

# ***Interactive comment on “Relationship between climate, environment, and anthropogenic activities in coastal North China recorded by speleothem $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$ ratios in the last 1 ka” by Qing Wang et al.***

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Author’s response to anonymous Reviewer#2 - “Relationship between climate, environment, and anthropogenic activities in coastal North China recorded by speleothem  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  ratios in the last 1 ka”

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Firstly, we would like to thank anonymous reviewer#2 for his/her comments and constructive suggestions, which will improve the manuscript, and for recommending this study for publication in *Climate of the Past*. Please find enclosed point by point responses to the comments. The referee suggestions and comments are displayed in red, and our answers in black.

### General

In their manuscript, Wang et al. tried to use oxygen and carbon stable isotope records in a stalagmite sample from Kaiyuan Cave to address the relationship between climate, environment and human activities. However, the manuscript was very poorly written and some parts are even unreadable (e.g., lines 25-30 in abstract and lines 23-25 in section 4.3). There are numerous grammar mistakes and redundancies in the writing (e.g., the first two paragraphs in section 3). Moreover, the authors often made awkward statements without reasoning. It is difficult for me to recommend its publication in *Climate of the Past*. Following I provide a few major comments and minor issues, and hope they would be helpful in authors' resubmission and future research.

Thank you for your comments. We will improve the expressions and consider to find language editing service by professional institution, and make the manuscript much easier to read.

### Major issues

1. I am surprised that the current manuscript has a significant overlap with their previous paper published in *Climate of the Past* (Wang et al., 2016, 12, 871-881), although the authors did include new datasets (e.g., 2 more U/Th dates,  $\delta^{13}\text{C}$  data) in this manuscript. Some paragraphs were even simply copied from the previous one (e.g., in introduction, background, ...). This is typically unacceptable in scientific journals.

In this manuscript, the stalagmite KY1 was dated by U-Th technique, and discussed the climatic-environmental meanings by  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$ . The  $\delta^{18}\text{O}$  ratios of

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the upper part of stalagmite KY1 has been discussed and published in Wang et al., 2016 Clim. Past. As for this problem, the discussions of  $\delta^{18}\text{O}$  ratios will be deleted substantially only in comparison with  $\delta^{13}\text{C}$  ratios in the next modification. All repeat contents will be improved.

2. The age model the authors constructed for the sample is not reliable. How could it be possible to assign a specific calendar year to a lamina, particularly given the slow growth rate and relatively large U/Th age error bar? Some U/Th dates were randomly thrown away, without careful reasoning.

The dating results of the upper part (0-42.769mm) of stalagmite KY1 has been published. The dating results of the lower part of stalagmite KY1 is established by the methods of interpolation and extrapolation. By the boundary of the position of 64.5mm, we calculate the average growth rate of the part of 42.769mm-64.5mm first, and then extrapolate the age of the position of 75mm by the average growth rate. The position of 45mm is much close to the boundary of 42.769mm, so we chose position of 64.5mm. The expressions need to be improved.

3. The authors really need to learn how to scientifically present their data. They need to add errors when present measurement data. Significant figures also need to be considered.

Thank you for your suggestion. We will check all the manuscript, and consider to modify the figures.

4. Section 4, it would be beneficial to show pictures of sample KY1, its lamina and micro-sampling locations.

We will add a picture to show the stalagmite and the sampling positions.

5. In section 4.1, why not re-measure the subsamples if the authenticity of the sample is uncertain?

The reason is technical limitation probably. We will check the error, and we will modify

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the impressions.

6. In section 4.2, it seems very odd to exclude the U-Th age at 45mm from the age model.

The dating results of the lower part of stalagmite KY1 is established by the methods of interpolation and extrapolation. By the boundary of the position of 64.5mm, we calculate the average growth rate of the part of 42.769mm-64.5mm first, and then extrapolate the age of the position of 75mm by the average growth rate. The position of 45mm is much close to the boundary of 42.769mm, so we chose position of 64.5mm. The expressions need to be improved.

