September 21, 2018

Dear Dr. Combourieu-Nebout,

We submit for your consideration the second revision of our manuscript "A Stalagmite Test of North Atlantic SST and Iberian Hydroclimate Linkages over the Last Two Glacial Cycles". The first revision was evaluated by two reviewers, one of whom argues that the manuscript was acceptable for publication in its current form. The second reviewer offered a series of suggestions for restructuring the manuscript, clarifying some points in the text, and improving some of the figures. We address these edits below.

Sincerely,
Rhawn Denniston

1) From figure 6 it can be seen that besides stalagmites BG67, and BG6LR, also GCL6, and BG66 (the part dated at 219 kyr) show evidence of recrystallization at the growth axis. This does not necessarily mean that the record cannot be trusted, because it is unclear when the recrystallization took place (i.e. it could be shortly after initial deposition). Nevertheless, it requires caution with the sampling for the chronology and for the C and O isotopes. Therefore, it is essential that the sample positions of both dating and isotope samples need to be shown clearly. Where sampling was done at the growth axis in recrystallized parts these should be replicated by sampling on the left or right of the growth axis. This is not necessary for the entire stalagmite, but it has to be shown that the isotope signals and chronology are robust and not affected by recrystallization.

We have updated the manuscript text to note the possibility of alteration in parts of BG66 (alteration of GCL6 is already in the manuscript). The discussion of early post-depositional alteration, which does not meaningfully impact U-Th dates, was also added. As the reviewer requested, lines showing sampling traverses have been added to the figure. Isotopic analyses along secondary traverses were performed on two stalagmites. As is evident from the figure added to the Supplemental Material (Fig. S7), the isotopic values are consistent (within analytical errors) between the two transects and the trends are also similar, demonstrating that recrystallization did not materially diminish the paleoclimate information recorded in these sections.

2) The structure of the manuscript needs to be improved, I cannot identify a clear red line that is followed through in sections 4 and 5. I believe the paper will be easier to read if the interpretation of the proxies ($\delta^{13}C$, $\delta^{18}O$, $\delta^{234}U$, and growth intervals / hiatus) are set in a "Results + interpretation" section (what is now section 4). In section 5 environmental links can be discussed with other paleoclimate records, and I would suggest to divide this in first order based on timescales and 2nd order the proxies followed by a short intermediate conclusion:

a. Environmental links on orbital timescales; i. Growth intervals / hiatuses ii. $\delta^{13}C$ + $\delta^{234}U$ iii. $\delta^{18}O$ iv. Conclusion
b. Environmental links during Greenland stadials / Heinrich events; i. Growth intervals / hiatuses ii. $\delta^{13}C$ + $\delta^{234}U$ iii. $\delta^{18}O$ iv. Conclusion
c. Environmental links during DO events; i. Growth intervals / hiatuses ii. $\delta^{13}C$ +

$\delta^{234}U$ iii. $\delta^{18}O$ iv. Conclusion

Thanks largely to the detailed and thoughtful comments by the four reviewers who evaluated the first version of this manuscript, the structure of this paper evolved substantially into the revision. We appreciate the suggestion by reviewer 2 of the revision (reviewer 5 overall) regarding the format of our presentation, but feel that it is sufficiently clear and concise in its current form.

3) GNIP data from Porto is used to show relationships between the $\delta^{18}O$ composition of meteoric rainfall and rainfall amount and air temperature. Porto is not indicated on the map in Fig. 1. Importantly it is located 200 km north of the cave sites and experiences a different type of climate with over 1250 mm of annual precipitation, i.e. 750 mm more than at the cave sites. If there are GNIP stations south of the cave sites these are more likely to provide useful information for the interpretation of the $\delta^{18}O$ as the data from Porto (perhaps Lissbon??). The relation between $\delta^{18}O$ and air temperature (i.e. a slope of 0.2‰/°C) cannot be simply extrapolated to the cave site, as these relationships are often site-specific.
This is a fair point. Unfortunately, the GNIP data from Lisbon are insufficiently detailed or long to be of much use in this case, but we have included (and discussed) GNIP data from Vila Real and Portalegre, and added both of these locations to the map. Interestingly, the slope of the air temperature/precipitation $d^{18}O$ relationship is similar at all three sites. Thus our argument regarding the limited impact of temperature on stalagmite d18O compositions appears to hold.

Other important comments: Line 107: The altitudes of the caves are not indicated. Line 225: I strongly suggest to restructure this to "Results + interpretation"
The altitudes of the caves are not located in the Cave Settings section but are instead in the Environmental Setting section.

Line 269 "The second portion of the Hendy Test": This should be discussed in this paragraph but it is not. Instead the authors continue to describe factors that affect the $\delta^{18}O$ composition of meteoric precipitation, and only come back to this point in lines 309-319. Please restructure and use sub-headers in this section like:

4.3. Assessing isotope equilibrium 4.3.1 Hendy tests
4.3.2. Modeled isotope values
4.3.3. Replication 4.4. Interpretation $\delta^{13}C$ 4.5. Interpretation $\delta^{18}O$
4.6. Interpretation $\delta^{234}U$

Once again we appreciate the reviewer's suggestion for restructuring this section of the manuscript but here we also feel that the current structure provides the most straightforward means of presenting these arguments.

