Authors' Response to Anonymous Referee #2

We thank the referee for his (her) valuable comments and suggestions. Following is our response.

Referee's comment:
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 $\delta^{18}O$ 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.

Authors' Response:
Thanks for your comment. Yes, our presentation about EEMD is short. If the article can be accepted, we will make a more detailed description to the EEMD method.

Tan et al. (Palaeogeography, Palaeoclimatology, Palaeoecology 280, 432-439, 2009) said: "There is a significant negative correlation ($R=-0.385$, $N=33$, $P<0.05$) between the $\delta^{18}O$ of DY-1 and the annual rainfall."
This is a special case of DaYu Cave and Tan et al. analysed the possible reasons.
We cannot find the other correlations (mentioned by Tan et al.) between $\delta^{18}O$ and precipitation that are close to or lower than 0.35.

About Huangye Cave, Tan et al. (Holocene 21, 287-296, 2011) said in abstract: "We developed a composite oxygen isotopic record of cave calcite for the last 1860 years based on three stalagmites from the Huangye Cave in eastern Gansu Province, northern China. The $\delta^{18}O$ values reflect monsoon precipitation changes, with lower $\delta^{18}O$ values representing higher precipitation and vice versa."

Referee's comment:
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.

Authors' Response:
Thanks for your comment. Yes, the record used by us is a mix of several proxies, with different resolutions and temporal coverage. However the synthesized ^{10}Be record can be regarded as a proxy of solar output. We also tried to seach for other proxy such as ^{14}C for solar activity in the last 1000 years. But we can not find suitable record with a high resolution.

Referee's comment:
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.

Authors' Response:
Thanks for your comment. Tan et al. (Palaeogeography, Palaeoclimatology, Palaeoecology 280, 432–439, 2009) analysed the periodicities of the $\delta^{18}O$ record of DY-1 (fome DaYu Cave) by the power spectrum analysis, but they had not analysed the periodicities of the Huangye stalagmite $\delta^{18}O$ data.

Referee's comment:
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.

Authors' Response:
Thanks for your comment. In this paper, the cycles of the three records are mainly shown by EEMD. And these cycles are partly verified by the spectrum analysis results. We donot compare the spectrum analysis results of the three records.

Referee's comment:
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.

Authors' Response:
Thanks for your comment. Perhaps, we will further discuss the impact of volcanic activity, aerosol forcings, ENSO, greenhouse gas forcings, etc on the Asian monsoon in the later study. But this is not the focus in the article.

Referee's comment:
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 ~ AD 2180±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 0.3 between precipitation and d18O might mean that precipitation reconstruction fails.

Authors' Response:
Thanks for your comment. The comment is similar to the comment of Anonymous Referee #1. The following sentence will been added to the end of the Page 6:

In Figure 4, the curve has reached the top in AD2001, and began to show a downward trend. According to the change rule of the curve, it may be a possible trend that that $\delta^{18}O_R$ will become smaller and smaller in future decades, even in future 200 years, and maybe reach to the lowest in ~ AD 2180±30.

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

Authors' Response:
Yes, the English of the paper need to be polished further.

Referee's comment:
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).

Authors' Response:
The wavelet analysis has been widely applied in climate changes, seismic exploration, turbulence, radar monitoring etc in the past 30 years. It has good ability to make multi-resolution analysis in both time domain and frequency domain. Professor Lin Zhenshan (one of the authors) began to adopt wavelet analysis in the study of the atmospheric science twenty years ago (Lin Zhenshan: The wavelet and the hierarchy of climatic system, Met. & Atmo. Phys., 61, 19-26, 1996). Professor Lin has made some contribution to the application and promotion of wavelet analysis in China (Lin Zhenshan and Deng Ziwang: Research on Climate Diagnosis by wavelet analysis, Meteorology Press, Beijing, 1999). However, there are some important limitations as the following:

1) The choice of wavelet basis functions limits the applicability of the technique, as the basic functions of wavelet transformation are fixed and do not necessarily match the shape of the considered data series in every instant in time.
2) The periods deduced from wavelet analysis are often dependent on the empirical parameters. The parameters are dependent on both the methods and experiences by the authors.
3) There is boundary effect the wavelet analysis cannot avoid, which leads to the distortion of the period. Professor Lin has put forward a variety of methods to eliminate the wavelet boundary effect, one of which is the symmetric extension method that is widely adopted in China. Take the boundary line as the symmetry axis, and then copy the original sequence by using the symmetric axis as a mirror in the symmetric extension method. However, it is clear that any elimination of the wavelet boundary effect has its limitation, especially when there are not enough data.

The data in this paper are not long enough to overcome the false information caused by the boundary effect of wavelet analysis. While there are many advantages of EEMD method as pointed out in our paper, we think it is more suitable in this case.

Referee:

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

We were shocked at death of Thomas J. Crowley, and we are very sorry and sad.