Second Review

Only after reviewing a second time the manuscript entitled "**Implementation of a Multi-resolution Analysis Method to Characterize Multi-Scale Wave Structures in Lidar Data**" by Tremoulu et al., I came to an understanding of the novel aspects of this work. The authors use the variance method by Mze et al. (2014) as a benchmark to derive GWPE based on lidar temperature measurements and found that the usage of details 4 and 5 from a DWT – using the 8 th order Daubechies wavelet – captures best the wave energy in comparison to the variance method. They use this knowledge to argue that their MRA method can further be used to determine GWKE based on co-located simultaneous lidar wind measurement.

The topic is timely, the methodology has potential to advance GW diagnostics from lidar observations, and the demonstration of scale-specific energy distributions is valuable. However, the manuscript needs some serious improvement in writing style and several important methodological clarifications and robustness checks are needed before publication. I therefore, still, recommend **major revisions**.

Please have a close look at my comments and questions below.

Abstract

You state that the variability of background removal techniques complicates the intercomparison of GWs. To address this issue you're proposing yet another background removal technique. I would suggest to rephrase that and pinpoint to other deficiencies of the background removal techniques.

The background removal is one aspect of multiresolution analysis but the main advantage of the method is the decomposition of our signal (temperature or wind from lidar measurements) into corresponding to different bandwidth. Moreover, the MRA method has the advantage to decompose identically a temperature signal either it is lidar or RS data (the filtering of GW in the different database has to be taken into account).

Line 8: Since you're not putting it into context I would suggest to cancel "intense" and rather say "[…] and applied to lidar temperature and wind measurements on the night of 20 November 2023 at La Réunion."
Done

Line 8-10: I would suggest to write "By decomposing the signal into dyadic vertical wavelength bands and an appropriate choice of corresponding details, the MRA can improve the detection of GW-induced perturbations in the spectral range of 0.8km to 12.8km vertical wavelength by simultaneous background removal and denoising."
Done

Line 11: I would suggest to write "We use the variance method described in Mze et al. (2014) as benchmark for determining GWPE and ask the question: "How well do the different filtering techniques compared with the variance method?" Given an overall agreement between our novel

MRA and the variance method, we conclude that the MRA can also be used to determine reliable GWKE."

Done

Line 11-15: I suggest to erase these lines.

Done

Line 16: Since you're arguing that the MRA provides tunable vertical wavelength bands you must conduct a sensitivity study to show the effect of e.g. the wavelet's order and vertical resolution on GWPE and GWKE profiles. It is sufficient to add this sensitivity study to the appendix.

Introduction

Line 32: Why are you emphasizing propagation in the vertical? I suggest to erase "vertically propagating" to keep the statement more general.

Done

Line 35: I would appreciate if you could add Reichert et al. (2021) to the long list.

Done

Line 37: Erase "dynamical" and "excellent". Make it more concrete e.g. "[…] with temporal and vertical resolution in the order of 1min and 100m respectively."

Done

Line 45: Not entirely sure what you mean by "fluctuating dynamical component". Do you mean "fluctuating atmospheric variable – such as temperature and wind – "?

Done

Line 46: Be specific. Give me a number. What do you mean by "large-scale"?

Large-scale is a reference to larger scale perturbations in the atmosphere such as tides or planetary waves.

The sentence has been changed to : "However, a major challenge lies in effectively distinguishing GWs from larger-scale perturbations such as tides or planetary waves."

Line 48: Erase "by removing the background signal."

Done

Line 54: That is not entirely true. Mountain waves can have small horizontal scales in the order of 20km and are stationary, i.e. have low frequencies. Please rephrase.

We agree on this point. Throughout the entire article, we discuss the vertical wave component. The method lies on determining the GWs vertical wavelength, and when it's possible to compute the horizontal wavelength.

The sentence has been changed to :" For GW analysis, high-pass filters are typically applied in the height domain to eliminate tidal or large-scales waves influences. "

Line 59: GWPE was so far only mentioned in the abstract. Please write it out here again.
Done

Line 61: New line after the Ehard reference.
Done

Line 64: I would appreciate a reference to Reichert et al. (2024) and Ungermann and Reichert (2025).
Done

Line 82: Instead of "GW energy" it should be "GWPE" since the variance method only applies for GWPE and not for GWKE.
Done

