

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.

Anonymous Referee #2

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General Comments

The paper presents an extension of Empirical Mode Decomposition to a 2-D space over a sphere. In so doing, the authors overcome the obstacle of equal-area partitioning of the sphere. The results, analyzed for an artificial dataset and global temperature, present compelling evidence of the strengths of the method. The paper is well written and thorough, and certainly a useful contribution to “Hydrology and Earth System Sciences”. However, I have several comments about the presentation of the methodology and the way the results are presented. These need to be addressed by the authors before I can recommend publication.

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Specific Comments 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.

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.

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.

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.

5. Pg. 141, 1st paragraph. 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

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 to the initial variance?

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Technical Comments

1. Pg 406 Line 7 : “an artificial dataset”
2. Page 407 Line 1 : “dimensional time-series”
3. Page 407 Line 4 : “predefined”
4. Page 415 Line 3-4 : “regular drid at a monthly”
5. Page 415 Throughout : You are referring to Figure 6 where it should be Figure 7, 7a, 7b

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