Dear Dr. Combourieu-Nebout,

We submit for your consideration the revised version of our manuscript “A Stalagmite Test of North Atlantic SST and Iberian Hydroclimate Linkages over the Last Two Glacial Cycles”. We received from four reviewers detailed and expansive suggestions for improving this study, and we have implemented the vast majority. As a result, this manuscript has been greatly improved and expanded and is now ready for reconsideration by *Climate of the Past*.

In the following section, we address, point-by-point, the comments made by each reviewer. Comments are italicized and our responses immediately follow. Because changes to the manuscript are so extensive and, in many cases, our responses to individual reviewer’s suggestions are dispersed throughout the manuscript, we do not describe here in detail the specifics of our responses to every comment, but ask you and the reviewers to evaluate the revision in its entirety.

Sincerely,
Rhawn Denniston

Reviewer 1

There is no well-defined structure to the manuscript.
We have added additional headings and subheadings, and have substantially expanded the content of the original sections.

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.
One of the caves described in this study (BG) was unmapped when we originally submitted our manuscript, but we have now obtained a detailed cave map for this publication. Maps and vertical profiles for both caves are now included as a figure in this revision. In addition, considerably more detail on the geology and environmental setting of each cave has now been added.

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 are not particularly sensitive to the initial Th correction, as we now demonstrate in the manuscript. Whether an initial $^{230}Th/^{232}Th$ ratio of 4.4 ppm (the default value in many studies) or 13.5 ppm (the value calculated from cave dripwater) is used, the ages are similar, and the resulting ages do not impact our interpretations.

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.
Images of the stalagmites has been moved to the body of the text and includes the U/Th dates and demarcations of the zones of alteration.

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.
We have substantially expanded the discussion of the drivers of $d^{13}C$ and $d^{18}O$ values and variability.

The authors measure the isotopic composition of precipitation and cave water, but prefer not to discuss the d18O of the speleothems, this is strange.
We have dramatically expanded our discussion of oxygen isotopic ratios in the stalagmites as well as in precipitation and cave dripwater. This includes a discussion of isotopic values of dripwater, plate-grown calcite, bedrock, and vegetation, with the associated data in a supplemental table.

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.
Offsets between $d^{234}U$ values in individual stalagmites complicate their integration into a single cohesive time series. The only stalagmite for which the comparison of $d^{234}U$ and $d^{13}C$ made sense was BGLR6, which spans the longest interval of time, and this figure is now included in the text. We have, however, plotted each stalagmite's $d^{234}U$ vs its $d^{18}O$ and $d^{13}C$ and placed this in the Supplemental Material.

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 have expanded the discussion of the 6/5e transition and also go into greater depth regarding the origin of oxygen isotopic values and 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 in Dorale and Liu, 2009), we performed Hendy tests to address this comment. The results of these analyses are presented in detail in the revision.

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. We did not add another table as addressing the reviewers' concerns required increasing the number of figures. However, we have attempted to more clearly represent the timing of hiatuses in the text.

The authors don't explore the very good and interesting data. The discussion is missing explanation on the correlation between d13C and 234U.
We now provide a more thorough explanation of the links between $d^{234}U$ and $d^{13}C/d^{18}O$ in the revised discussion. As we point out, the $d^{234}U$ data are not meant to represent the same sort of

fine-scale paleohydrologic record as $d^{13}C$, but serve instead to support our contention that carbon and oxygen isotopic variations reflect hydroclimate, which in turn are linked to regional SST.

And why there are large changes in d13C during sometime intervals for which there are smaller changes in SST and in the percent of temperate trees?
This is an interesting question that we address in the manuscript. We have tried to raise this and related questions more clearly in the revision. The most likely answer is that the pollen is sourced from a large region while the stalagmites record changes at the scale of a single cave.

Reviewer 2
Chronology. The six speleothems used for this study are complicated samples in terms of growth axis (very variable along the samples), evidences of dissolution, minor and major hiatus, etc.). In fact, the growth of the six speleothems is very discontinuous and thus making difficult the detection of all the hiatus by U-Th dates. I see two possible ways of improving the chronologies that should be carried out by the authors. First, more dates are necessary in some stalagmites and, this time, analysing a higher amount of calcite would be desirable (I already pointed out this in my previous review. . . 50-150 mg for U-Th dating is insufficient with samples where U concentration is low as it happens here). Sampling a higher amount is possible and necessary to get more accurate dates. Errors of above 2000 years are common in Table 1 and I think they can be improved.
We acquired several additional dates using the larger sample sizes suggested here. These dates refined, to some degree, the age models, which were recalculated using these new dates.

Second, I suggest including some petrographic analyses (thin slides) to help on the identification of hiatus. I am not sure if the authors have done that study since it is not shown but comments on the textures and fabrics are made on 146-157 lines. A figure on this issue in the Supplementary material would be desirable.
We have relied primarily on the U-Th dates to construct the age model. Identifying hiatuses using thin section petrography would not necessarily allow a more accurate age model as the duration of the hiatus cannot be determined independently from the U-Th dates. We therefore feel that the in-depth petrographic analysis of these stalagmites is beyond the scope of this study. We have, however, included in the supplemental material images of several hiatuses to allow the reader a better sense of their macroscopic petrography.