Line 85-86: I suggest to erase "The comparison focuses on key GWs properties, including vertical wavelength content, localization with height and time, and associated GWs energy." and write: "This ability is quantified by deriving profiles of GWPE which are compared to GWPE profiles based on the variance method by Mzé et al. (2014). For this analysis, we use lidar temperature measurements from one night in November 2023 at La Réunion. How well does GWPE based on our novel MRA method agree with the established variance method? In a subsequent step, we also compare GWKE profiles based on co-located lidar wind measurements."
Done

Line 88: What do you mean by "exact energy conservation"? Energy is either conserved or not. Please erase "exact" here.
Done

Line 89: I dislike "continuous" in this context, because you are using the discrete wavelet transform and not the continuous one. Please erase.
Done

Line 90: What do you mean by "traditional filter approaches treat each scale in isolation"? The filters you are using are all pretty broad and divide the signal in – if you want – two scales: small and large. Please rephrase.
We wanted to point out the way traditional approaches filter out the GWs. Traditional approaches use high-pass or low-pass filters (one cutoff wavelength) or band-pass (2 cutoff wavelengths). On the other side, the MRA provides a multi-scale decomposition (a n number of consecutive band-passes at each step of the decomposition).
The sentence has been changed to :"This capability enables the investigation of interactions between wave components of different scales, which is not possible with conventional filtering approaches."

Line 92: Please erase "All four methods are applied to the same lidar temperature and wind datasets" If you accept my suggestion from above, this is already explained.

Done

Line 92-94: Please erase. This statement is unclear and not helpful here.
Done

Line 94-96: Please write: "Our work aims to analyze how each filtering technique impacts GW energy profiles and highlights how MRA's multi-scale, energy-conserving framework can complement existing techniques for characterizing atmospheric GWs.
Done

Materials and Methods
Lidar Data

Line 107: It's a bit weird to read "propagating" waves, because every wave is propagating unless it's evanescent. Maybe you can specify the waves you're talking about.
Done

Line 108: Please exchange "produced" by "measured".
Done

Line 130: I don't understand how the data is resampled from 150m resolution to 100m resolution. Does that mean, you're interpolating the data? Also, with the mentioned resolutions, I would expect to be sensitive to waves with vertical wavelengths >300m and periods >30min, why are you using 1km and 1hour instead?
The data is oversampled so that we can use the same specific bandwidth when applying the MRA. We interpolate the data using the spline method.
We agree that, from a theoretical perspective, a vertical resolution of 150 m combined with 15-min integrated temperature profiles would suggest sensitivity to gravity waves with vertical wavelengths greater than 300 m and periods longer than 30 min. However, as discussed by Chane Ming et al. (2023), the effective range of detectable wavelengths and periods is strongly influenced by the signal-to-noise ratio. In practice, vertical scales shorter than 1 km—primarily corresponding to the d_1 and d_2 decomposition levels—are dominated by photon-counting noise, particularly in the upper atmosphere above 60 km. The resampling of the data to a 100 m vertical grid does not introduce additional information but rather smooths the signal. Consequently, a lower cutoff at 1 km was applied to ensure that the analysis is not contaminated by noise.

In addition, with a 15-min temporal integration, a gravity wave with a 30-min period would be sampled by only two data points. Such temporal sampling is insufficient to reliably characterize a wave in terms of both phase and amplitude. A minimum of four to five sampling points per wave period is required to achieve a meaningful characterization.

Since noise is subject to this work, please mention the noise amplitude in your lidar temperature and wind measurements.

Temperature measurements have white noise with an amplitude that follows the conservative growth.

This would be actually a good spot to show the measurements (maybe even overlay contours from ERA5) and show the reader the 5km-wave that you are focusing on later.
Figure 1 has been added to provide a visual representation of the temperature and wind measurements, with emphasis on the gravity wave structure analyzed in the manuscript.

GW Analysis techniques
The time averaged background profiles method
Line 136-138: I suggest to rephrase: "This approach assumes that observed periods of GWs are shorter than the measurement period such that phase averaging cancels the GW signal and the nightly mean profile can be assumed to define a thermal background."
Done

Line 138: What do you mean by "spectral bands of GWs are difficult to define"?
Here, the 7.5km Hanning window filters waves with vertical wavelengths greater than 7.5 km. In this context, the filtering of the nightly mean profile as the background to extract gravity waves is hard to handle. Moreover, the assumption can be misleading with waves with periods greater than the measurement period.

Line 138-141: Please erase "Alternatively, […] spectrum". It's irrelevant.
Done

Line 141: Please write: "[…] profile is smoothed with a 7.5km Hanning window." Please motivate the window size.
Done, motivations of the window size.

