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Comment on egusphere-2022-1275

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Referee comment 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)" by Yosuke Yamazaki, EGUsphere, https://doi.org/10.5194/egusphere-2022-1275-RC1, 2022

This is an interesting piece of work, potentially worthwhile for a publication: there are already extensive literature performing the continuous wavelet transform to time series in atmospheric science. However, this work is new: the continuous wavelet transform is applied to time series, that itself is nothing new, but in the wavenumber space. The author shows that this methodology can characterized certain wave activities rather well, as shown in Figs. 2-4.

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The main problem with the present manuscript, as it stands for now, is to present it with a rather sensational fanfare, calling it specifically a "combined Fourier-wavelet spectrum". As far as I can follow, it is nothing other than just performing a continuous wavelet transform in time and a Fourier transform in space (longitude): these are two independent linear operations, that can be performed in any order. There is nothing to "combine", but just to perform two independent things in sequence. In other words, the adjective "combined" is nothing other than just a misnomer.

With an attempt of the author convincing the readers that this is "revolutionary", the author provides rather lengthy technical details of the so-called Hayashi's method (that itself is nothing other than just performing the Fourier transform in time and longitude) and the continuous wavelet in the last half of the introduction section. However, I do not see those technical details to be any importance for this method. As I just said above, what is done in practice is very simple: perform FFT in longitude, and the continuous wavelet analysis in time. We just need few more extra words on specific.

On the other hand, the presentation of the proposed methodology in Sec. 2 is rather "muddled", and difficult to get this very simple point straight.

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A simple methodology is always a beauty. However, if the author intends to present the present manuscript as a proposal paper of a new methodology, a more careful review of the existing methodologies is required.

First of all, planetary waves can be extracted in a straightforward manner, at least in principle, by the normal-mode decomposition. A full description of this methodology is provided in Zager et al. (2015, GMD, https://gmd.copernicus.org/articles/8/1169/2015/) with a software publicly available to apply this methodology. Also please refer to a workshop report for further backgrounds: Zagar et al. (2016, BAMS, https://journals.ametsoc.org/view/journals/bams/97/6/bams-d-15-00325.1.xml).

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.

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Second is lack of a proper review of the wavelet method. The most important question, in this context, for me is a choice between the continuous redundant wavelet and the discrete orthogonal wavelet. Here, the author chooses the former, but without explanation. The choice is just puzzling for me considering a very fact that the latter is much more robust, with much more potential applicabilities, as my series of work suggest: see a list of reference below.

Performing a continuous wavelet analysis is like a decomposition of a finite domain data into continuous wavenumbers, when only the discrete integer wavenumbers have a meaning.

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Finally, if detection of a planetary-wave packet is the main issue, a discrete set of wavelets can be constructed based on normal modes, in an equivalent manner as the Meyer wavelet is constructed based on the Fourier modes. Though I do not think that the author has to try this possibility in the present work, all those potential possibilities must be clearly mentioned in the manuscript.

It is obvious that the author is only taking a small first step forwards for exploring all those wider possibilities.

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Specifics:

L79-80, the standard wavelet technique is not directly applicable to longitude-time data: false. 2D wavelet transform can easily performed in analogy with the 2D Fourier transform: refer to my publications below.

L90, parameter -> variable

Eqs. (5)-: the frequency, omega, must be discrete, as the case for the wavenumbers. please comment on this

L122: state explicitly that continuous wavelet is applied in time

Eq. (16): the actual data set only has a finite length in time. comment on this

Eq. (21): if not, this expression is puzzling: since continuous wavelet is applied here, obtained coefficients must also be continuous: why we suddenly get a discrete expression?

Sec. 2: as far as I can follow, the longitudinal dependence does not play any role in the presentation, though Eqs. (29) and (30) retain it. at least a word would be required for a clarification: otherwise, in my own reading, Sec. 2 is essentially just repeating Sec. 1.3. If not, what is a difference, except for a longitudinal dependence added in Eqs. (29) and (30)?

Eqs. (29), (30): the exponent, -t^2/2 must be replaced by -t^2/2s? if not, I do not know how to connect this expansion with (17), as invoked after Eq. (34).

Eqs. (29), (30): the given decomposition modes are only localized in time, thus it appears to me that the author essentially fails to address a question of the propagation of a wave packet, that should happen both in time and space.

Eqs. (35) and (36): Psi* here must depend on both s and omega: how do you specify them?

Fig. 5b, c: they should be better presented by standard Fourier transforms: the plots do not take any advantage of wavelet, either.

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