Review of cp-2017-146 “A Stalagmite Test of North Atlantic SST and Iberian Hydroclimate Linkages over the Last Two Glacial Cycles” by Denniston et al..

The authors present a very interesting speleothem record from the west Iberian Peninsula that covers most of the last 250 kyr. The climate in this region is affected by the course and strength of the westerlies during the winter season and is therefore well chosen to conduct this study. The record is unique as it is the first terrestrial record from the west Iberian Peninsula with an independent chronology that covers the last 7 marine isotope stages. This record definitely needs to be published. However, I have a few concerns regarding the presentation of the sample positions for the isotopes and the dating on the stalagmites that prohibit me from assessing the quality of the records. Furthermore, the structure of the manuscript and figures need to be improved, and I have several additional comments that need to be addressed.

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.
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

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.

Other important comments:

Line 107: The altitudes of the caves are not indicated.

Line 225: I strongly suggest to restructure this to "Results + interpretation"

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 $\delta^{234}U$

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?

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).

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.

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.

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).

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.

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.

Figure 1:
Porto is not indicated on the map.

Figure 6:
Scale bars are missing.

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.

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.

Please indicate the timing of YD, HS, and GS in this figure as blue shaded bars like in Fig. 12.

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.

Figure 12
Labelling of YD, HS, and GS are missing.