The sliding polynomial fit Method
Line 144: Include a proper reference: Savitzky and Golay (1964)
Done

Line 153: I'd suggest to say "[…] perturbations with vertical wavelengths >10km are strongly suppressed […]" because the filter transfer function is not entirely zero at 10km.
Done

The spectral filtering method

Line 155-156: I'd suggest to say: "[…] method to separate GWs from a thermal background by choosing the right cutoff wavelength."
Done

Line 156-157: Low frequency does not necessarily mean large-scale. Stationary mountain waves have a frequency of zero but can be of small horizontal scale! Please adjust.
Here, the low frequency component means a large vertical scale. The article is about the vertical

component of GWs. We agree that low frequency does not necessarily mean a large horizontal scale, however by looking at the vertical scale.

Line 165: The 5km-wave falls from the sky here. You never mentioned that one before. As suggested above, I recommend to show the actual measurements in Section 2.1 and show the reader the 5km-wave. Then you can reference to it here.
As answered above, Figure 1 has been added to provide a visual representation of the temperature and wind measurements, with emphasis on the gravity wave structure analyzed in the manuscript.

Table 1: As questioned above, why do you state >1h instead of >30min?
Please refer to section Lidar Data line 130 response.

The multi-resolution analysis method
Line 215-216: "characterize" is too inconcrete. Just say "[…] passbands are used to derive the GWPE and GWKE of the dominant 5km wave." At this point, I still wonder, why haven't you adjusted the passbands at least on the low-frequency end so that they agree better with the spectral limits of the variance method? The Hanning window could be set to 6km. The cutoff wavelength of the Butterworth filter could be set to 6km.
We changed "characterize" by "[...] passbands are used to derive the GWPE and GWKE of the dominant 5 km wave [...]"
As we said in the article, the passbands depend on the vertical resolution of our data. We can adjust the passbands on the low-frequency end however on the high frequency end, it won't be adjusted.

Equation 6: Please add parenthesis.
Done

Line 234-236: Please explain again, how you did the oversampling? Is it just linear interpolation from 150m to 100m resolution? If yes, the interpolation has a smoothing effect and the spectral power in this oversampled range does not reflect true noise amplitudes. For lidar temperature measurements Hauchecorne and Chanin (1980) describe how to determine temperature uncertainties. Did you check whether the noise as you define it and the one from Hauchecorne and Chanin (1980) agree? For wind measurement I'd suggest to infer instrument noise from another measurement under quiet atmospheric conditions.
We used spline interpolation from 150m to 100m. The noise as we define in the article was constructed with Alain Hauchecorne. It is an equivalent way to consider noise in temperature measurement.

Line 247: What do you mean by "primary propagation characteristics"? Please specify.
The sentence has been changed to :"This process enables the estimation of the average propagation of GWs during the night"

Line 252: How do you mitigate edge effects? Please describe.

To mitigate the edge effects, we apply the MRA to the complete range of the data (20 to 80 km for temperature data for instance), then we reduce the vertical range to 30 - 70 km. It mitigates the edge effects, the effects of the aerosol layer underneath 30 km and above 70 km the noise effects.

Comparison of the different methods
Case study: 20 November 2023

Line 263-265: Please erase "During […] November 2023), and ". Your work is of methodological nature and the source of the observed GW is not of importance.

Done

Line 268-269: Please erase "Vertical […] (not shown)" OR show the wind profiles in the appendix. Later you show that the lidar and ERA5 temperatures are off by 10K at 30km altitude. This is a huge discrepancy! So it would be of interest how well ERA5 and radiosonde winds actually agree.

We add to the appendix a comparison between the ERA 5 and Radiosonde Wind profiles. Showing a good agreement between the two datasets.

Figure 2: Please mention how ERA5 fields such as temperature and wind are filtered.

These perturbations are extracted by applying multiresolution analysis (MRA) to the wind and temperature data and retaining the d_5 detail component.
We mention how ERA 5 fields are filtered in the figure legend.

Application to lidar temperature profiles

Line 283: Please erase "applied to […] November".

Done

Line 284: The way you phrase this gives me the impression that your aim is to analyze this 5km wave that is present in ERA5 and your observations. However, you're presenting a method that requires to be independent from a priori knowledge of the underlying signal. So, please rephrase this.

We pointed out in the previous section that a dominant mode of 5 km vertical wavelength of GW is present, and this is the mode that we want to focus on in this section.

