

Interactive comment on “A Stalagmite Test of North Atlantic SST and Iberian Hydroclimate Linkages over the Last Two Glacial Cycles” by Rhawn F. Denniston et al.

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As is described below, we are currently in the process of obtaining additional information from the caves and the stalagmites described in this study. We are requesting from the editor additional time in order to fully address the comments made by each of the reviewers. Thus, our responses should be considered preliminary and incomplete versions of the detailed and developed discussion that will accompany a later revised draft of this manuscript. Here we present the itemized comments by the reviewer followed by our response.

There is no well-defined structure to the manuscript.

C1

We are adding additional (sub)headings, including sections on the construction of a multi-year cave monitoring protocol and its results, and the potential drivers of oxygen isotopic variability.

There are too many hypothesis and ideas but with no clear background to support them. There are no descriptions of the caves from where the speleothems were sampled.

We are adding to the Supplemental Material maps of both caves. BG has never been mapped but we are currently working with an experienced team of cavers to create a map suitable for publication. A previously constructed map of GCL will be adapted for publication. More detail on the ages and isotopic compositions of the bedrock hosting each cave will be added, as will isotopic compositions of vegetation and soil organic carbon. In addition, we have recently (January 2018) visited both caves and collected additional environmental monitoring data (temperature, barometric pressure, humidity, and drip rates) acquired via data loggers over the past 1-3 years, and dripwater obtained over the recent months.

The correction factor for one cave is the crustal value and for the other is a value determined from the cave drip water, and the difference is substantial. What is the justification to use different correction factor? What can be the reasons, different host rock, soil type, vegetation? Or maybe determining the correction on present-day drips may not be the correct methodology?

The GCL samples contain low ^{232}Th abundances and are largely insensitive to the initial Th correction. Whether an initial $^{230}\text{Th}/^{232}\text{Th}$ ratio of 4.4 ppm or 13.5 ppm is used, the ages are nearly identical. And given that the caves are formed in different bedrock and that we lack independent constraints on the initial Th ratio, we use the default value of 4.4 ppm.

The authors need to put the Figure of the studied speleothems in the text, not in the supplementary material, and indicate the measured ages on the figure, and where the

C2

hiatus are. It is important to add petrographic images showing the altered region and regions of hiatus.

This figure will be adjusted as requested and moved from the Supplemental Material to the manuscript.

The d18O record follows closely the d13C record. The similar pattern suggests that d18O is also reflecting temperature and humidity, or storm track changes. The authors need to elaborate on this, not to conclude that many factors influence d18O and they include a sentence saying that d18O may be influenced by kinetic effects and evaporation. . . . If evaporation and kinetics would be a major process why there is a good correlation with d13C. These kinds of sentences need to be properly discussed. Thus although it is correct that many factors influence d18O, it is also true for d13C.

This is a fair point. We mention in the manuscript that changes in carbon isotopic ratios can represent a response to hydroclimate which can be associated with vegetation type or density, changes in soil microbial activity, PCP in the epikarst, shifts in drip rates and degassing of CO₂ from water into cave air, and kinetics. As for oxygen, d18O responds to circulation, rainfall amount, air temperature, and seasonality. Thus, during times of “drying”, one might expect to see increases in d13C and d18O if rainfall is diminished, rainy season is shorter, moisture source more proximal, or temperatures higher. The discussion integrating both d18O and d13C will be expanded and the d18O data will be included in the figures.

The authors measure the isotopic composition of precipitation and cave water, but prefer not to discuss the d18O of the speleothems, this is strange.

This is a fair point. Please see previous reply.

Why d234 is only shown for part of the record in Figure 6. I would like to see on Fig 6, superimposed also the d18O record.

We attempted to construct a composite figure showing all of the d234U data, but the

C3

image was confused by the offsets between individual stalagmites. The only stalagmite for which the comparison of d234U and d13C made sense was BGLR6, which spans the longest interval of time. We will, however, create a similar figure using the d18O data from this stalagmite.

It is clear that during the termination MIS6 to MIS 5 and a more coherent discussion is needed, not just hypothesis and suddenly bring d18O to explain seasonal biases.

We will expand the discussion of the 6/5e transition and also go into greater depth d18O regarding the origin of oxygen isotopic variability.

Did the authors performed Hendy test on those speleothems, do verify which of them might have not form in isotopic equilibrium since the repetition test does not work?

While we are not convinced that the Hendy Test is a reliable means of assessing equilibrium crystallization (as demonstrated convincingly in Dorale and Liu, 2009), we are performing Hendy tests to address this comment. These analyses are currently underway and the results will be presented in the revised version of the manuscript.

The manuscript is rather confused and a Table showing periods of non-growth can help. Did the authors take into consideration the error on the ages and age model in the final correlations with other proxies in Figs. 6 and 7?

The data were presented based solely on the age models.

The authors don't explore the very good and interesting data. The discussion is missing explanation on the correlation between d13C and 234U.

We will include a more thorough explanation of the links between d234U and d13C in the revised discussion. The d234U data are not meant to represent the same sort of fine-scale paleohydrologic record as d13C, however. The utility of d234U was as support for our contention that carbon isotopic variations reflected hydroclimate, which in turn were linked to regional SST.

C4

And why there are large changes in $\delta^{13}\text{C}$ during sometime intervals for which there are smaller changes in SST and in the percent of temperate trees?

We note in the manuscript that pollen obtained from Iberian margin cores is regionally sourced. One cannot compare at fine scales the changes in vegetation occurring over a cave to those occurring regionally. With that said, we do point out and attempt to explain intervals when SST and $\delta^{13}\text{C}$ appear to be decoupled.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2017-146>, 2017.