Replies to Reviewers' comments on

"A method to derive Fourier-wavelet spectra for the characterization of global-scale waves in the mesosphere and lower thermosphere, and its Matlab and Python software (fourierwavelet v1.1)"

I am thankful to the Reviewer, Dr. Jun-Ichi Yano, for taking the time to provide more comments. My response to individual comments can be found below. Like the last time, the comments from the reviewers are highlighted in blue, and the text from the revised manuscript is highlighted in red.

RC1 (Dr. Jun-Ichi Yano)

In the original review, I stated that "This is an interesting piece of work, potentially worthwhile for a publication". My basic position on this manuscript has not changed after the revision. Though I did not say this explicitly in initial review, I do not find any serious defect in this work. In this very respect, I do not wish to block the publication of this work in any fundamental manner. The only remaining question is the quality of the publication that both the Editor and the author wishes to achieve. My following comments intend to serve for this purpose:.

[**Response**] I am glad that the reviewer did not find any serious defect. It is my intention to deliver my work in its best shape. So, I truly appreciate that the reviewer is trying to help from his perspective.

Most seriously, the two major points made in my initial review are practically not at all taken into account in revision:

1) review of the existing methodologies

2) proper review of the wavelet method

1) more specifically, I proposed to refer to Zagar et al (2015, 2016). However, the authors rejects to do so by simply stating that the normal mode approach is not relevant in the present context. Most seriously, this remark is found only the response to the reviews, and very strangely, not found in the manuscript text. As a result, as it stands for now, the text reads like claiming that this Fourier-wavelet method is an only possibility of describing the propagating waves. Of course, the readers who are familiar with those existing methodologies would just wonder why one must use this when an existing method works well enough: what is an advantage of this proposed methodology against the existing methodologies?

[**Response**] I have revised manuscript to expand on the detection of normal modes in the troposphere and stratosphere. The Zagar et al. (2015) paper is cited therein:

(lines 34-39): "Global characteristics of normal modes can be predicted based on the linear wave theory (Kasahara and Puri, 1981; Zagar et al., 2015; Marques et al., 2020). Spectral analysis of meteorological data has confirmed the existence of waves similar to those theoretically predicted in the troposphere and stratosphere (e.g., Madden, 2007; Sakazaki and Hamilton, 2020). However, characteristics of traveling planetary waves in the MLT region are expected to deviate considerably from those of theoretical normal modes due, for example, to dissipation and mean winds (Salby, 1981c)."

2) As it stands for now, there is even no proper lead sentence io introduce what the wavelet is. There is only a very short lead paragraph in Sec. 1.3, which is even misleading:

a) Torrence and Campo (1998) is just an introductory essay on wavelet. It would be misleading to state that the methodology of this paper is "described" in this essay: please cite a more proper textbook. my preference is one by Mallat.

[**Response**] In the revised manuscript, Sec. 1.1 introduces the wavelet technique citing the Mallat book.

(line 95-100): "The wavelet analysis (e.g., Mallat, 1999) is a multiresolution analysis technique using a 'wavelet', which is a short-term duration wave. A wavelet transform can be performed on one-dimensional (1-D) time-series to derive a 'wavelet spectrum', which is usually presented in a time versus period diagram. The wavelet spectrum is useful for identifying wave activity that is localized in time. The wavelet algorithm avoids the use of a moving window, which makes the technique more computationally efficient than the short-term analysis."

Sec. 1.3 gives a brief summary of the Torrence and Compo (1998) technique, providing the information that is required for understanding Sec. 1.4 (Fourier-wavelet analysis). It is not my intention to give a full description of the Torrence and Compo technique. This is now clarified in the revised manuscript.

(lines 145-148): "A wavelet analysis is performed in time. The method considered here is the continuous wavelet transform described by Torrence and Compo (1998). Their wavelet software including those in Matlab and Python are available from the website [http://atoc. colorado.edu/research/wavelets/]. The Torrence and Compo technique is widely used in atmospheric science due to its ease of use. Below the technique is only briefly summarized. Readers are referred to Torrence and Compo (1998) for full details of the technique."

b) As the authors states, with the continuous wavelet, a user can arbitrary select any frequency resolution, but only in a meaningless manner: this is just like trying to define the amplitude of noninteger wavenumbers over a finite domain, say, a wavenumber 1.345. Of course, one can do this, but we all know that this is meaningless. Strangely, the community simply does not realize the same with the continuous wavelet. The present author is not an exception. More formally stated, such an attempt contradicts with Heisenberg's uncertainty principle. Please refer to Mallat's text for the full discussions.

[**Response**] The text describing the advantage of the continuous wavelet transform over the discrete wavelet transform in Sec. 1.3 has been removed.

