Dear editor, dear anonymous referees,

We are grateful for the comments we received and answer to them in the following. We have copied your comments below and our answers are printed in blue.

Anonymous Referee #1

The reviewer's concerns about the original manuscript have mostly been solved, and the revised manuscript is considered acceptable for publication. I have the following minor comments, but I want to leave these points to the authors.

- Now I understand that the many data points for each wind shear and wavelet order combination in Figure 4 correspond to different altitudes from 0 to 50 km with an interval of 0.1 km. This point should be clearly described in the text because it is unclear in the current description.

We have added the number of wavelength ratios to make this point a bit more clear: “Figure 4 illustrates the distributions of **500** wavelength ratios derived from the lowermost 50 km of the simulated altitude range and Table 3 lists the corresponding median deviations as well as interquartile ranges (IQR).”

- I came to understand the meaning of 'Non-Stationary' in the title, but I still recommend changing it to avoid a potential misunderstanding. Rephrasing it to 'Non-uniform' will be more appropriate.

We understand the distinction between “non-stationary” and “non-uniform” as the former refers to variable statistical properties in time while the latter refers to variable statistical properties in space. However, we refrain from rephrasing the title and exchanging “non-stationary” by “non-uniform” multiple times in the manuscript. From our perspective the dimension, i.e. time and space, is not of importance for our analysis. The suggestions we make are applicable to timeseries as well as vertical profiles.

Anonymous Referee #2

The revised manuscript "Limitations in Wavelet Analysis of Non-Stationary Atmospheric Gravity Wave Signatures in Temperature Profiles" by Reichert et al. reads much better now. Still, there are a few minor corrections required before publication in AMT.

SPECIFIC COMMENTS:

l.174: In Eq.(10) you are using m_o as the vertical wavenumber of the GW.
However, in this equation the vertical wavenumber m should be an independent parameter.
Later on, you should state that in Eq.(11) you choose m_o=m to illustrate the effect of biases that are introduced if no amplitude scaling is applied.

We thank the reviewer for this comment and have adapted Eq.(10). In addition, we added the half sentence after Eq.(12): “[…] when choosing $m=\frac{m_0}{s}$, i.e. the Morlet wavelet's vertical wavenumber is equal to the GW's vertical wavenumber.”

l.175: initial temperature -> initial GW temperature amplitude

We thank the reviewer for this comment and have added “amplitude”.

l.303: The statement "obliquely propagating MW" is too vague!
You should explicitly mention that above 35km possibly not the same GW is observed as below 35km because the vertical wavelength no longer matches the expectations for a mountain wave.

We have altered lines … in the following way:
"Following the traditional analysis, this jump might be interpreted as a hint on two distinct and often termed "quasi-monochromatic" wave packets. The difference between computed and measured vertical wavelength (Fig. 8d) could be an indication for two different wave packets, one below and one above ~32km. However, with our new best-practice approach there is evidence that the observed signature reflects a MW undergoing a rapid wavelength shift. ERA5 temperature perturbation fields and co-located OH-airglow imagery (both not shown) provide more evidence that the MW observed by CORAL propagates steeply within the lidar's field of view. After all, this work is of methodological nature and the geophysical interpretation of the results is not in our focus."

TECHNICAL COMMENTS:

l.100: where -> where, in our case,

We changed that as suggested.

l.121: extend -> extent

We changed that as suggested.