Line 287: I mentioned that earlier. Marlton et al. (2021) have shown that temperature biases between lidar and ERA5 at Hawaii (also tropical) are in the range of plus minus 1K at 30km altitude. A bias of 10K in this altitude would be about 2-3 standard deviations larger than what is reported in Marlton et al. (2021). Please comment on that.

We thank the reviewer for pointing out the comparison with Marlton et al. (2021). While a ±1K bias is indeed the standard expected across the ECMWF ERA5 reanalysis globally, the specific 10K discrepancy observed at 30 km over La Réunion can be explained by several reasons.

The dynamics in the tropical Southern Hemisphere are distinct, and biases observed at mid-latitudes cannot be strictly transposed to the tropics, where large-scale dynamical drivers differ. The accuracy of reanalysis heavily relies on data assimilation. The Northern Hemisphere benefits from a dense network of in-situ measurements. Conversely, La Réunion (21°S, 55°E) is isolated in the Indian Ocean, a region notoriously sparse in observational data. With fewer local radiosondes to constrain the model, ERA5 background states in this region carry higher uncertainties.

As this paper demonstrates, the night of November 20, 2023, was characterized by intense, short-scale GW activity. Lidar provides high-resolution, instantaneous local measurements capturing the peak amplitudes of these waves. ERA5, constrained by its grid resolution and physical parameterizations, tends to smooth out or misrepresent the phase of such small-scale dynamical features. Therefore, the 10K difference is a representation error highlighting the model's inability to resolve the strong local GW perturbation, rather than a systematic instrumental bias.

Line 287: Please erase "however". Makes no sense here.
Done

Line 291-293: Please erase "Above 50km […] small scale variability." I don't see noise induced features. Also, the ERA5 temperature profile does not remain isothermal.
Done

Line 302: Instead of "downward phase propagation" it must be "downward phase progression".
Done

Line 306: What do you mean by "incoherent perturbation values"? Please comment on that.
We wanted to write :" structures that are inconsistent with gravity waves".
We have revised our sentence to ensure it is consistent with our response.

Line 313-314: You're work would improve if you'd use the variance based GWPE profile as established benchmark and compare the other GWPE profiles to it. Then you can show that your MRA method delivers roughly the same results and you can use this as an argument to compute reliable GWKE profiles in the second step.
We thank the reviewer for the comment. We now use the variance based GWPE as a reference and compare the other GWPE to it. We introduce two tables instead of one : the first table (numbered table 2) is the relative error between variance based GWPE and the other GWPE profiles, the second table (numbered table 3) is the relative error between MRA based GWKE and the other GWKE profiles.

Line 321: GW breaking or dissipation is given already when the GWPE profile deviates from the conservative growth profile (dashed line). Please rephrase.

We agree with the reviewer. Theoretically, in the absence of dissipation, the GWPE per unit mass should increase exponentially with altitude (following the adiabatic growth line) to conserve energy density. Consequently, any deviation from this theoretical growth indicates that dissipation or saturation processes are already occurring. We have rephrased the sentence to clarify that the dissipation starts as soon as the profile deviates from the adiabatic line, leading to the observed minimum at 55 km.

Line 326: Please note. Conservative growth depends on stratification and scale height. they drastically change from 30 to 80km. What is used for the dashed profile? Also, lateral propagation of waves can result in a GWPE increase exceeding the conservative growth rate.
In this case, we used the mean scale height of ~ 7 km (same scale height used by Mzé et al. (2014))

Line 328: Please make a statement about edge effects.
As we responded previously, the edge effects are mitigated by reducing the height range. Hence, no edge effects are visible in the GWPE or GWKE profiles.

Figure 5: Figure 5 in this manuscript and Figure 6 in the previous manuscript show a phaseshift in the GWPE profile based on the variance method below 40km. Please comment on that.
In the previous version of the manuscript, a phase shift was effectively present, primarily resulting from an error in the processing of the raw data used to derive temperature profiles. The variance is taken as the reference and is computed directly from the raw lidar signal (in photon counting mode), which is proportional to atmospheric density. Because temperature profiles are obtained through vertical integration, a phase shift may arise in the presence of gravity waves. This shift is expected to depend on the vertical wavelength of the gravity waves. However, given the broad spectrum of propagating gravity waves, a reliable estimation of the phase shift remains challenging.

Table 2: Please get rid of the two decimals. It makes the table only less readable.
We deleted the two decimals and split the table in two tables : one for GWPE and the other for GWKE.

