Climate of the Past Discussions

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Comment on cp-2021-20

Anonymous Referee #2

Referee comment on "The 4.2 ka event in East Asian monsoon region, precisely reconstructed by multi-proxies of stalagmite" by Chao-Jun Chen et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-20-RC2, 2021

I find this article difficult to review. Throughout both the introduction and discussion, the authors make broad-brush arguments which are overly speculative, and in my opinion are not supported in this manuscript.

The manuscript presents an exceptional stalagmite archive with optimal U and Th characteristics for dating—2s age errors on individual U-Th samples of <20 years, with almost no 230-Th correction needed. The stable isotope record shows large, clear excursions within the ~2000 yr duration period, and additional Mg/Ca, Sr/Ca, and Ba/Ca ratios have also been measured, setting the stage for a strong manuscript. A robust, comprehensive analysis of the measured geochemical proxies is greatly lacking, however, making the manuscript weak, and difficult to review beyond the results section.

Some examples—

--The U-Th data reveal incredibly large variations in U content in the stalagmite over just these 2000 years… from 1,500 ppb to 25,580 ppb! Why is this? A U/Ca record alongside the Mg/Ca, Sr/Ca, and Ba/Ca records would have been a helpful addition.

--The d18O and d13C records on the mean age model show a very abrupt and large shift from heavier values to lighter values around 4330 yr BP, with d13C moving 5‰ lighter and d18O moving 2‰ lighter. [Note, a similar abrupt shift to lighter values appears to occur around 3500 y BP, though about half the amplitude.] The closest U-Th ages to the 4330 y BP shift (also the paper's stage I to stage II transition boundary) are at 4440 y BP and 4230 y BP (aka 100 y away from the transition), and indeed the age model presented in Figure 2 shows the 95% confidence band to be +/-100 yrs at 4330 y BP. With such a significant transition in the stable isotope record, and an archive able to produce U-Th ages with < +/-20 yr precision, it is a shame to not have a more precise timing on this abrupt shift in stable isotopes.

--The d18O and d13C records show gradual transitions between stages I, II, and III (excluding the stage I to stage II transition)… so how were these stage boundaries' ages defined?

--By eye it is easy to see the correlation between Sr/Ca and Ba/Ca in Figure S1 (r^2 = 0.89, Table S1). A crossplot between the two ratios would be a nice addition, showing the slope. Mg/Ca has two large shifts that dominate its record, which are not replicated in Sr/Ca or Ba/Ca (Figure S1). Mg/Ca does not have a linear correlation with Sr/Ca nor Ba/Ca (r= 0.22 and 0.17, respectively), so I do not see any evidence to support the authors' claim that all three ratios covary. As the authors chose to do a PC analysis, a P1 loading v P2 loading crossplot would have been useful to conclude which ratios are dominating the different components. The authors instead state (Line 189): "The variance of PC1 is 67.15% (Table 3). Therefore, we chose PC1 as the variable representing Ba/Ca, Sr/Ca, Mg/Ca". The authors then make the sweeping claim "There was significant correlation between PC1 and $\delta^{13}C$, $\delta^{18}O$, Ba/Ca, Sr/Ca, Mg/Ca (Table S1)." Yet the actual r values between Mg/Ca, d13C, and d18O and PC1 are only 0.48, 0.24, and 0.37, or 23%, 6%, and 14% variance explained, respectively. The authors also state that PC1 (I believe largely Sr/Ca and Ba/Ca) increased during stage I and III, and decreased during stage II, however in Fig. 3 it appears PC1 was gradually decreasing through stage I and II, with a sudden increase at the stage II-III transition, and continued gradual increase through stage III, in contrast with the authors' claims.