

Interactive comment on “2-D Empirical Mode Decomposition on the sphere, application to the spatial scales of surface temperature variations” by N. Fauchereau et al.

N. Fauchereau et al.

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The authors wish to thank the two anonymous referees for their constructive comments. We address these comments below and indicate what changes we have made to the manuscript, where relevant.

Response to comments by referee 1

1) *It is perhaps not fair for the authors to say that 2-d wavelets explore only 3 components. While it is true that if you were interested only in an orthogonal decomposition, you would use 3 directions (namely, lat., lon., and diagonal); however, a 2-D continuous wavelet transform (CWT) would, in my opinion, more or less achieve what the authors have attempted in this work (for example, see the web-site that has a toolbox called "Yet*

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another wavelet toolbox", where they have performed a 3-D CWT on a sphere). The one downside might be that you get a highly redundant representation with a CWT; however, if the scales are "well separated" then I suspect that the process (computationally cumbersome with both EMD and CWT) and the results would, in spirit, be similar.

As far as the authors are aware, the distinction between a continuous and discrete wavelet is simply whether the mother wavelet has been defined as a continuous or discrete function and that an n-dimensional wavelet transform is an operation of the 1-D (continuous or discrete) wavelet along multiple directions in this n-dimensional space. Thus we do not see how working with a continuous or discrete transform avoids the need for a choice of directions. However, we certainly do not claim to be experts in Wavelet analysis and may therefore be mistaken.

2) The authors claim that it is a completely data-driven methodology. Is that really true? The methodology assumes a certain kind of decorrelation (exponential variogram) for fitting the "surface extrema envelopes".

It is true that we chose to use Kriging to generate the extrema envelopes, which requires a choice of variogram model. However, alternative surface fitting techniques should provide similar results (provided the fitted surfaces are smooth). We have moderated our claim of "a completely data-driven methodology" to be one of "a data-adaptive and largely data-driven methodology".

3) Section 6 can perhaps be expanded, if possible. In relation to the overall length of the article section 6 is too small. The results that they have obtained are not new. If the analysis is repeated for each month instead of the annual mean, one might end up with a more spatially variable field. How would the method fare in such a case - especially where "small-scale" variability is prominent?

We've expanded section 6 slightly, with more discussion on the meaning of the results and why surface air temperature provides the ideal real test-bed for EMD on the sphere.

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If EMD is performed on individual months, we don't obtain substantially more spatially variable fields, as at any given month the spatial variability of surface temperature is still the result of the combination of local (e.g. topographic), regional (e.g. ocean / land contrasts) and global (radiative forcing given the sphericity of the earth) forcings.

4) Page 414, section 5, 3rd para: should be Figure 4, and Page 415, section 6, para 1 should be Fig. 6a and 6b; paras 2, 4, 5 and 6: should be Fig. 7. There are a few typos scattered across the manuscript, which the authors might want to check carefully.

The authors thank the reviewer for noting these inconsistencies in the text. We have corrected these errors in the revised manuscript.

Response to comments by referee 2

1) My main concern is that the description of the algorithm is purely descriptive. The article would be substantially stronger and a better reference if the authors extend the mathematical formulation of 1D EMD to two dimensions over the sphere - following the initial development of Huang (1998), or even Sinclair and Peram (2005) - in this last paper the authors also skip the mathematical description of the 2D algorithm.

The manuscript has been revised to provide a more mathematical approach in preference to the descriptive one we originally adopted.

2) My second concern is the number and presentation of the figures. There are too many figures, and they don't present the results in the most efficient way. We only really need to see the difference between the regular 73 by 144 grid and the zonal equal area partitioning of the sphere ONCE, not in every example. In addition, the quantity of figures makes it difficult to compare the resulting modes to the original functions used to create the figures. I recommend the authors cut back on the figures and make their comparison easier for the reader.

We've removed figures 2a and 6a as we agree that the difference between the regular and the interpolated grid needs to be presented only once. In addition, we made the

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figures more compact so that the reader can more easily compare the fields and the corresponding Intrinsic Modes Surfaces.

3) *Because of the innovative nature of this technique, a schematic diagram explaining the algorithm (the maximum envelope, minimum envelope, - much like in Huang (1998) for the 1D case) would be very useful. I recommend including this figure.*

As one of the reviewer's concerns (point 2) was the number of figures, we've chosen not to add a supplementary one. A schematic diagram can be found e.g. in Huang et al (1998) and Peel et al (2005), and the reader is now referred to these publications in the text.

4) *In the introduction, the authors should mention Singular spectrum analysis as it is data-derived empirical way of separating scales that doesn't require a pre-defined function.*

We've added a reference to this method in the introduction, however it makes the assumption of stationarity and linearity (because in essence it applies an EOF decomposition to lagged time-series).

5) *Is the quasi-orthogonal nature of the decomposition in 2D mathematically proven? If so where? If not, can you confidently say they are quasi-orthogonal?*

We agree that the decomposition is not orthogonal in the truly mathematical sense, the extent to which the decomposition can be considered orthogonal is related to the stopping criterion of the sifting process as discussed by Junsheng et al. (2006). We are however, confident that quasi-orthogonality is guaranteed by the construction of the algorithm, by this we mean that the scales, which contribute most to the variance in each mode are well separated between modes.

6) *The variance should be expressed as percentage of the initial variance of the data, or as some normalized value that lets the reader understand how much of the initial energy is captured by the decomposition. Is the sum of the variance of the IMSs equal*

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to the initial variance?

The variance is now expressed as the percentage of the initial variance of the data for the application on the air temperature, the variances add up to 99.5 of the original variance, which is expected due to the variance computation and some rounding errors.

Minor editorial comments have been taken into account in the revised manuscript.

References:

Huang, N.E., Shen, Z. , Long, S.R., Wu, M.C., Shih, H.H., Zheng, Q.N., Yen, C., Tung, C.C. Liu, H.H. 1998 The empirical mode decomposition and the Hilbert spectrum for nonlinear and non-stationary time series analysis, Proceedings of the Royal Society London A, 454, 903-995.

Junsheng, C., Dejie, Y. and Yu, Y.: Research on the intrinsic mode function (IMF) criterion in EMD method , Mechanical Systems and Signal Processing, 20, 817-824, 2006.

Peel, M. C., Amirthanathan, G. E., Pegram, G. G. S., McMahon, T. A., and Chiew, F. H. S.: Issues with the application of Empirical Mode Decomposition Analysis, in: MODSIM 2005 International Congress on Modelling and Simulation, Modelling and Simulation Society of Australia and New Zealand, edited by: Zerger, A. and Argent, R. M., December 2005, 1681–1687, ISBN: 0975840029, 2005.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 405, 2008.

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