The advantage and applicability of the discrete wavelet transform is noted in the conclusion section (Sec. 4).

(lines 355-369): "Future work includes the improvement of the technique for faster computation and broader applications. The technique introduced in this paper relies on the 'continuous' wavelet transform. Studies have shown that the ‘discrete’ wavelet transform has some advantages such as non-redundancy (and hence more efficient computation) and straightforward invertibility (e.g., Mallat, 1999; Yano et al., 2001b, a, 2004). The discrete wavelet transform may be implemented in the Fourier-wavelet technique.

An important limitation of the Fourier-wavelet technique is that it can resolve only global-scale waves. Along with tides and traveling planetary waves, gravity waves are also important in the MLT region (e.g., Fritts and Alexander, 2003; Smith, 2012), with a wide range of zonal wavenumbers (up to 100 or so) (e.g., Miyoshi and Fujiwara, 2008; Liu et al., 2014). Since gravity waves are often localized in space, the Fourier-wavelet technique would not be able to fully capture them. A 2-D wavelet analysis (e.g., Kikuchi and Wang, 2010) would be useful. An easy-to-implement ‘wavelet-wavelet’ technique for evaluating gravity-wave amplitudes and phases may be developed as an extension of the Fourier-wavelet technique presented in this paper.

Although this study has focused on waves in the MLT region, the Fourier-wavelet method could be applied to data from other regions of the atmosphere. The technique may also be useful in research areas outside atmospheric science. The extent of applicability of the technique is still to be explored.”

c) In short, there is no real advantage with the continuous wavelet against the discrete counterpart, apart from an easiness of using it. This very last point should be made absolutely clear in a final version with more relevant references (including a textbook by Mallat).

[**Response**] In the revised manuscript, the text describing the advantage of the continuous wavelet transform has been removed, and it is stated that the Torrence and Campo (1998) technique has been widely used because of its ease of use.

(lines 145-148): “The method considered here is the continuous wavelet transform described by Torrence and Compo (1998). Their wavelet software including those in Matlab and Python are available from the website [http://atoc. colorado.edu/research/wavelets/]. The Torrence and Compo technique is widely used in atmospheric science due to its ease of use.”

The Mallat reference is cited in Sec. 1.1 when the wavelet technique is mentioned for the first time.

3) The introduction as it stands for now, mostly consists of a review of the existing studies on the plain planetary waves. The given review hardly motivates the present study.

In this context, the author totally neglects the following full paragraph from my initial review:

"In this respect, the introduction is slightly confused as it stands for now: its first half reviews previous works detecting "linear" planetary waves. Then, suddenly, at L55, the author decides to talk about the stratospheric sudden warming: this is clearly a nonlinear process that cannot be described by a single wave. The authors further begins to remark that the observed waves are rather "intermittent" (in own wording), and they can emerge even like bursts: that is all fine with me: these observed waves are not perfectly linear, and they are often generated by forcings as well as instabilities, and those evolution can be very nonlinear. However, after said all those (though the author does not comment on them), if one wishes to understand those phenomena as a part of the wave dynamics, an obvious way to go is to perform the normal-mode decompositions so that one can see explicitly which modes are involved in processes in which manner, etc. Those are very basic backgrounds of the atmospheric-wave dynamics, that should remind the readers."

[**Response**] Traveling planetary waves in the MLT region usually do not behave like normal modes of the linear wave theory. That is the reason the normal-mode decomposition function technique has limited applicability there. I have re-emphasized this point in the revised manuscript.

(lines 37-40): "However, characteristics of traveling planetary waves in the MLT region are expected to deviate considerably from those of theoretical normal modes due, for example, to dissipation and mean winds (Salby, 1981c). Also, some traveling planetary waves in the MLT region are considered to be unstable modes locally generated by atmospheric instability, rather than normal modes (e.g., Pfister, 1985; Meyer and Forbes, 1997)."

(lines 45-46): "The zonal wavenumber and wave period of these waves are consistent with Rossby modes of the linear wave theory, but their meridional and vertical structures are generally different from those of theoretical Rossby modes."

(lines 54-56): "Observations also sometimes show westward-propagating planetary waves in the MLT region whose periods do not match those of normal modes (e.g., Qin et al., 2022a, 2021b)."

An only hint for a need for the wavelet is a short phrase of "a burst of wave activity" (L57). However, without any proper elaboration, it is even not clear what the author is exactly referring to. Finally, at L73, the author states, "This is the motivation.....". However, unfortunately, I cannot identify a sentence that can be called a motivation in any earlier part: then what is this "This"? The author quickly adds a well known fact that "The wavelet analysis is useful for identifying wave activity that is localized in time" (L79): however, how often we observe such isolated waves in the atmosphere?etc The author simply fails even to provide such basic information.

