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Climate of the Past
Discussions

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.

1. Presentation of d18O values: My major concern is that the d18O values are only shown in the supplement although they - as is also acknowledged by the authors them-

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selves - show some similarity with speleothem d13C and SST off the Iberian margin. I agree that the interpretation of speleothem d18O values on these long time-scales may be not straightforward, but the same is true for d13C. Actually, speleothem d18O values should even be less influenced by local or drip site-specific effects than d13C values and show a more consistent signal. Thus, I am convinced that a combined presentation and discussion of the d13C and the d18O values (similarities/differences) would be much more informative for the reader and also result in a more robust climate record.

This fair point was raised by other reviewers. d13C is a local signal while d18O is a more (pan)regional one. Analysis of regional precipitation (i.e., the GNIP stations at Lisbon and Porto) reveals a complex paleoclimate signal in precipitation d18O. The pronounced seasonality of rainfall leads to strong statistical relationships between both air temperature and rainfall amount that are difficult to disentangle, particularly when spanning intervals of time, such as are contained within this record, in which the seasonality of precipitation likely changed. Nonetheless, we will add a more developed discussion of the d18O data to the manuscript.

2. Discussion of d13C values: The discussion of the d13C data in terms of climate variability is by far too short. The authors mention several processes potentially affecting speleothem d13C values, but then do not discuss which of these processes they consider most important for the observed orbital and millennial scale variability of their record. Is it vegetation density and, resulting from that, soil pCO2? Is it the degree of PCP? Is it drip rate and the resulting changes in disequilibrium isotope fractionation? Or a combination of all these processes? As mentioned above, a detailed comparison (maybe for individual stalagmites) with the d18O records could provide additional information. Even if it is not possible to identify one or two dominant processes, the discussion must be extended in order to present this important information to the reader.

We will expand the relevant discussion based largely on observations made during our multi-year cave monitoring program. However, we reiterate that without a considerably expanded (trace elements, dye tracing, daily/weekly drip water sampling) protocol, these questions are difficult to constrain.

3. General presentation of the data: The authors state several times in the MS that the d13C (and d18O) signals recorded in the different stalagmites agree in phases of concurrent growth. I agree that this is an important criterion to test whether the stable isotope signals reflects changes in past climate or are dominated by changes in the local karst system/cave. However, in the current form (Figures 6 and S2), it is almost impossible for the reader to judge how good this agreement really is. Please present the corresponding sections on a different time-scale (in several plots) or maybe even calculate correlation coefficients. This is the only way to clearly present the agreement and the differences in timing, evolution and absolute values.

This point was made by other reviewers, and new figures will be added to the revision to address this shortcoming in the manuscript.

4. Dating: The total record is based on 76 ages, and I absolutely acknowledge the associated amount of work and costs. However, whereas for some stalagmites (BG6LR), a large number of ages were determined, for others (BG68, BG67, BG41) only a few samples were dated (and even "dummy" ages inserted). As some of the records clearly show hiatuses that are not accounted for by the current age models (BG67, BG68, Fig. 5), I strongly suggest to date a few more samples (10 may be enough) to improve the age models and, in particular, to better constrain the timing of the hiatuses.

This point was made by other reviewers, and we are currently obtaining additional dates.

Lines 85ff.: The results of the cave monitoring, in particular the detailed drip rate data should be presented in the results section.

We are creating a new sub-section within Results that is dedicated to our multi-year

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cave monitoring data. And as previously noted above, we will include cave maps.

Lines 120-121: Please provide more details about the "isotopic analysis of cave drip water" (methods, results, number of samples, etc.).

This information will be included in the cave monitoring section discussed above.

Line 171: MIS 9 should be MIS 7?

The reviewer is correct and this change has been made.

Line 178 ff.: "The similar carbon isotopic trends and values across many of the areas of overlap argue for a consistent climate signal as the primary driver of isotopic variability (Fig. S2)." As stated in my general comment, the similarity is not visible in the current diagrams. Please provide more detailed plots for the corresponding time intervals.

This point was made by other reviewers, and we will create new plots that better display intervals of overlap.

Line 189ff.: "rate of CO2-degassing from water entering the cave" This is a very common mistake in the speleothem literature. The rate of degassing is always very fast. The degree of degassing, however, which is determined by cave pCO2, has an influence on supersaturation and precipitation rate, which in turn may result in disequilibrium effects. It should also be noted that the degree of disequilibrium will be modulated by drip interval, with long intervals resulting in a higher degree of disequilibrium.

