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Climate of the Past
Discussions

Interactive comment on "Western Mediterranean hydro-climatic consequences of Holocene iceberg advances (Bond events)" *by* Christoph Zielhofer et al.

Anonymous Referee #1

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Review of "Western Mediterranean hydro-climatic consequences of Holocene iceberg advances (Bond events)" by Zielhofer et al.

The study presents an improved version of a published Holocene ostracod δ18O dataset from Lake Sidi Ali from the Middle Atlas in Morocco. The authors relate pronounced shifts in their record to the well-known Bond-events. The authors suggest that the study site's hydroclimate response to Atlantic cooling during Bond events changes from drier winters during the Early Holocene to wet winters during the Late Holocene. The paper is very well written, and I like the discussion on the Mid-Holocene climate shift, but I'm not convinced yet about the interpretation of the ostracod δ18O, and not

all correlations with the Bond events.

Concerns: 1) The authors have published the interpretation of the δ18O record in Zielhofer et al. (2017). They interpret the δ18O record as a proxy for winter precipitation, which is based on a multi-proxy approach with a charcoal, and cedar pollen abundance records. Although, I can follow their line of arguments that cedar trees need enough moisture and the charcoal record may represent fire activity, I don't see a clear correlation between the charcoal record and the cedar pollen abundance. It is the coherence between these proxies that led the authors to the interpretation that the δ18O represents winter precipitation. If this coherence is really there, then I would like to see a correlation matrix with significance levels between the cedrus pollen, charcoal, and ostracod δ18O. This should be done for different timeslices, or perhaps with running correlations as done for the comparison with the Bond record. But in any case significance levels should be indicated.

2) The authors clearly state that lake Sidi Ali is a closed basin lake where the Precipitation - Evaporation balance (P-E) plays an essential role in controlling the oxygen isotope composition. This is evident from the highly elevated present day δ18O values of the water that range from 0 to +4 ‰ whereas the surrounding karst springs and streams range from -6 to -9 ‰ (Zielhofer et al., 2017). The lake shows a huge range in surface area varying from 2 to 2.8 km2 (Zielhofer et al., 2017) due to varying P-E balance on interannual / decadal timescales. This is extremely likely visible in the δ18O of the water. This can be controlled by both evaporation during the dry season, and by replenishment during the winter season, but not only through winter precipitation.

3) In order to show that the ostracod δ18O variability represents the δ18O of the water the authors calculate the theoretical calcite δ18O values based on the present-day water δ18O and the isotope fractionation factor from Friedman and O'Neill (1977). Why not using the much more recent isotope fractionation factor from Kim and O'Neill (1997)? Please show a range of possible temperatures that can be calculated taking into account different isotope fractionation factors.

4) One aspect that has not been discussed is the role of changing water temperatures on ostracod δ18O. Particularly during the Early Holocene where the authors argue that there are less cedar trees due to heat stress. There are four phases where there is a clear correlation with the δ18O at 10.2, 8.2, 6.0, and 5.2 (I do not see a coherence at 7.3), why are these phases not interpreted as cooler summers? Cooler water temperatures may also result in heavier calcite δ18O, and could provide a different interpretation that is consistent with the cedar pollen abundance record. This may also be in line with "Atlantic cooling".

5) I do see a possible correlation with the HSG record from Bond et al. for the Early Holocene for the positive δ18O peaks around 11.4, 10.2, 8.2. However, for the peaks at 9.3, 7.3, 6.7, 6.0, and 5.2 the variation in the δ18O is either very small or the timing is not comparable to the Bond-events. The timing might be due to age-model uncertainties. But in its present form, I'm unable to assess whether the Bond events and the positive peaks in δ18O are within error of the age model or not, because the age uncertainties are not indicated in Fig. 2. This is definitely a must.

6) The 25-point running correlation calculated between the δ18O and the Bond record shows correlation that barely reach 0.3, is this significant? Can you draw a line that indicates the 95% confidence level? I'm aware that age model uncertainties should also be taken into account, so this can be discussed.

7) During the Late Holocene the authors try to link peaks at 4.6, 4.2, 3.2, 2.7 to peaks in HSG. I truly think that this is very hard to see, because the variation in δ18O is very small. During the late Holocene these timings are also linked to cedrus pollen abundance peaks?? or troughs?? Without reading the text and by simply looking at the figure with the blue bars it is not possible for me to determine whether the authors think there is an increase or a decrease in the cedrus pollen abundance. Therefore, I find this unconvincing.

8) The paper shows no figure with a comparison with regional records to test their

interpretation of the ostracod δ18O record, for example the pollen record from MD95-2043 (Fletcher et al., 2013) should be included. Furthermore, if the authors are correct and their δ18O record represents winter precipitation, then a figure with a comparison with NAO records is necessary.

The discussion of the climate mechanisms in the paper is based on the interpretation of the ostracod δ18O record representing winter precipitation variability. However, in order to support the discussion, the authors need to show that the interpretation of the ostracod δ18O record is robust.

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