Review "The 8.2 ka event in northern Spain: timing, structure and climatic impact from a multi-proxy speleothem record" by Kilhavn et al.

This manuscript presents a speleothem record from northern Spain (El Soplao cave) that covers the 8.2 ka event with a well-stablished chronology. The record was presented in a previous paper focused on other time period (Rossi et al., 2018) and the chronology has now been improved. The main highlight is the combination of proxies to really infer the climate signal in this region as response to the 8.2 ka, combining growth rate, stable isotopes and trace elements with adequate resolution. There is a nice discussion to interpret the proxies and an excellent comparison with other speleothem records from W Mediterranean. The authors conclude that this event was synchronous in Greenland and S European records, with similar structure. I just have few comments that can be easily solved in a new version of the manuscript, previously to its acceptance.

We thank reviewer 2 for very positive and constructive comments and suggestions. We just wish to clarify that this record (SIR-14) has not been presented in the previous paper of Rossi et al. (2018), which focused on another stalagmite (SIR-1) located in the same cave chamber. However, several new U-Th dates were measured on SIR-1 to improve its chronology for a better comparison of its Holocene growth phase with the SIR-14 record.

1. The influence of temperature and amount of precipitation in the rainfall isotopic composition (i.e. $d^{18}O$) is not easy to determine in this region. I like the approach of separating both influences as it is presented in Fig. S2 (lines 112-115 in the main text) but I think that, from the graphs, an acceptable correlation with temperature can be inferred, excepting for samples with very high precipitation. I think those samples may correspond to heavy summer storms that can provide very negative values although temperature is high. The authors may want to check it. Therefore, this figure and the obtained correlations need a more detailed consideration and probably giving a more important role to temperature variation.

The reviewer makes a good point. There are stronger correlations (statistically significant) for temperature vs d18O for the lower bands of rainfall (less than 100 mm amount of monthly rainfall). From the GNIP data the maximum effect of temperature on d18O variation is 28 % (r = 0.53). Thus, it appears that the temperature effect is potentially important when there is less precipitation, however, it cannot alone explain the variations in d18O. We have added this to our discussion about driving factors for the d18O (see section 5.1.4).

2. Besides, in line 465, it is considered a $d^{18}O$ – temperature gradient between 0.24 – 0.34 ‰/°C, following GNIP results presented in (Domínguez-Villar et al., 2008), values that can be higher in other areas in northern Spain (please, check Moreno et al., 2021 for information at event-scale). If those values are higher, they won't be counterbalanced by the temperature dependence of water-calcite isotope fractionation in the cave. Thus, I would not exclude so rapidly temperature as an important influence on $d^{18}O$ record. I think that temperature influence can be higher than 0.11 ‰/°C as pointed the authors in line 468. Still, I agree with the authors that very likely, the effective recharge was a more important factor on $d^{18}O$ values.

We thank reviewer 2 for pointing us to this paper: Moreno et al. (2021). However, Moreno et al. suggest a temperature effect of maximum +0.38 ‰ $^{o}C^{-1}$, which is not significantly higher than the estimate from Domínguez-Villar et al. (2008), and this would still be partially counterbalanced by the temperature dependence of water-calcite isotope fractionation in the cave (leaving a residual of +0.14 ‰ $^oC^{-1}$). The magnitude of variability in SIR-14 d18O (from -5.9 to -4.3 ‰), and the known low temperature variability of the Holocene, would relegate temperature to being of minor influence. Nevertheless, we have added this paper and the additional information about temperature gradients from other sites in Spain to the manuscript (section 5.1.4).

3. Although I agree that other records such as lake or marine sediments lack the adequate resolution (in the sampling and in the chronology) to provide information about the timing of the 8.2 ka, I don't agree about neglecting the information they can offer on the impact of that event. I think that information can be of importance to get the regional picture and try to stablish the forcing mechanisms. It is important to include some lacustrine records and archaeological sites in the discussion section 5.2.3 since they are indicating, in general, a dry period during the 8.2 ka event, contrarily to what is observed in the speleothem records. I would recommend checking the Basa de la Mora record (a well-dated Holocene record from a lake in the Central Pyrenees) (Pérez-Sanz et al., 2013); the pollen record from marine core MD952043 and references therein (Fletcher et al., 2013) and a compilation of archaeological sites from the Ebro valley that were abandoned during the 8.2 ka due to dry conditions (González-Sampériz et al., 2009). There is also a recent paper on this topic (García-Escárzaya et al., 2022). I think all these records will enrich the discussion and may allow to define different regions in Iberia with distinct responses to the 8.2 ka event.

We thank reviewer 2 for this suggestion. However, as the reviewer points out, most of these other records lack the adequate resolution to provide information about the timing of the event. Additionally, most of these other records show a much longer-lasting climate anomaly, typically spanning ~300-400 years. Thus, it is likely that (at least) some of these records are not showing a response to the short-lived 8.2 ka climate event but are rather linked to summer insolation (as pointed out by Morellón et al., 2018). There seems to be an overall consensus towards drier conditions in regions associated with the Mediterranean and more humid conditions in regions associated with the North Atlantic in the early Holocene (although there are exceptions), suggesting that there is a different response in different regions. However, without more precisely dated records that capture this difference in climatic response lasting for ~150 years (the average duration of the 8.2 ka event), it would make things ambiguous to include these records. Nevertheless, we have added some additional discussion regarding other records in the region, as we agree with the reviewer, they offer important information about the climate in the early Holocene.

Minor comments:

- I miss the age model figure for SIR-1

The age-model for SIR-1 is shown in the supplementary material, figure S8D.

- I am surprised that generating a new chronology for the presented records provides such differences in timing comparing with the previous ones (more than 200 years of temporal shift in some cases). This is important to me since considering one or the other way of generating the age model makes the 8.2 ka event to be synchronous or not. I wonder if the authors considered to improve the chronologies with more dates, not only with a different modelling approach to get a more robust approach here.

The reviewer is right in pointing this out: the new age models can make the 8.2 ka event to be synchronous or not. However, we created the new chronologies independently of their associated proxy records to avoid potential biases in tuning the records to one another. The new chronologies were created by using the same approach described by Corrick et al. (2020). First, the U-Th ages were recalculated using the most recent estimates of the decay constants (Cheng et al., 2013) and by modelling the initial 230Th/232Th activity. These calculations were conducted by co-author John Hellstrom, who was not privy to the proxy data. Lastly, these recalculated ages were used to create the new chronology by the Finite Positive Growth Rate Model. To improve the chronologies with more dates would be the ideal way to go to test the synchrony of the event. However, it is beyond the scope of this paper to refine the dating of the selected published records. These were already selected based on several criteria and considered of high-quality (i.e., sufficient resolution and well-constrained chronologies).

- Line 691: the reference Zielhofer et al., 2019 does not correspond to SW Europe (it may be better to talk about W Mediterranean).

We have modified the text here.

References

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