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Climate of the Past Discussions

Interactive comment on "Hydroclimatic variations in southeastern China during the 4.2 ka event reflected by stalagmite records" *by* Haiwei Zhang et al.

Anonymous Referee #2

Received and published: 17 October 2018

General Comments: The manuscript describes a new, well-dated isotopic record of environmental change from the transitional period between the Middle and Late Holocene. Overall, the data appear high quality and collected/analyzed properly. The presentation of the material and interpretation of these data is generally good, but does require some additional thought and discussion. Some structure and figure design changes are needed, as indicated in specific and technical comments below. Generally, this review agrees with comments made by reviewer 1, although this reviewer believes the peat record to still be of importance as it offers evidence of replicated environmental change in a different proxy, despite chronological limitations. This reviewer believes that after restructuring (detailed well by reviewer 1) and addressing the con-

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cerns below (particularly about isotopic interpretations of wet/dry), the manuscript will be acceptable for publication.

Specific Comments: 44: 'specific level' is unclear/vague. Do you mean a specific physical level (like a layer at a depth) or a geochemical threshold?

47: Be careful about assigning direct causality between the climate changes and societal responses. For some better studied sites, a direct impact of climate change leading to societal collapse may be well-established enough to confidently state such. However, for many others it may be more accurate to highlight that the climate and societal changes coincided and were likely associated, but not certainly proven. It is also important to acknowledge that climate-societal interactions are usually much more complex than our simplified paleo-perspectives (e.g., there was drought, so therefore their society suffered and collapsed).

76: Data source for Guixi data?

77: A sentence further explaining the climatic set up and characteristics of the spring persistent rainfall would help here, since it seems to be an interesting and important regional characteristic

82: "Data from..." sentence is convoluted and difficult to read as is.

87: Is it important that it was found after days of heavy rain? Was it previously not open/accessible?

92: You have some taxonomic inconsistency reporting plants here: Pinus is a genus, Taxodiaceae is a family. "Camelliaoleifera" should be a binomial genus/species: Camellia oleifera. Bamboo is only given as a common name. Preferably, you should list plants on the same taxonomic level (probably just genus), and species level is probably not necessary for your discussion here. Also, Taxodiaceae is no longer a recognized plant family; it has since been absorbed into Cupressaceae.

129: Was evidence of hiatus examined petrographically? Or is this conclusion simply

based on the age distribution? The top 50 mm have a few petrographic boundaries I can see in Fig 3 that might be worth examining closer for short hiatuses petrographically (if you haven't already done so) (e.g., Railsback 2013).

131: The linear age-depth model looks sufficient. It might be worthwhile to age model with BACON or StalAge and see if that changes any results/interpretation significantly.

150: It may be useful to note that even stalagmites that deposit with kinetic fractionation can still preserve valuable climate data BECAUSE of the fractionation. So even if your stalagmite isn't in isotopic equilibrium, it can still have useful data (though your interpretation of the isotopes may be different).

165: "orbital" is not a timescale. Millennial timescales should suffice. However, on the timescale you are examining, orbital forcings are not a factor, so this is a somewhat weak/irrelevant point. Focus on what the literature says about controls on d18O for the decadal/centennial range you are examining.

171: "We suggest"- Are you suggesting that conclusion newly in this paper? Or was this the conclusion of Zhang 2018 you cite? If the latter, I would rephrase to simply state that data from E'mei cave concluded that EASM-NSM balance controls the d18O, and not say "we" concluded it.

171: Earlier (65) you said there was only one published stalagmite from SE China, but isn't Zhang 2018 another published record from SE China?

178: A sentence clarifying and summarizing how you are interpreting the d18O in Shennong Cave would be nice here, since you state several possible ways to interpret d18O for the region.

190: Your d13C summary is generally good. Some supplemental resources you might want to examine include Oster et al., 2010; Meyer et al., 2014; Noronha et al., 2015;Wong and Breecker, 2015 to get more recent studies and summaries on d13C.

200: I think your dismissal of the effects of degassing and PCP is premature. Some

degassing must occur in order for CaCO3 precipitation to occur (being deposited in perfect 'isotopic equilibrium' is impossible, since a system in equilibrium will not undergo any reactions or change). And the presence of soda straws and stalactites (which I assume are present in the cave) means PCP is also occurring. The negative relationship between d13C and growth rate suggest to me that PCP is perhaps quite important as a control. Perhaps more importantly, you could argue that vegetation dynamics are a major or the major control on d13C, but when multiple factors are working in concert (e.g., drier conditions both lead to less vegetation and greater PCP which both lead to higher d13C values), dismissing one or more potential factors is not even necessary.

214: Do you have any supporting evidence that the d18O for your stalagmite reflects annual precip (e.g., through drip water monitoring?) or is this an assumption? I think the match between it and Dongge make a decent argument that your stal is recording long-term aggregates rather than 'flashy' storm events. But how you decided that it is annual precip should be mentioned.

