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Climate of the Past Discussions

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Interactive comment

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 #4

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General comment:

This paper presents a long discontinuous palaeoclimate record covering the majority of the last 230 ka based on seven speleothems from Portugal. The d13C record shows substantial similarity with SST off the Iberian Margin on orbital to millennial time-scales suggesting that speleothem d13C values reflect past climate variability. It is supplemented by d234U and stalagmite growth resulting in an, overall, consistent picture with more negative d13C and d234U values for periods of warmer SST. In contrast, colder periods, such as the HS and the GS are reflected by hiatuses (in some cases) and more positive d13C and d234U values. In general, this is a nice speleothem record

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based on several stalagmites and a large number of U-series ages, and its potential as a climate proxy is evident. Thus, this record should eventually be published.

However, I have several problems with the paper in its current form:

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 themselves - 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. 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. 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. 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.

In summary, the paper is well written, the data are interesting and have the potential to make an important contribution to reconstruction of terrestrial climate change on the Iberian Peninsula. However, in its current form, I cannot recommend the paper for publication in CP due to the points mentioned above. Further, detailed comments are provided below.

Detailed comments:

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

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

Line 171: MIS 9 should be MIS 7?

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.

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.

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.

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.

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.

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).

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.

Line 233ff.: “. . . the most notable of which is the shift toward higher δ234 C values at the MIS 5e/6 transition ($\sim$130 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.

Line 264: HS11 should be HS6?

Line 330ff.: “This early interglacial peak . . .” Please highlight the corresponding feature in Fig. 7. It is not clear to me which peak is meant.

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

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 ($\sim$127 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

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caution.

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).

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

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).

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.

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.

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).

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

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:

Other Spanish speleothem records: Muñoz-García MB, Martín-Chivelet J, Rossi C, Ford DC, Schwarcz HP (2007) Chronology of Termination II and the Last Interglacial Period in North Spain based on stable isotope records of stalagmites from Cueva del Cobre (Palencia). Journal of Iberian Geology 33(1): 17–30.

Rossi C, Mertz-Kraus R, Osete M-L (2014) Paleoclimate variability during the Blake geomagnetic excursion (MIS 5d) deduced from a speleothem record. Quaternary Science Reviews 102: 166–180. doi:10.1016/j.quascirev.2014.08.007.

Fletcher WJ, Sánchez Goñi MF (2008) Orbital- and sub-orbital-scale climate impacts on vegetation of the western Mediterranean basin over the last 48,000 yr. Quaternary Research 70(3): 451–464. doi:10.1016/j.yqres.2008.07.002.

Environmental changes in SE Spain Candy I, Black S (2009) The timing of Quaternary calcrete development in semi-arid southeast Spain: Investigating the role of climate on calcrete genesis. Sedimentary Geology 218(1-4): 6–15. doi:10.1016/j.sedgeo.2009.03.005.

For the transition from MIS 6 to MIS 5: Regattieri E, Zanchetta G, Drysdale RN, Isola I, Hellstrom JC, Roncioni A (2014) A continuous stable isotope record from the penultimate glacial maximum to the Last Interglacial (159–121 ka) from Tana Che Urla Cave (Apuan Alps, central Italy). Quaternary Research 82(02): 450–461.

doi:10.1016/j.yqres.2014.05.005.

MIS 6.5: Bard E, Delaygue G, Rostek F, Antonioli F, Silenzi S, Schrag DP (2002) Hydrological conditions over the western Mediterranean basin during the deposition of the cold Sapropel 6 (ca. 175 kyr BP). Earth and Planetary Science Letters 202(2): 481–494. doi:10.1016/S0012-821X(02)00788-4.

MIS 7: Spötl C, Scholz D, Mangini A (2008) A terrestrial U/Th-dated stable isotope record of the Penultimate Interglacial. Earth and Planetary Science Letters 276(3-4): 283–292. doi:10.1016/j.epsl.2008.09.029.

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