now be logical to switch to spectra in terms of phase speed and direction. This helps you very intuitively to see different wave sources and wind filtering - quite a bit of work out in the literature. The vertical wavelength then mixes the wind and the source effect and the zonal means and the variations of winds with longitude is not really helping.

Thank you for the suggestion. Nevertheless, we have finally decided not to include it for two reasons:

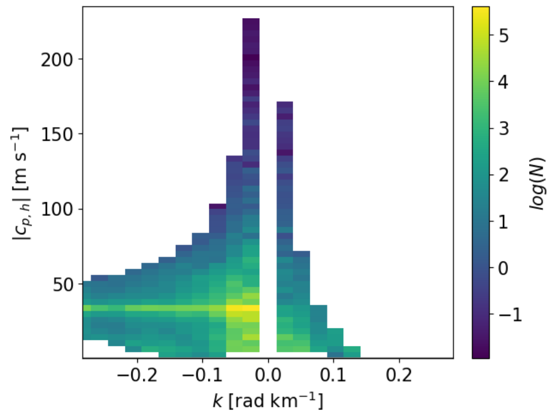
1) Since the primary goal of the work is to estimate input spectra for the GW parametrisation, our analysis needs to provide information in the form compatible with the parametrisation. This is, at least for MS-GWaM (Voelker et al., 2023), wave action density on the *(k,l,m)* grid.
2) The phase-speed spectra can be easily considered for individual subdomains (below, see wave action density spectra for l=0 for the two examples shown in the manuscript as a case study). Analysis of the spectra for larger number of subdomains would however be complicated since the canonical shape of the spectrum (e.g., Eq. 3 of Orr et al., 2010) depends on wind and would thus be different for each subdomain.

Voelker, G. S., Bölöni, G., Kim, Y. H., Zängl, G., & Achatz, U. (2024). MS-GWaM: A three-dimensional transient gravity wave parametrization for atmospheric models. *Journal of the Atmospheric Sciences*, *81*(7), 1181-1200.

Orr, A., Bechtold, P., Scinocca, J., Ern, M., & Janiskova, M. (2010). Improved middle atmosphere climate and forecasts in the ECMWF model through a nonorographic gravity wave drag parameterization. *Journal of Climate*, *23*(22), 5905-5926.



Subdomain near Kanin Peninsula



Subdomain at Scandinavian Peninsula

The PCA on the other hand is not motivated. Why do you want to do this? What does the modes tell us? Wouldn't it be better to perform PCA on phase-speed spectra? And how would results change if you go to a different day, e.g. in March?

Thank you for pointing out the missing motivation. Generally, our motivation is to see how variable the GW spectra are and if we can reduce the dimensionality of the spectra to be able to predict the spectra based on the large-scale flow by machine learning methods.

We made the following changes in the manuscript that should increase the clarity:

- The motivation in the introduction was reformulated.
- The introduction of Section 3.3 was slightly modified ("which is a key factor in assessing how simplified a GW parametrisation can be" added) and the motivation for questions asked within the section was stated ("The first question gives an estimate on how the spectra launched by GW parametrisations should look like for different latitudes. The second question assesses the spatial variability of the spectra within the latitude ranges and the possibility to compress the spectra by PCA, so that the parametrisations can more easily encompass this variability.").
- To the discussion section, a paragraph mentioning the limitation of analysing one week only was added: "A certain limitation of this study is the restriction of the analysis to a single week due to computational limits. Thanks to the high number of subdomains with various atmospheric conditions in both hemispheres, we can

however consider our results to be representative when related, for example, to the zonal wind.”

L271 retrograde -> generally opposite ?

Thank you for the suggestion, changed accordingly.

L303 GW filtering -> this is for me associated with (critical level) wind filtering. You mean the projection/identification/extraction … still not the right word, but come up with something more positive saying afterwards we have the GW only

Changed to “separation”.

L305 flow -> dynamics ? Its not only u and v

Thank you, the text was changed accordingly.

L306 This is here not connected and would need to be introduced in the discussion of the model setup. You may also overestimate some scales.

Thank you for the comment. The text was moved to the section where the simulation is introduced.

L311 You cannot expect the readers to know the Chew paper. Should be introduced in the intro or really discussed in the math part, if you want to delineate your method from others.

The aim here was to discuss the error we make by using the Fourier fitting method rather than compare the methods. We reformulated the text to make this clearer.

The new text is:

Although similar fitting procedures were used in previous studies (VanderPlas and Ivezic, 2015), Chew et al. (2024) observed that such methods introduce errors in the amplitudes. However, since atmospheric quantities have mostly power-law spectra and we focus on small wavenumbers, our approach resembles a more precise method introduced by Chew et al. (2024, Constrained Spectral Approximation method), where the procedure is iterated for the wavenumbers with the largest amplitudes, which reduces the error in our results.

L323 I think if you have the input data, i.e. model output at high frequency its better to use frequency rather than m as it is less subject to changes by e.g. refraction. However, of course, when you have it is a strong constraint as you need to foresee the application when you do the model run and you can apply it to short runs only. A fair one sentence statement would be good to make.

For dedicated experiments which are saved at short time steps a k,l,frequency analysis is the better choice ...

Thank you, the following sentence, as you suggest, was added to the text:

For dedicated experiments which are saved at short time steps, a k,l, ω analysis is the better choice as the frequency is less subject to changes by, e.g., refraction compared to the vertical wavenumber.

L324 Please have a look at Strube et al as well. This paper discusses an altitude dependency of how well this may work. (DOI: {10.5194/amt-13-4927-2020})

Thank you for suggesting the paper. The following sentence was added to the discussion section:

"The effectiveness of a horizontal filtering of GWs is in accordance with the work of Strube et al. (2020) for higher altitudes."

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 $\lambda\left\|\hat{f}\right\|^2$ 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.