Additionally, I would like to see a figure with all the age models together (an example is provided in Fohlmeister et al., 2012) to show the intervals that are really replicated.
A figure showing depths versus ages (with errors) for each stalagmite has now been added to the Supplemental Material.

The authors emphasized along the manuscript the good replication of this dataset and I cannot agree with that. They refer to Fig S2 many times to show replication in d13C records. . .. And in that figure it is evident that replication is really minimal (very short periods and not well reproduced patterns). The authors have to focus their interpretations where chronology was better assessed and replicated and be very cautious where the presence of hiatus was not so well replicated. In fact, sentences like "deposition of multiple stalagmites was punctuated by hiatuses of similar time spans. . ." (lines 181-184) should be avoided (they are not true) and need to be

more concrete: I just see one interval where two stalagmites stop growing at the same time, at ca. 100 ka BP.
We have expanded the discussion related to the overlap of coeval stalagmites, including isotopic values and hiatuses. In addition, we added to the supplemental material a figure showing the carbon and oxygen time series for coeval stalagmites.

Other example: "The reproducibility of carbon isotope ratios between coeval BG stalagmites argues that their d13C values may be viewed as an integrated time series not substantially impacted by inter-sample isotopic offsets" (lines 223-225).
Please see our response to the previous comment.

There is also highlighted the coincidence with hiatus in S France and N Spain stalagmites (lines 291-296) and this is not always true (Figure 6).
As mentioned above, we have expanded our discussion of the timing of hiatuses in the Portuguese and regional records, and hope that this makes more clear the general overlap of hiatuses in some but not all intervals.

Present-day cave environment. To my knowledge, this is the first time paleoclimate records from these two caves are presented. Then, it should be mandatory to understand present-day processes that would help to interpret past records. For example, distinguishing if the correlation of hiatus is with cold or with dry events (or both) is important and must be supported by more present-day data.
It is a tall order to require modern cave monitoring to interpret hiatuses from the last glacial. Climatic boundary conditions have changed dramatically since the Pleistocene. However, we have attempted to address this question in the revised manuscript.

The authors need to understand what is happening today regarding calcite precipitation in the cave. Does it happens the whole year or focused in the rainy season? Is it more abundant during warmer years? In lines 238-239 it is said that "any seasonal biases in calcite crystallization remain poorly constrained". Then, how can they link the data to NAO that is a winter process? I think that some interpretations will be better supported by more monitoring data.
We now include detailed discussion of the isotopic composition of dripwater and plate-grown calcite from BG. However, the reviewer's point is a good one and we have dramatically reduced the discussion of the NAO in the revision.

Besides, the switch of 2 per mil in the d13C record from sample CGL6 "for ease of comparison to SST" needs a justification in the text, not a simple note in the figure caption. I do not think such shift in measured values is justified at all without a deeper understanding of the cave environment (soil, host rock, etc).
With the new U/Th dates on GCL6, the resulting age model has essentially eliminated this offset. We have expanded the discussion of these considerations, however, by including bedrock $d^{13}C$ values and modeled stalagmite $d^{13}C$ values. The vegetation over the cave has been replaced by agricultural eucalyptus in recent years and very little active calcite is forming in GCL. As a result, it is difficult to model stalagmite carbon isotopic values. However, bedrock $d^{13}C$ values between BG and GCL are only ~1‰ apart.

Representation of data. Finally, I also have some concerns on the representation of data versus age. In general, I find too "optimistic" sentences or interpretations in the text that are not always easy to seen in the figures. A good example is Figure 6 that is used to emphasize the excellent correspondence of d13C from the stalagmites with other records but the scale does not permit to see it!! Examples: how can we see the positive change during the YD (lines 299-300)? How can we see the hiatus at 80-78 ka (lines 292-293)? What about the "effective moisture from 170-160 ka and 145-135 ka? (lines 303-304). Figure 6 needs more ticks in the x-axis to follow the text and some dashed lines or bars to help the reader to find in the figure the events indicated in the text.
These are all good points. We have added to the revision a figure that divides the BG record into four shorter intervals and plotted the stalagmite $d^{13}C$ and $d^{18}O$ against Iberian margin SST.

Regarding representation of data, I also missed some other records that are cited and compared in the text several times, such as Villars cave or many other marine records. Fig. 8 where a zoom is shown for two different intervals would be the place to include those other records. If not, the reader has to go to previous references to compare visually other figures with this new dataset. For the YD, for example, there are many other records available.
We tried but were not able to include these data on existing figures without overly complicating them. As the manuscript already has a large number of figures, we have left this concern unaddressed.

