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Interactive comment on "Multi-scale analysis of the Asian Monsoon change in the last millennium" *by* T. Jiang et al.

Anonymous Referee #2

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This paper applies a Ensemble Empirical Mode Decomposition (EEMD) method and spectral analysis to paleoclimate records (isotopes from stalagmites in China, solar activity proxy and northern hemisphere mean temperature). The authors discuss the potential links between those variables.

Major comments: The methodology part (EEMD) is a repeat of already published material (Wu and Huang), but the presentation made by the authors is quite obscure, with ill-defined notations and no real effort at synthesizing the methodology and expressing its crucial elements.

The authors mainly repeat the description of the Huangye stalagmite data from the original publication, but they do not really discuss (or seem to doubt) how the isotopic record is relevant for their conclusions. The papers of Tan et al. mention correlations

between d18O and precipitation that are lower than 0.35 (from what I see in the data, this correlation is due to the trend of the last decades). Therefore, a lot of care should be used in interpreting and extrapolating such data.

There is no real discussion of what the cosmogenic isotope record means. The record used by the authors is a mix of several proxies, with different resolutions and temporal coverage. Therefore, the EEMD analysis might only tell something on the way the record was produced, and nothing about solar variability.

I am a bit surprised that the Fourier analysis of the authors does not find the same peaks as those found by Tan et al. (Palaeogeography, Palaeoclimatology, Palaeoecology 280, 432–439, 2009), with a very similar dataset and the same program for spectral analysis. Since a large part of the interpretation of data relies on those spectra, their instability casts some doubts on the results.

The spectral analysis results for the three records are very different. The authors seem to manipulate data until they find something that seems coherent. I do not approve of such a procedure.

In order to be complete, the authors should do the same exercise with volcanic activity, aerosol forcings, etc. The attribution exercise that is done here is very partial.

The authors make a prediction (p. 9, l. 7): "Meantime we predict the Asian monsoon is strengthening gradually and the Asian monsoon rainfall is increasing gradually in the next several decades or even the next 200 years, in $\sim$ AD 2180$\pm$30 the local climate will reach to the next wettest period." This cannot be serious. The data do not extend into the 21st century. Looking at the time series, I cannot see any hint temperature increases in the 21st century. And, this is were the correlation of $\sim$0.3 between precipitation and d18O might mean that precipitation reconstruction fails.

Minor comment: The English is very poor. The manuscript would have needed a cross check from a native English speaker.

I do not see how EEMD would give better or more insightful results than a wavelet decomposition (the ups and downs of the analysed signal are very symmetric).

The authors should be aware that Tom Crowley passed away in May 2014. Thanking him in the acknowledgments is rather strange.

Conclusion: For all the major comments, I am sorry to recommend a rejection of the paper.

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