These are fair points and are in agreement with the basic thesis of the study, which is that through any of a number of drivers, changes in stalagmite d13C reflects hydroclimate that is, in turn, linked to SST. We will expand/correct the discussion to reflect Reviewer 4's specific comments here.

Line 199ff.: "Thus, increases in carbon isotopic ratios are interpreted here as primarily reflecting a combination of desaturation of voids above the cave and decreased organic CO2 production within the soil zone, both of which are consistent with a vegetative response to cooler, more arid climates (Baker et al., 1997; Genty et al., 2003).” Although I generally agree with the interpretation of the authors that more positive d13C values probably reflect drier conditions, this conclusion requires more discussion of other potential processes (see my general comment). The only process that is reasonably excluded are changes in vegetation type (C3/C4) based on pollen evidence. All other processes (changes in drip rate, supersaturation, ageing of organic material in the soil, etc.) are mentioned, but not discussed at all.

Our discussion of controls on origins of stalagmite carbon isotopic variability will be expanded considerably.

Line 207ff.: “Decreases in effective precipitation and/or bedrock dissolution rate, both of which are associated with increased aridity, have been tied to elevated speleothem δ234U values (Hellstrom and McCulloch, 2000; Plagnes et al., 2002; Polyak et al., 2012), and are interpreted similarly here.” Even if this has been discussed elsewhere, for the interested reader, it would be good to briefly (2-3 sentences) mention the underlying process here.

This portion of the discussion will be expanded in order to better clarify the links between d234U and hydroclimate.

Line 211ff.: “As differences in δ234U values between stalagmites may arise from distinct infiltration pathways, we restrict this part of the analysis solely to stalagmite BG6LR, which represents the longest individual stalagmite record of this time series.” The same argument holds true for d13C values, which may also strongly depend on differences in infiltration pathways. Differences between the individual stalagmites (in agreement between d13C and d234U) may even provide additional information about the processes occurring in the karst. Please show all the d234U records.

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

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the longest interval of time. We will, however, add the requested figure to the revision.

Line 215ff.: Even if the interpretation of the d18O values may be difficult, I am convinced that they contain important information, which should be presented to the reader and discussed in detail (see general comment).

This point was made by other reviewers, and as discussed above, we will considerably expand this section to address these concerns.

Line 231ff.: “The consistency and coherence among carbon (and oxygen) isotope values of coeval stalagmites . . .” Again, this must be presented in a more comprehensive and quantitative way. In the current form, the reader simply has to believe this statement.

This point was made by other reviewers, and as discussed above, we will considerably expand this section to address these concerns, including through a new figure that focuses on the areas of overlap.

Line 233ff.: “. . . the most notable of which is the shift toward higher δ234 C values at the MIS 5e/6 transition (âĹij130 ka) in stalagmite BG611 that contrasts with the sharp decrease in carbon isotopic ratios in BG67 (Fig. 6).” The corresponding growth phase in stalagmite BG611 appears very short to me (just a few stable isotope data points). Thus, I would not give too much weight to this section of the record. This again highlights the necessity to present the data on a different age scale better showing the details of the record.

This is a fair point and the wording will be changed to reflect the limited number of data points in BG611.

Line 264: HS11 should be HS6?

The reviewer is correct and this change will be made.

Line 330ff.: “This early interglacial peak . . .” Please highlight the corresponding feature

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in Fig. 7. It is not clear to me which peak is meant.

This section of the plot will be highlighted.

Line 333: Fig. 2 should be Fig. 1?

This change will be made.

Line 335ff.: "Next, stalagmite δ13C values are lower during GI 20-22 (MIS 5a/4; 84-72 ka) than in either the Holocene or MIS 5e, suggesting that maximum warmth and precipitation were not coincident with peak summer insolation (âĹij127 ka) (Fig. 6)." I strongly disagree with this statement. In particular on these long time-scales, differences in absolute d13C values should not be interpreted in terms of the warmest/coldest or the driest/wettest period. As the authors acknowledge themselves, a variety of parameters may change on these time-scales (karst properties, vegetation type and density, cave ventilation, etc.). Thus, the absolute values should be interpreted with caution.

