

## ***Interactive comment on “Multi-decadal climate variability in southern Iberia during the mid- to late-Holocene” by Julien Schirrmacher et al.***

### **Anonymous Referee #1**

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I find the manuscript from Schirrmacher of particular interest, especially for its combination of terrestrial and marine proxies in two close regions, which at the end show significant differences in the marine response to climatic changes. Of particular relevance to re-dating two records and giving a new age models, which produce high-resolution data. I have two general observations and several along the text. General observations 1) I think for a special issue dedicated to 4.2 ka event, it would be useful to expand the discussion on this point. The chronology suggested for this event is not really coincident with the interval where it is often found (see for instance for the Mediterranean Bini et al. this issue Climate of the past discussion). The range of ages found for this interval overlaps two dry event you find (i.e. 4.4 and 3.8 ka cal BP), but separated by an interval of wetter conditions. So the impression that this interval is much shorter

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and/or older than in other records (but see also Isola et al. this issue or Kaniesky et al. this issue). For such reason your record is of particular interest because, it seems well dated and can give new perspectives on the topic. I think adding some sentences on these points would be very useful. 2) In many points the authors state that there is a good agreement between different proxies and different archives, when it is not always the case. In some instances the authors recognize these discrepancies but in other not. I suggest some changes along the text. 3) Probably it would be useful to plot in figure 2 also the older ages model. In a way that older records will not be anymore selected if not recalculated on the new age models. Specific points

Pag. 2 line 4, just to note that the Neoglacial onset in the Apennine (central Italy) started at 4.2 too (Zanchetta et al., 2012 QR) Pag. 2 line 6: mean primary productivity (MPP), it is the first time quoted in the text. Pag. 3 line 2, delete regime after Mediterranean Pag. 3 line 7 mm instead of ml Pag. 3 line 14 delete during winter (written twice) Pag. 4 line 14 why you don't refer to Reimer et al. 2015? Pag. 4 lines 18-19 linearly interpolated. ...be more precise which ages are interpolated. Pag. 6 line 2, 10 ccm? Do you mean 10 cm? or? Pag. 6 line 13 Proxy restrictions? What do you mean precisely? Pag. 6 line 17 Jalali et al. 2016, 2017 Pag. 6 line 23 Vogts et al., 2009, 2012 Pag. 7 line 18 how changes of 1 °C are significant considering the accuracy of the methods? Pag. 7 line 20 "Annual mean SST in GeoB5901-2 vary stable? Is very stable around ca. 20.0°C? It would be useful to give numbers as mean ± sd. This can give also an idea of significant deviation from the mean. Pag. 8 line 11 ...well matches. ...later you wrote it is not always the case. It is better to write something like "there is a general agreement. ... Pag. 9 I think here to expand the discussion on 4.2 event is of particular interest. Pag. 9 line 15 "drought episodes are paralleled. ..." I think once again caution is necessary and description of mismatching is necessary. Pag. 9 lines 26-26. How is it possible that insolation decrease and SST increase? Is there anything wrong in this sentence? Or a further explanation is necessary? Which temperature are really recording your proxy? It probably needs some explanation. End pag. 9 beginning pag. 9. Please check carefully. It is little confusing and it is not always evident

to understand which SST you are referring to (mean, seasonal). Pag. 10 lines around 15, some further, more explicit comment on what temperature are measuring with your proxies is necessary. Pag. 10 lines 19-21. I don't think second decimal can be considered significant considering the age model. Pag. 10 lines 21-21. "These events, notably, differ from. . ." Surely this part needs to be expanded a little more. . . . Pag. 10 I have no particular problem about the selection of Goslin et al. 2018 record, but there are also others. Is there any special reason? Is this record better dated? More robust? Pag. 10 line 26 ". . . .compare well. . . ." See previous observations.

Overall, the manuscript is well written and quite clear. Considering my general observations I think it needs moderate revision before acceptance. I recommend expanding the part related to 4.2 cal BP events.

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