Clim. Past Discuss., 10, C693–C696, 2014
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Climate of the Past Discussions
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Interactive comment on "Variation in the Asian monsoon intensity and dry-wet condition since the Little Ice Age in central China revealed by an aragonite stalagmite" *by* J.-J. Yin et al.

Anonymous Referee #3

Received and published: 6 June 2014

The manuscript by Yin et al. presents a new 700-yr-long aragonite speleothem d18O record from a previously reported cave in central China. The speleothem has been well dated, and the resolution sufficient for such a high-resolution climate reconstruction. Whilst I have little doubt that the speleothem record does present some useful paleoclimate information of the region, it is apparent that the authors have not adequately interpreted the record to the degree required to explain some of the patterns observed in the reconstruction. Therefore, I do have significant reservations in recommending this manuscript for publication in Climate of the Past in its current form. Also, I found there were numerous sections of the paper that were difficult to follow and sentences that were not structured properly.

The structure of the paper is also misguided in my opinion. For example, why do the authors choose to leave it to the final section (4.4) to compare their speleothem record with other regional speleothem records? It seems more logical to begin the Discussion with this sectionâĂŤi.e. first demonstrating the monsoonal/climatic significance over the region, and then evaluating the mechanisms to explain the observed changes (e.g. Solar forcing, ENSO etc.).

In order for this manuscript to be considered for publication in this journal, the authors are required to provide a better appraisal of the d18O systematics in their speleothem, provide additional references as to the dominant controls on the d18O of modern and past rainfall in the region, and spend some time editing the paper to remove the numerous grammatical mistakes.

Major Comments:

1. Section 2: âĂć Suggest adding a figure showing the seasonal nature of rainfall at the surrounding meteorological stations. 2. Section 4.1: âĂć Beginning on Line 4, the authors say that “Changes in the summer monsoon intensity will affect all above factors”. How will monsoon intensity affect, air temperature, which will in turn affect cave temperature? The lead in to the next sentence does not make any sense: “Therefore, it is better to compare stalagmite d18O with the local instrumental weather records”. Better compared to what? âĂć There is mention that in Fig. 8, the “Lianhua d18O record is comparable to the Yichang rainfall record before 1940..”. I would say this relationship looks rather weak by eye. For example, the meteorological data show that at around 1936 there was a large spike in rainfall, which was quite a bit larger than the previous high rainfall events around 1900-1920. Looking at the d18O record, the d18O really do not shift by that much ($\sim$0.1‰ between $\sim$1925-1935 given an approximate 60-70% increase in annual rainfall. Hence, I am hesitant to attribute this d18O signal exclusively to the “amount effect”. The authors should at least provide the correlation coefficient between the d18O and rainfall data for the 1910-1930 period. âĂć I agree with reviewer #2 that the authors should also plot the previously published LH2 record by Zhang et al.

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(2013) to demonstrate that consistency in speleothem d18O is reached amongst several records from the same cave. This is a better test for isotopic equilibrium deposition than the Hendy Test. âĂć An adequate assessment of the d18O systematics between the site of vapor condensation and the site of aragonite precipitation in the cave is significantly lacking. I thus have significant reservations in accepting the interpretation of the speleothem d18O post-1940. Specifically, the authors claim that the lack of correlation between speleothem d18O and local rainfall amount after 1940 "may be explained by the opposite direction of rainfall and temperature changes". But, in Zhang et al., (2013) (and Johnson and Ingram, 2004) it is stated that "In monsoon-dominated low-latitude and tropical regions of monsoonal China, temperature effects are commonly insignificant because the small temperature-dependence of the oxygen fractionation between water and calcite ($\sim$0.23‰°C) in the cave (O'Neil et al., 1969) tends to be canceled out by the small, nearly equal, but opposite, relationship for air temperature and d18O of the rainfall in areas intensively influenced by summer monsoon". Hence, it seems unlikely that temperature can explain the d18O shift, especially because a shift of $\sim$4 °C (only a $\sim$ 1°C increase is observed) would be required to explain the $\sim$1‰ increase observed in the speleothem record. Rather than the speleothem d18O increase since 1940 being a function of temperature or local rainfall amount, could it be explain by a shift in the overall strength of monsoonal winds and associated shifts in "pre fractionation" of moisture advected to the study site?

I have nothing further to add regarding sections 42-4.4, as to what has already been raised by referees #1 and #2.

Minor comments: âĂć Section 4.1, first paragraph, Line 7: . . ."precipitations are" should read "precipitation is. . .". âĂć Figure 1: In the caption the authors mention that the arrows indicate "water vapor flow". Please be more specific as to what season water vapor flows from these two different sources. Also, these moisture trajectories are likely to have different isotopic signatures owing to their differences in moisture source and also different transport pathways. âĂć Figure 4: Is this annual precipitation? What is

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the source of the data? Please verify in the caption. âĂć Figure 8: In the caption, more details as to the source of the meteorological data is required. Is that large T spike at ∼1920 consistent with other meteorological records from the region? âĂć CAM (1981) citation in the Fig. 7 caption, and also listed in the main text, is not listed in the reference list.

Interactive comment on Clim. Past Discuss., 10, 1305, 2014.