222: Wouldn't wet intervals be those with z scores less than zero? (Not greater, like you have written). Also, You earlier state that d18O is interpreted in your area as the ratio between EASM and NSM amounts, with lower values meaning a greater fraction of EASM. Shifting the seasonality of precipitation can therefore change the d18O in the stalagmite without actually changing annual precipitation amounts. Additionally, a stalagmite d18O that decreases could be because the EASM gets more intense (more overall rainfall), but also when the NSM decreases more than the EASM (less overall rainfall). Be careful about interpreting d18O as amount unless you have supporting evidence.

223: "More wet intervals": Wouldn't a better measurement be "more years wetter than average"? This sounds like you are just counting the number of times you have a span below 0 Z-score (so a highly variable record with many changes above and below average could easily have more 'wet intervals' than a record that is all wetter than average in a single long 'wet interval').

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227: Controlled by what variable of summer monsoon precip? Amount? EASM/NSM like your cave?

230: Growth rate is often not a direct function of precipitation amount (e.g., Railsback 2018). If you believe growth rate in your stal is a direct relationship to precip amount, some supporting evidence/arguments would be beneficial.

238: 150 years is a pretty long time to be a vegetational response delay in terms of vegetation coverage, particularly if there is not a significant shift in vegetation type. Do you have a more detailed explanation of why the vegetational response would take 150 years? Are there alternative reasons that could explain the lag? Perhaps d13C is showing actual precip amount changes, and the 'lag' is because the d18O can reflect proportional shifts in EASM/NSM that may not result in actual precip amount changes.

255:The previous paragraph contained records in monsoonal China covering the 4.2 ka event. Why are they separate from section 4.3?

287: While the argument linking EASM intensity to AMOC is sound, the IRD record is not particularly strong evidence since the variance in IRD between 3700 and 4500 yr BP is quite small. Are there alternative records for AMOC intensity you could use, or perhaps support this by bringing in records also showing monsoonal changes in Africa and South Asia at this time.

294: Are you calculating coherence, or do you mean the variables co-vary?

Fig 1: Map A is too far zoomed out and is difficult to see sites. Ideally would have main part of map and this figure focused on eastern China. Small inset map could provide wider context. Maps also need a legend identifying icons and color scheme for basemap. Highlight your site on main map better (e.g., larger text, unique color, pointing arrow). Another map or layer on this map showing typical modern location of the summer monsoon influence/extent would be beneficial. No scale on map B.

610: You bring up several more climatic influencing winds here that are never discussed

or mentioned in your paper. If they are important, they need to be discussed, or at least mentioned why you are not considering them.

Fig 4: Labeling the y axes with the environmental interpretation (e.g., wetter/drier, more intense EASM, etc) would aid the understanding of these plots

Fig 6: Labeling the axes with the cave name (or directly on the plot) along with in the caption would make the plot more readable. The coarse resolution of Xiangshui makes it very difficult for me to conclude anything about the covariation between it and your record. I do think the Dongge records visually matches well.

Fig 7: Labeling the axes with the sample/cave/site name (or directly on the plot) along with in the caption would make the plot more readable. Also, labeling the Y-axes with the environmental interpretation (wet/dry, monsoon N-S offset, etc) will help.

Fig 7: The yellow bars don't align well with your d18O record. Is there a reason they are offset from the low value intervals of your record?

Technical comments: 73: Your latitude/longitude is flipped

81: Shennong Cave or Shennong cave? Capitalization consistency. Also, this sentence seems unnecessary and out of place as you already mentioned that the cave is in the region of spring persistent rainfall.

160: Re-examine your use of commas in this sentence. It's unclear which phrases are meant to be grouped in the list of influences.

188: Prior not needed to be capitalized

196: Cave or cave? I think that Cave should be used when referring to specific named caves here and throughout, but it's more important for you to be consistent with capitalization.

211: 'wetter to drier conditions' is better, because there wasn't a major regime shift into definitively 'dry' conditions from earlier 'wet' conditions

255: "A remarkable drought" is better here than "the remarkable drought", since you haven't discussed the drought for the past few pages.

268: "the large dating uncertainties and the low resolution" Change to "by large dating uncertainties and low proxy resolution in many records"? or something more clear

610: Westerly used here sounds like you are saying westerly monsoon, but you are probably just referring to the westerlies, correct?

Figure 2: You may wish to recolor the portion with the red-green lines. Almost 1 in 10 people suffer from some degree of red-green colorblindness.

Fig 3: Just a design though: Your age markers are red on the plot, but black on the stalagmite. The red marks on the stalagmite are XRD. For consistency and ease of eye-matching of this figure, you might consider making the age markers on the stalagmite red and the XRD markers a different color.

Figure 4: You may wish to recolor the portion with the contrasting red-green Z score. Almost 1 in 10 people suffer from some degree of red-green colorblindness.

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