Application to lidar wind profiles

Line 359: As mentioned earlier the excitation mechanism is not of importance for this work. Also, what do you consider small-scale? These inertia-GWs are usually in the order of hundreds of kilometers.
Even if the excitation mechanism is not of importance for this work, these inertia GWs have short vertical scales to be precise. In the review of Plougonven and Zhang (2013), they mentioned Sato et al. (1994) that show a dominant mode of short vertical wavelength (~ 4 km) in the stratosphere.

Line 367: What do you mean by "MRA results highlight a connection between GWs"? You must better show and explain what you mean.
Here, we aim to draw the reader's attention to the potential interactions between gravity waves (GWs) across different scales. The multiresolution analysis (MRA) decomposes the signal into distinct

vertical-wavelength bandwidths. An examination of the gravity wave kinetic energy (GWKE) reveals that, for certain components, GWKE decreases up to a specific altitude, whereas for others it increases over the same altitude range. These contrasting behaviors suggest the existence of interactions between gravity waves of different scales. Moreover, such interactions are not captured by conventional analysis approaches.

We can rewrite the sentence as follows : "MRA results suggest interactions between small- and large-scale GWs".

Line 380: Since you do not have a separate discussion section, please add some interpretation on to why GWKE profiles differ.

From the previous point, the GWKE profiles differ because each profile does not represent the same part of the gravity spectrum. GWs frequency can change during their propagation through the middle atmosphere, so their energies have to change.

Line 381: Please motivate why you are investigating the energy ratio.

The energy ratio, as defined by Geller and Gong (2010), provides information on the intrinsic periods of gravity waves. Accordingly, applying multiresolution analysis to temperature and wind measurements makes it possible to examine the evolution of gravity wave frequencies during their propagation. Previous studies have shown that gravity waves are filtered differently in the stratosphere and the mesosphere (Wilson et al., 1990).

The overall purpose of using the MRA is to document the vertical propagation of GWs through the middle atmosphere and extend it to the troposphere.

Line 393: Please add more discussion to your findings. What can one learn from analyzing the energy ratio?

The energy ratio is a piece of information about gravity waves' intrinsic period. A further analysis is to compute the intrinsic period by using the ratio and the coriolis parameter. Furthermore it is possible in this case to compute the momentum fluxes.

Line 401-402: Please erase "A transfer […] middle atmosphere." You have not studied momentum fluxes but only energies. Concluding on potential secondary wave sources is fetched too far.

The study of energies can lead to potential secondary sources if we combine these results with the momentum fluxes results. The analysis is not done here but can be done for future works.

Summary, Conclusion and perspectives

Line 407: Please erase "during a split jet episode"

Done

Line 411: What do you mean by "intrinsic bias"?

We aimed to underline that, beyond filtering differences, the implementation of a given technique constitutes a second source of bias between the approaches.

We erased "intrinsic bias" because it was misleading.

Line 413: When the measurement is not even 5h long, I would expect to have no PW or tidal signature in derived perturbations. Please rephrase.

Morel et al. (2002) show that large amplitudes of middle atmospheric tides are observed by the lidar at La Réunion. Time coverage is distributed from 4h to 8h per night.

In this case, PW or tides might not be expected however on a different night they might be derived. It is important to deal with them at this point.

Line 426: You mention this several times. The paper would benefit from a sensitivity test using another wavelet or another vertical sampling. Adding this to the appendix would be sufficient.

We add a sensitivity study to the appendix by comparing the vertical wavelength bands with different vertical sampling and comparing the GWPE and GWKE by using different orders of Daubechies wavelet.

Line 453-455: Please erase. This statement is too far fetched.

Done

Minor comments on typesetting and spelling:

1. Please use consequently either "multiresolution" or "multi-resolution".
   Done
2. Please abbreviate "gravity wave" consequently as "GW".
   Done
3. Please abbreviate "gravity wave potential energy" consequently as GWPE.
   Done
4. Please abbreviate "gravity wave kinetic energy" consequently as "GWKE".
   Done
5. Please erase "density" consequently every time GWPE or GWKE is mentioned. I and also the reviewer 2 suggested that in our first review. This suggestion is simply because GWPE density would refer to energy per volume but you are determining GWPE per mass.
   Done
6. Already mentioned by reviewer 2 in the first review: Throughout the article, the same method is called differently, e.g. "The time averaged background profiles method" and "the nightly mean temperature profile method", "the sliding polynomial fit method" and "the Savitzky-Golay method". Select one way of calling it and use it everywhere!
   Done
7. When mentioning variables, define them please. The following variables are not defined in the current version of the manuscript: \Delta t, \Delta z, N_t, N_z, g, N, d, n
   Done