

Interactive comment on “Late Holocene climate

variability in the southwestern Mediterranean region: an integrated marine and terrestrial geochemical approach” by C. Martín-Puertas et al.

Anonymous Referee #1

Received and published: 20 October 2010

This is a very nice paper that integrates results from marine records of the Alboran Sea and a lake record of the South Iberian Peninsula to study humid/arid conditions in the southwestern Mediterranean region during the Late Holocene at inter-centennial resolution. For that purpose, the author uses different geochemical proxies from which it has been possible to establish four main stages of moisture variability during the Late Holocene in this region. In addition, these records have also been compared with records from Central Europe, North-central Africa and Eastern Mediterranean regions

C908

concluding that the southwestern Mediterranean region climate evolution oscillated in synchrony with western-central Europe and west tropical Africa and oppositely to eastern Mediterranean, supporting a seesaw pattern of climate variability for the Mediterranean during the Holocene.

Therefore, I recommend publication after author address my comments.

I really like the very good correlation that marine Mg/Al ratio and lake Rb/Al ratio show during this period denoting the consistency of both ratios as precipitation and/or humid conditions proxies. I also find very well exposed and convincing the way the author presents Zr/Al and Mg/Al ratios, in the Alboran Sea, and Rb/Al and Sr/Al ratios, in the Zoñar Lake. However, I do not have the same feeling with the explanation for U/Th ratio. The use of the U/Th ratio in this manuscript has some assumptions that should be reviewed, checked and/or corrected, and furthermore, better developed in the manuscript. First of all (page 1661, lines 16-29), Jiménez-Espejo et al. (2007) did not use the U/Th ratio as a “good indicator of changes in the oxic bottom conditions”, but the U/Al ratio, together with Mn/Al and Fe/Al ratios. By reading this paragraph I have no clear idea why the authors use Th to normalize the U instead of Al, is there any justification? In this case, this should be included in the text, if not, why don't plotting U/Al ratio, since normalization with Al has been applied to all of the elements? In addition, I also suggest to justify the use of Al normalization in the text. And second, the relationship between deepwater ventilation conditions and U/Th ratio is strictly assumed to be linear in here and nothing is said about focussing of sediment or changes in primary productivity (Magnini et al., 2001). I suggest to discuss U/Al in parallel with Mn/Al in order to justify the origin of the U/Al peaks, since redox processes in the burn-down layer can have more complex interpretation (Magnini et al., 2001).

In addition, in this same paragraph (lines 23-24) it is said that deepwater ventilation is induced by increased aridity and/or cooler temperatures during winter in the Gulf of Lion, and then the author suggest that U/Th ratio can be used as a proxy for both, precipitation and temperature. The main trigger mechanism for deepwater formation,

C909

and thus deepwater ventilation, in the western Mediterranean Sea is the winter cooling of surface waters by north and northwesterly winds blowing in the Gulf of Lion until those waters get denser and sink to the deep basin (Millot, 1999; Lacombe et al., 1985; MEDOC, 1970; and others). In addition, Pinardi and Masetti (2000) suggested that sinking of surface waters should be also affected by the amount and depth of the Levantine Intermediate Water (LIW) before WMDW formation events. Hence, increased aridity and/or cooler temperatures are “conditions associated with improved deep-water ventilation” (literally from Myers and Rohling, 2000), but not the cause itself of increased deepwater ventilation. So, I suggest rewriting this sentence and look for more appropriate references.

Thus, following the above comments, the straight forward assumption that U/Th ratio can be used as deepwater ventilation proxy and then, precipitation and temperature proxy, is not proved. Then, if the author thinks that there is some authigenic U accumulation on the sediments that could be related to changes in deepwater ventilation I suggest to discuss better the meaning of U/Th (or U/Al) ratio in the context of redox conditions, likely together with other elements like Mn. Finally, if the author considers that there is enough good information that U/Th (or U/Al) ratio can introduce for the discussion of the paper, thus it should be discussed in the text. However, I'm not sure how much U/Th discussions contribute to the manuscript and, in the light of the other proxies, it could be removed.

Finally, I liked the summary done in the conclusions section about humidity changes in the southwestern Mediterranean region in four main stages. I suggest the author to present these stages before in the discussion of the proxies, likely section 6 would be enriched with that, since this is the real result of integration both marine and lake records and would make more easy to follow all the discussion section in four different time periods.

Other more specific comments:

C910

1. Page 1657, Line 5: Magnini et al. (2001) do not use the U/Th ratio but the U-content.
2. Page 1657, Line 19: Remove reference Frigola et al., 2007 since this work is not from the southwestern but northwestern Mediterranean region.
3. Page 1658, Line 1: “...hydrological fluctuations in the continent and possible traces of human impact.”
4. Page 1658, Line 11: although the two ideas expressed between lines 9-13 are referred to the Alboran Sea it will be better to split this in two different sentences: i.e. “...dust and coastal/riverine inputs (e.g. Martínez-Ruiz et al., 2203). The thermohaline circulation of the Alboran Basin is controlled...”. In addition, the sentence about thermohaline circulation and the information of the Zoñar Lake result very disconnected from the rest of the text in Regional Setting. Also, use Strait of Gibraltar instead of Gibraltar Strait.
5. Page 1659, Line 23-26: the age model is for the cores not for the entire Alboran Basin. In addition, it would be better to expand the sentence of the centimetres covered by each sediment core during the last 4 ka and remove the brackets. In line 27 use “...based on...” instead of “...based in...”. In line 28: “For Zoñar Lake core, the age-depth...”.
6. Page 1661, paragraph from lines 16-29: remove or rewrite. See comments above.
7. Page 1663, line 1: “...associated with...” instead of “...associated to...”.
8. Page 1663, Lines 15-26: when talk about the Pb-enrichment in the studied cores I suggest to denote in the text that the observed changes corresponds to the studied cores or records, since it seems, from the text, that what the author is saying is valid for all the Alboran Sea and Zoñar Lake, what is not proved (e.g. Line 19: “...Figure 3 shows Pb-enrichment in sediments (Pb/Al ratio) of Alboran Sea and Zoñar Lake records: roman lead pollution is recorded in both, but medieval signal only occurs in the Alboran Sea records.”

C911

9. Page 1663, Lines 23: would it be possible to develop a little in deep why the Pb-peak is not observed in the Zoñar Lake because of the type of sediments?
10. Page 1664-1665: I would delete all the text in reference to U/Th ratio. Discussion of Mg/Al, Rb/Al and Zr/Al proxies is already very consistent and does not need the support of U/Th proxy that, in my opinion, it just makes things more complex and unclear.
11. Page 1666, Line 7: more than synchronous these cold SST Alboran events “fit quite well” with both, cool pulses in the west. Med and global polar cooling, as denoted later (some disagreements...).
12. Page 1667, Line 28: “...and eastern regions in the Mediterranean.”
13. Figures: I suggest to increase the font-size from the different plots of the figures for easier reading. In Figure 3 it would be helpful to label both plots with letters (a and b). In addition, in Figure 4 and 5 it would be also helpful to denote the MCA and the LIA periods in some way. Finally, in the text, plots from Figure 5 are also named with different letters so it would be necessary to label the graphs with those letters.

Interactive comment on Clim. Past Discuss., 6, 1655, 2010.

C912