[**Response**] In the revised manuscript, I have elaborated on the transient nature of global-scale waves in the MLT region.

(lines 64-69): "Traveling planetary waves in the MLT region sometimes show a burst of wave activity that lasts for a few wave cycles. This can be attributed to changes in the zonal mean state of the atmosphere, which controls propagation conditions, atmospheric instability, and critical layers (e.g., Salby, 1981b, c; Liu et al., 2004; Yue et al., 2012; Gan et al., 2018). A wave burst is often observed around seasonal transition, but its characteristics (e.g., magnitude, peak period, meridional structure, and so on) vary from year to year, so that it is difficult to predict them (e.g., Gu et al., 2019; Liu et al., 2019; Yamazaki et al., 2021). Also, some wave burst events occur during sudden stratospheric warmings."

The logic of the presentation is very loose at the best at many occasions. The most notable example is found in L80-81, which states: "the standard 1-D wavelet technique is not directly applicable to two-dimensional (2-D) longitude-time data...." If I understood the author's response to my comment correctly, this is nothing other than a trivial statement that any 1D transformation technique (wavelet or Fourier or else) is not directly applicable to any 2D data, because the transformation method is only 1d, and not 2D. Thus, we must adopt either 2D wavelet or 1D Fourier technique. It does not follow at all the we need to invoke Hayashi's method to overcome this difficulty. We just need to invoke any available 2D techniques.

[**Response**] The manuscript has been revised to make the motivation clearer and more logical.

(lines 86-104): "Characterization of global-scale waves requires the identification of the zonal wavenumber and wave period (see equation 1), which demands two-dimensional (2-D) spatiotemporal data (more specifically, data as a function of longitude and time). Techniques such as 2-D fast Fourier transform (FFT) (e.g., Hayashi, 1971) and 2-D least-squares fitting method (e.g., Wu et al., 1995) can be applied to the data to evaluate the zonal wavenumber and wave period of global-scale waves and their amplitudes and phases. Taking into account the transient nature of global-scale waves in the MLT region, a short-term analysis is commonly used. That is, a 2-D spectral analysis is performed on a short-time segment of the data, then the analysis window is moved forward in time (e.g., Maute, 2017; Forbes et al., 2018; Liu et al., 2021). This way, it is possible to evaluate temporal variations of global-scale waves. However, such a moving-window approach is computationally expensive because the spectral analysis needs to be repeated for multiple times. As a solution to this problem, this study proposes the application of wavelet analysis. The wavelet analysis is a multiresolution analysis technique (e.g., Mallat, 1999). A wavelet transform can be performed on one-dimensional (1-D) time-series to derive a 'wavelet spectrum', which is usually presented in a time versus period diagram. The wavelet spectrum is useful for identifying wave activity that is localized in time. The wavelet algorithm avoids the use of a moving window, which makes the technique more computationally efficient than the short-term analysis. The main objectives of this study are (1) to introduce a simple method to derive 'wavelet-like' spectra from 2-D longitude-time data, which can be used for the characterization of global-scale waves in the MLT region, and (2) to deliver easy-to-use software in two user-friendly languages: Matlab and Python. For (1), the 2-D FFT method of Hayashi (1971) is used, and it is modified by adopting the wavelet technique of Torrence and Compo (1998). The Hayashi (1971) method is easy-to-implement and its spectrum directly gives the wave amplitude in units of the input data, which is easy to interpret."

In conclusion, I see that still substantial revisions are required before this manuscript becomes worthwhile for publication. [I click "major revision" because more than a cosmetic modification is required: the author clearly failed to note this point in then initial revision] As the very minimum, misunderstanding concerning the wavelet by the author must definitely be corrected.

Thank you again for a guideline for improving the manuscript.

More Specific:

L77: please add a reference discussing on the discrete wavelet in a more proper manner, for example: Yano et al (2001a, b)

Here I cite the Mallat book. But Yano et al. (2001a, b) are cited in Sec. 4 in addressing the future work.

L83, "Hayashi's method is combined with the wavelet technique": probably is would be more proper to say that "Hayashi's method is modified by adopting the wavelet in representation in time."

The change has been made as suggested.

(line 102): "For (1), the 2-D FFT method of Hayashi (1971) is used, and it is modified by adopting the wavelet technique of Torrence and Compo (1998)."

L130, Yano et al (2001, 2004): please also to refer to the second part (Yano 2001b) for a completeness.

The change has been made as suggested.

(lines 356-358): "Studies have shown that the 'discrete' wavelet trans- form has some advantages such as non-redundancy (and hence more efficient computation) and straightforward invertibility (e.g., Mallat, 1999; Yano et al., 2001b, a, 2004)."