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 8th 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.

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."

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."

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 compare 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."

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

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.

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

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."

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

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

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

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.

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

Line 61: New line after the Ehard reference.

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

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

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."

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

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

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.

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.

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

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.

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.

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

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 insetad?

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

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.

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."

Line 138: What do you mean by "spectral bands of GWs are difficult to define"?

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

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

The sliding polynomial fit Method

Line 144: Include a proper reference: Savitzky and Golay (1964)

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.

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."

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.

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.

Table 1: As questioned above, why do you state >1h instead of >30min?

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.

Equation 6: Please add parenthesis.

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.

Line 247: What do you mean by "primary propagation characteristics"? Please specify.

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

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.

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.

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

Application to lidar temperature profiles

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

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.

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.

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

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.

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

Line 306: What do you mean by "incoherent perturbation values"? Please comment on that.

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.

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

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.

Line 328: Please make a statement about edge effects.

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.

Table 2: Please get rid of the two decimals. It makes the table only less readable.

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.

Line 367: What do you mean by "MRA results highlight a connection between GWs"? You must better show and explain what you mean.

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

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

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

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.

Summary, Conclusion and perspectives

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

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

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.

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.

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

Minor comments on typesetting and spelling:

1. Please use consequently either "multiresolution" or "multi-resolution".
2. Please abbreviate "gravity wave" consequently as "GW".
3. Please abbreviate "gravity wave potential energy" consequently as GWPE.
4. Please abbreviate "gravity wave kinetic energy" consequently as "GWKE".
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.
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!
7. When mentioning variables, define them please. The following variables are not defined in the current version of the manuscript: Δt, Δz, N_t, N_z, g, N, d, n