

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 #1

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This manuscript proposes to apply empirical mode decomposition (EMD) in 2 dimensions over the sphere as a means of identifying various spatial scales. Overall, the manuscript is very nicely written and has a smooth flow to it. The “elimination” of the end-point/edge effect on a sphere (sounds trivial, after the fact!) is nice. The length of the article and the related discussion upto section 5 is just about perfect (a schematic/flowchart of the method *might be* a useful addition, but not necessary). Barring a few typos and a couple of places where figures were incorrectly referred and some other issues, I would recommend publication.

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Outlined below, in no order of importance, are a few issues (mostly, loud thoughts) which I would like the authors to consider:

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 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.
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”.
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?
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.

In summary, I would recommend publication with minor revisions.

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

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