

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.

Anonymous Referee #1

Received and published: 17 December 2017

Rev of the manuscript:âÜ3

By Denniston et al., Iberia stalagmite I reviewed very similar manuscript by the same authors several months ago submitted to âÜQSR. Unfortunately I found that the authors did not respond to the comments. Reading âÜagain the manuscript I only can be more critical.âÜ The manuscript deals with the link of Iberian Hydroclimate and North Atlantic SST as âÜevident from speleothems d13C and 234U and growth dynamics of stalagmites from two âÜcaves in Western Portugal spanning the last 230 ka. âÜ This study presents new speleothems records from Portugal, a unique and important âÜregion exploring how the North Atlantic SST influences the pale-

[Printer-friendly version](#)

[Discussion paper](#)



oclimate and environment of the Iberian Peninsula. The speleothems record from this part of the world is important and new and should be published, but the manuscript needs major revisions. However, there is no well-defined structure to the manuscript. 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. 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 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 hiatus are. It is important to add petrographic images showing the altered region and regions of hiatus. The $d_{18}O$ record follows closely the $d_{13}C$ record. The similar pattern suggests that $d_{18}O$ is also reflecting temperature and humidity, or storm track changes. The authors need to elaborate on this, not to conclude that many factors influence $d_{18}O$ and they include a sentence saying that $d_{18}O$ may be influenced by kinetic effects and evaporation. . . . If evaporation and kinetics would be a major process why there is a good correlation with $d_{13}C$. These kinds of sentences need to be properly discussed. Thus although it is correct that many factors influence $d_{18}O$, it is also true for $d_{13}C$. The authors measure the isotopic composition of precipitation and cave water, but prefer not to discuss the $d_{18}O$ of the speleothems, this is strange. Why d_{234} is only shown for part of the record in Figure 6. I would like to see on Fig 6, superimposed also the $d_{18}O$ record. It is clear that during the termination MIS6 to MIS 5 and a more coherent discussion is needed, not just hypothesis and suddenly bring $d_{18}O$ to explain seasonal biases. 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? The manuscript is rather

[Printer-friendly version](#)[Discussion paper](#)

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 authors don't explore the very good and interesting data. The discussion is missing explanation on the correlation between $\delta^{13}\text{C}$ and ^{234}U , 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? As it is written the study of the speleothems record does not add new insight to the understanding of the relationships between SST and the Iberian Hydroclimate Linkages. The authors try to justify the speleothems record rather than explaining what is unique about the record. To summarize, I find the data important and new, but the style, the arguments, the discussion and the introduction to the paper very weak. The manuscript needs major revision before it can be accepted for publication.

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

Printer-friendly version

Discussion paper