Additionally, I have not found in the text any explanation about the representation of pollen data. Is that a combination of records? A stack? How is it made? And regarding the representation of ice cores, why do not use the "real" ice core for the beginning of the record? The older part can be compared to the synthetic curve, but for the 0-125 ka I suggest to include NGRIP record.
The pollen data are indeed from multiple cores, and this point is now more clearly made. Per the reviewer's suggestion, original NGRIP data are now used from 0-122 ka, while the synthetic Greenland record is used for the remainder.

Minor remarks: - line 285 and line 293. Why Fig. 2?? This is certainly a mistake, I am afraid.
This change has been made.

line 119-120: explain the correction you did using cave drip water
A more detailed description of these methods is now included in the revision.

Table S1.There are many reversals not explained in the text.
Table 1 has now been moved to the body of the manuscript, and the expanded figure of images of the stalagmites that now include U/Th ages demonstrate the stratigraphic consistency of the age model when considering the error envelopes.

Reviewer #3
Denniston et al provide a new and long δ13C and d234u reconstruction of hydroclimate from two caves in Portugal. The stalagmites are securely dated, but have many hiatuses which may be related to climatic variation. In general, warm conditions are associated with lower δ13C values, suggesting enhanced soil productivity and or decreased prior calcite precipitation, among several hypotheses for the controls on δ13C. Where available, the d234Ui values show

similar variations to the δ13C, suggesting the record is one of effective moisture. I find the paper to be clearly written and well-documented, with a copious degree of reconciliation with the literature that attempts to integrate multiple lines of evidence for hydroclimatic change over Iberia. The presentation does not require improvements in general. However, I wonder if this paper would have more of an impact if it were 2/3rds the length and focused primarily on the record at hand, and its δ13C correlation to the marine SST records? The level of detail is appreciated but may detract from what is by all other accounts is a great record.
We thank the reviewer for these comments. Given the comments by reviewers 1,2, and 4, however, we have opted to expand the manuscript in order to better develop the data and interpretations.

Reviewer 4
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.
As previously discussed, the origins of carbon and oxygen isotopic values and variability are explored in considerably greater detail in the revision.

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.
As mentioned above, we have substantially expanded the discussion of isotopic values and variability.

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.

As mentioned above, this issue has been addressed through the addition of new figures and an expansion of the relevant discussion in the manuscript.

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.
As mentioned above, additional dates were obtained and all age models were re-evaluated based on these new data.

Lines 85ff.: The results of the cave monitoring, in particular the detailed drip rate data should be presented in the results section.
Drip rates are discussed (briefly) and presented in the associated figure.

Lines 120-121: Please provide more details about the "isotopic analysis of cave drip water" (methods, results, number of samples, etc.).
These data are presented in considerably more detail in the revision, and a supplemental table with this information is now included.

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 change has been made as mentioned above.

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.
We have changed this phrase in the manuscript.

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.
As mentioned above, our discussion of these effects has been substantially expanded.

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.
The relevant discussion has been substantially expanded.

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.
A new plot has been added to the supplemental material with $d^{234}U$ values plotted against $d^{13}C$ for all stalagmites.

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).
As mentioned above, the discussion relating to the values and variability of speleothem $d^{18}O$ has been substantially expanded.

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 step has been taken. Please see above.

Line 233ff.: ". . . the most notable of which is the shift toward higher δ234 C values at the MIS 5e/6 transition (□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.
This is a fair point and the wording has been changed to reflect the limited number of data points in BG611 and the associated uncertainties.

Line 264: HS11 should be HS6?
The reviewer is correct and this change has been made.

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

We feel that this already busy figure would be made even more complicated by denoting the early interglacial peaks with an arrow or asterisk and have thus left this figure unchanged.

Line 333: Fig. 2 should be Fig. 1?
This change has been 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 (⍰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 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 have changed 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).
We created a scatter plot of SST and $d^{13}C$ (and $d^{18}O$) values for the revised manuscript. This figure is discussed in the text and presented in the supplemental material.

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.
As previously discussed in our response, we agree with this criticism and have substantially reduced the discussion of the NAO in the manuscript. However, the potential for changes in NAO mean state has been explored for the last millennium and for stadial/interstadials, and thus we feel that the NAO belongs as a small component of the manuscript.

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).
We agree. This statement was removed from the manuscript.

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.
This information has been added to the manuscript.

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.
While poorly constrained, we argue that the residence time of water above the cave is short, likely weeks. We have thus left the presentation of precipitation isotope data largely the same, albeit with caveats associated with biases introduced by our irregular dripwater sampling schedule.

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).
This figure has been changed as suggested.

Fig. 5: Check labelling of the plots. Some speleothem names are different than in the text.
We have altered existing plots and created several new ones but will make sure to double check for labeling mistakes.

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 mentioned above, additional dates were obtained and growth/age models recalculated. Also the manuscript now includes a more expansive discussion of issues surrounding short-lived hiatuses and their impact on age models.

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 have incorporated them where appropriate.