Line 369-373: Is this not in contrast with what is written in section 5 that there are large shifts from arboreal to semi-desert vegetation types? Or does the semi-desert vegetation consist of shrubs and little grasses?
There is no conflict. C3 vegetation dominated at all times but large <u>proportional</u> changes

were observed in semi-desert vegetation (overall abundances remained low).

Line 468: I'm not sure what the authors mean by increasing the age model by 4 and 1.3 kyr? Simply shifting the age depth model by 4 and 1.3 kyr? If the latter, this raises the question whether this is allowed by the age-depth model, because especially 4kyr is really a lot, and based on the uncertainties of the Th/U ages this cannot be done. The Th/U age uncertainties are much smaller for this stalagmite. Also the age-depth model is already an interpretation based on the COPRA algorithm, so some stratigraphic depths associated with a Th/U age may already be interpreted as older or younger as given in Table 1. If the authors seek an objective method to tune the two timeseries I would suggest to use ISCAM (Fohlmeister et al. 2012).
The age models were shifted consistently by these amounts as a method of tuning the overlapping time series to the SST record. The magnitude of both offsets (4 and 1.3 kyr) is smaller than the age uncertainties in the combined stalagmite and SST age models.

Line 513-514 "while hiatuses....<13.7°C).": This is not supported by the BG record. There are many low insolation phases with speleothem growth, and high insolation phases that coincide with an hiatus. I find the relation between the occurrence of hiatuses and NH summer insolation for the BG record weak.
We agree; within the larger context of the data, SST plays a more important role than insolation. The text has been changed to reflect this idea.

Line 521: The $\delta^{13}C$ record is not similar to the NH summer insolation apart from the last 50 kyr, and maybe two more lows around 220 and 150 kyr. I think this can be deleted.
We agree and have made this change to the manuscript.

Line 529-532: "although it.......be involved." Can be deleted, it is speculative and it doesn't lead to any conclusion. It is sufficient to write "The origin of this high variability is unclear. Replication of the Holocene portion of this record currently underway will help address this question (Thatcher et al., 2018).
We agree and have made this change to the manuscript.

Line 565-566: Antaractic δD and CH4 records are not mentioned anywhere else in the text, which is focused on the climate of the Western Iberian Peninsula, so this is not important for this study and can be deleted.
We were conflicted about including the Antarctic methane and deuterium data, as well, but decided to leave them in the manuscript to illustrate that the early interglacial peak is not an artifact of a small number of European data sets but is, instead, recognized as a global phenomenon. As a result, we have left this section unchanged.

Line 588: This is incorrect. There is a NAO reconstruction available from West Greenland that covers the last 5200 years (Olsen et al., 2012), and a Holocene speleothem record form Morocco that covers the time period from 11.5 to 2.6 kyr is interpreted in terms of NAO as well (Wassenburg et al., 2016). These two references should be mentioned here as well.
These two studies are now cited in the manuscript.

Figure 1: Porto is not indicated on the map.

Porto is present on our version of the map. Perhaps there was an issue with figure translation within the CotP system? As discussed above, we have also added the locations of two other GNIP sites.

Figure 6: Scale bars are missing.
We have added scale bars to illustrate the differential sizing of each stalagmite.

Figure 9: Why not plotting the records with the proxy uncertainty translated in time? This would be very useful in order to assess whether the records replicate or not.
We see the reviewer's point but hesitate to tune the records to each other given the number of stalagmites and the errors on the ages. We feel that the reader can adequately assess the degree of overlap/covariance within the constraints of the approach used for the figures.

Figure 10: I strongly suggest to plot the proxy uncertainties here as well to facilitate comparison with other paleoclimate records. In addition, I would suggest to include a graph like in the former Figure 6 that indicates the hiatuses in N Spain and S France with color coding for the specific sites, and please add the hiatuses from BG and GCL records. Right now it is sometimes hard to identify the hiatuses solely based on interruptions of the black line in BG and GCL records.
We agree that color-coded bars to illustrate site-specific hiatuses would be useful, and we have played around with this idea quite a bit. The trade-off in doing so, of course, is that the already busy figure gets increasingly complicated and difficult to make sense of. At the risk of seemingly overly resistant to the helpful suggestions by this reviewer, we again have left this portion of the manuscript unchanged.

Please indicate the timing of YD, HS, and GS in this figure as blue shaded bars like in Fig. 12.
Ditto our previous response. The issue is one of balancing clarity against the amount of information provided. Relative to the first version of this manuscript, we have markedly increased the number of figures in order to allow the presentation of this sort of detail.

Figure 11: Please indicate the timing of the GI with shaded bars according to NGRIP. Right now it is rather unclear which peak in the curve is indicated by which number.
We appreciate the reviewer's concern but feel that this figure is already complex and adding blue bars will only make it less readable.

Figure 12 Labelling of YD, HS, and GS are missing.
Labels have been added to the figure for YD and HS. Labels for GS are not added as they are not the focus of this figure.

Line 426-429: We also added a short discussion of the anomalously low $\delta^{18}O$ values associated with the GI-1 that was absent from our earlier drafts.