7. In section 4.3, the authors observe a quite interesting feature that “The UAT and Solfar Caves, affected by the maritime westerly wind, showed the latest  $\delta^{18}O$  mutation time; the Wanxiang and Spannagel Caves, nearly unaffected by the ocean, showed the earliest time; and the Kaiyuan Cave, affected by the East Asian Monsoon, fell in between.” What’s the possible mechanism behind this phenomenon?

Different climate types (e.g. monsoon area) and different location (e.g. inland or near the sea).

8. In section 4.3, the relationship between solar activity and  $\delta^{18}O$  were not sufficiently discussed. The authors argued changes in  $\delta^{18}O$  are corresponding to the variations in solar radiation (although I would argue it is not visually convincing). However, the authors did not explain why the changes of  $\delta^{18}O$  lags the changes of solar radiation and why the changes of  $\delta^{18}O$  respond to solar changes differently before, during and after LIA. These statements are very subjective.

The relationship between solar activity and  $\delta^{18}O$  were not sufficiently discussed because of limited space. The comparison need to be improved. We are also considering to delete this comparison.

9. In section 4.4, the authors argue that the variations of speleothem  $\delta^{13}C$  is con-

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trolled by the changes of proportion of C3- vs C4-, bio-productivity and the water-rock interaction. First, how to quantify the water-rock interaction? by time? Second, why the low  $\delta^{13}\text{C}$  values before 1482AD necessarily “indicate a lower proportion of C3-versus C4-plants, lower bio-productivity, and less water-rock interaction” (page 9 line 10)? Third, “This behavior results in higher proportions of C3- versus C4-plants and increased bio-productivity.” (page 10 line 33), which according to the authors will decrease the speleothem  $\delta^{13}\text{C}$  values, contradicting to the increase trend of  $\delta^{13}\text{C}$  values between 1480-1744 AD.

In this section, the quantification of the water-rock interaction is considered by time. The expressions need to be modified. The low  $\delta^{13}\text{C}$  values before 1482AD was affected by the variations of natural vegetation. But the effects of anthropogenic activities were more and more prominent, and exceeded the natural factors finally.

10. In section 4.3 and 4.4, the relationship between the ancient Chinese dynasties and stable isotopes are very weak.

Thank you for your comment. According to the record of stalagmite KY1, paleoclimate and history records, we discussed the correlation between the stalagmite record and the replacement of major dynasties of ancient China. We will find much more results and evidences to further research and verification.

Minor issues Page 1 line 19, what does “smoother” mean here?

Thanks. This is a mistake. We will change this word to “more smooth”.

Page 2 line 25, the authors stated “The areas of eastern and northern China influenced by the southeast monsoon are likely to be warm, but not as warm as the areas of southwestern China that are influenced by the southwest Monsoon (Tan, 2007)”. But when did it happen? MWP or LIA, or both?

Thanks. We will add some expressions.

Page 2 line 28, what does it mean “dry to wet to dry”? Thanks. Dry to wet and back to

dry again. We will improve the expressions.

Page 2 line 30, “Sever studies (Tan et al., 2003).” Incomplete sentence.

Thanks. That’s two references. We will correct this sentence.

Page 4 line 15, “KY1 had uranium concentrations ranging from 704 to 5147 ppt”. These are in fact thorium concentrations.

Thanks. We will correct it.

Page 4 line 18, figure 4 appears earlier than figure 3.

Thanks. We will adjust the order.

Page 5 line 18, not correct to have so many digits for isotope values. Same problem appears throughout the paper.

OK. We will check the  $\delta^{18}\text{O}$  values.

Page 7 line 16, “Comparing the  $\delta^{18}\text{O}$  value curve to contemporaneous records of Swiss Alpine glaciation (Holzhauser et al., 2005) showed no obvious correlation (Fig.7C).” Then what’s the point to mention here?

Thanks. We are considering to delete this comparison.

Page 10 line 20, a clear definition is needed here for the “drought/waterlog index cumulative departure curve”.

The cumulative departure curve of drought/waterlog index has been written in the published article (Wang et al., 2016, 12, 871-881) clearly. So we don’t show it in this manuscript.

Please also note the supplement to this comment:

<https://www.clim-past-discuss.net/cp-2017-73/cp-2017-73-AC2-supplement.pdf>

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