The reviewer is right in that we should not over-interpret the records. Even in a data set where consistency is, to some degree, tested by overlapping stalagmites, it could be easily true that there are differences in the absolute values between the stalagmite isotopic ratios over time. We will change the discussion accordingly.

Line 348: ". . . than expected based on the observed scaling with SST (Fig. 7)." This is an interesting point, which should be extended in a revised version of the MS. How good is this scaling for the whole record? It may be interesting to see a scatter plot of speleothem d13C vs. SST. In this context, how about the relation between MIS 7 and MIS 5? In the speleothem record, MIS 7 exhibits lower d13C values than MIS 5, which is not the case in all other climate records presented in the paper (Figs. 6 and 7).

This is an excellent suggestion! We will create a scatter plot of SST and binned d13C values for the revised manuscript.

Line 351ff.: "Alternatively, changes in the nature of the NAO . . ." I would remove the

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whole discussion on the NAO, which appears rather speculative to me. The NAO is an inter-annual phenomenon, and even if some studies have suggested persistent phases of NAO+ and NAO- in the past, a discussion on the millennial or even orbital time-scale is difficult. Furthermore, this would provide more space for a detailed presentation and discussion of the d18O values and the potential processes influencing the stable isotope signals.

As previously discussed in our response, we agree with this criticism and will minimize this portion of the discussion in the manuscript. However, as discussed in the manuscript, the potential for changes in NAO mean state has been explored for the last millennium and for stadial/interstadial intervals, so we feel NAO belongs as a part of the discussion of the data.

Line 381ff.: "Differences between the structure of the stalagmite and SST records during some time intervals suggest that land-sea connections across Iberia may have varied temporally and spatially." This statement goes too far (see above).

Discussion of the NAO will be minimized in the manuscript. However, it is clear that climatic boundaries (in the sense of Köppen) are non-stationary and that we have to anticipate variability with respect to maritime or continental influences at a given location or, for Portugal, between Csa and Csb climates.

Line 667ff.: "Conservative errors were added to account for the unknown "true" age of the stalagmite at these points." What do you mean by "conservative" errors? Please explain and motivate in detail how those were defined.

The term "conservative error" describes here an error that is as large as possible without causing stratigraphic inversion with respect to the bounding ages. In other words, the outside edge of the error window in the dummy date allows for a minimum of zero temporal offset (i.e., no hiatus), and this error is assigned to both the + and – side of the dummy age. Although one could consider estimating the dummy age error with the average error of the other dates, we believe that this could suggest higher precision

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than is warranted (especially as radiometric errors can vary drastically even on small distance changes). It would be prudent to assume smaller errors that are not based on a measurement. This portion of the age modeling technique will be added to the revision.

Fig. 3: Due to the long residence time of the water in the aquifer above the cave, the d18O signal of precipitation is smoothed (at least to some extent). Thus, instead of monthly means, it would be better to show the inter-annual variability and relationships.

Our multi-year cave monitoring program suggests that the residence time of water above the cave is short, likely days to weeks. This information will be included in the revised and expanded discussion of cave monitoring.

Fig. 4: Rather than showing just one year for NAO+ and NAO-, it would be better to show a mean state of all NAO+ and NAO- years within a specific period (e.g., the last 50 years).

Discussion of the NAO will be minimized in the manuscript, but we will add a third panel to this figure that shows the mean state of NAO over the last 50 years.

Fig. 5: Check labelling of the plots. Some speleothem names are different than in the text.

Thanks for catching this. Will correct the labels.

In addition, it appears to me that some of the samples contain apparent hiatuses (e.g., BG67), which are not resolved by the current dating and, thus, not accounted for by the age models. Therefore, I strongly recommend to determine a few more ages to improve the chronologies of these samples and to better constrain the hiatuses.

As previously discussed, we are obtaining additional dates, however several of these stalagmites are characterized by several changes in drip position that may or may not coincide with a cessation of growth. Short hiatuses (perhaps lasting as little as a few decades) may not be resolved by dating and thus must either be assigned a somewhat arbitrary duration (as constrained by the age model) or plotted as though growth was continuous.

Suggested additional references discussing climate variability on the Iberian Peninsula and in the Mediterranean as well as the timing of orbital and millennial scale climate change:

We thank the reviewer for these additional references and will incorporate them where appropriate.

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