

Interactive comment on “Paleohydrological changes over the last 50 ky in the central Gulf of Cadiz: Complex forcing mechanisms mixing multi-scale processes” by A. Penaud et al.

Anonymous Referee #1

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General comments Authors describe new dinocyst evidence obtained in the Gulf of Cadiz, which interpretation is based on a new chronology (tuned with the latest NGRIP time-scale) for the deepest part of the section studied. Then they contrast their results with other previous data in the region. Comparison between both sides of the Gibraltar Strait reveals interesting features that might be connected with the climatic changes and the reorganization of the marine currents. I found that the regional review, methods and argumentation are usually good. In general terms I am satisfied with the content of this manuscript, which in my opinion may be considered a notable piece of work. Nevertheless, in order to improve some parts, I suggest punctual rephrasing and perhaps putting more emphasis in some points that I consider not entirely clear: a) it

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should be noted somewhere in the text that in transferring the GICC05 chronology to SST event-based records in the North Atlantic is necessary to assume that the abrupt D/O warming events in both records are synchronous. Therefore, being rigorous it is not possible to evaluate properly any leads or lags in the climate system (Austin & Hibbert, 2012); b) I wonder if could be possible to develop a more ambitious age-depth model (e.g. by combining radiocarbon dates and age-points inferred by tuning) in order to improve the calculation of flux rates; d) I would appreciate a well-defined position about the possible influence of the deglaciation of North America in the regional context, especially during the MIS2 and Early Holocene; e) the existence of methane in the seafloor in the area, and their possible influence (e.g. C sequestration, productivity, evidence of sea-level changes, etc) could be also considered along the text.

Other minor specific comments are listed below.

Specific Comments

Page 2, Line 32: After reading the MS, it is not completely clear for me if authors believe that those changes observed in their record during the Early Holocene might be (almost partially) associated to variations in the Eastern North Atlantic circulation cells, perhaps promoted by the deglaciation in North America (i.e. 8.2 ka event) or other cold relapses described in the NGRIP record during the Early Holocene. I would appreciate a more clear position about this point somewhere along your text. Page 3, Line 54: Suggestion: sequestration of C in form of methane (CH₄) is another important component of the total C sequestration/emissions in marine environments, which also can affect the local productivity and biodiversity around the seepages (e.g. Judd & Hovland, 2007). Multiplicities of evidences exist in this area revealing the occurrence of CH₄ in the seafloor (e.g. León & Somoza, 2011). Consider to include some references to this point in your text, please. Page 4, Line 73: Also in form of CH₄. Page 4, Line 89: This sentence is a bit unclear for my taste. Some specific references could be useful here to illustrate your idea. Page 4, Line 92: insert a spacebar between HYDROGRAPHY and OF, please Page 7, Line 151: Figure 2 should be cited before Figure 3. May be you re-

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fer Figure 2 but not Figure 2? Reordered the references or rename the Figures, please. Page 8, Line 181: If some non- Quaternary specimens in your record come from re-working, anyone might wonder if any other Quaternary specimens have been reworked too. Clarify, please. Page 9, Line 195: I have some doubts about this. Apparently, with a limited set of age-points (radiocarbon or tuning), it seems possible that you could build a robust Age-Depth model able to attribute an age for each sample studied; and thus, to calculate the flux rates between each two samples. Why not? Page 9, Line 197: Do you mean you don't have enough subsampling resolution for individualize every single short event? Clarify, please. Page 9, Line 212: Nevertheless you have assumed above that chronologies of the main climatic shifts have been synchronic in Greenland and your site. Of course, such assumption prevents to establish any definitive conclusion about leads and lags. I can see a possible trouble here. You should be very clear about the possibilities/limitations of your new chronological approach along the text. Page 12, Line 268: insert a comma, please: just after HS5, during GI 12. Page 13, Line 273: Perhaps be useful to indicate the MIS intervals also in Figure 4. Page 15, Line 327: This differentiation between MIS-2 and deglaciation may be confused. Please, indicate what exactly mean everyone in this context. Perhaps do you mean Late Glacial instead of deglaciation? Page 15, Line 329: Indicate its position in Fig. 1, please. Page 16, Line 337: use lower case (gC/m²) Page 17, Line 371: Climatic changes affecting the regional freshwater inputs also may contribute to explain those similarities between last Glacial in the Gulf of Cadiz and mid-Holocene in the Bay of Biscay (e.g. Mikolajewicz, 2011). Might be this is another way for supporting a similar argument? Clarify, please. Page 20, Line 451: synchronicity (which may be observed in Figure 8c but seems a bit imperfect between ca. 43-35 ka) may be (at least in part) a result or the previous tuning. Synchronization between both areas is reliable, but not well-demonstrated until two independent sets of chronological data were compared. Such point may be clear along the text. Page 20. Line 456: In relation to pollen representation and the associated interpretations, you might have in mind that the difference between Interglacials and Interstadials has been clearly established in the literature (e.g. Birks and Birks,

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1980). Relative high tree pollen percentages observed during those interstadials (GI 12 and 8) might be considered increases but uncomplete ecological successions. Alternatively, it should be only a true tree expansion (complete forest succession) at the beginning of the Holocene. I suggest rephrasing consequently. Page 54, Line 1260: May be situated here the site of Wienberg et al., 2010 discussed in the text? Figure 2. Here you are assuming that the chronologies were exactly the same in Greenland and the Subtropical North-Atlantic, which perhaps it is not completely true. A certain latitudinal variation is likely (line 204 in your text), but perhaps impossible to detect after discarding your independent radiocarbon chronology. Of course you can believe (and argue) that the alternative tuning chronology is more realistic, but in my opinion this point (the lack of chronological independence) should be clearly stated in the text, in order to a better contextualization of your interpretations. Figure 7. Might be % Grain size $<0.063\mu\text{m}$? Revise if necessary, please.

References Birks, H.J.B. & Birks, H.H. (1980). Quaternary palaeoecology. Edward Arnold. London. Judd, A.G. and Hovland, M. 2007. Seabed Fluid Flow, the Impact on Geology, Biology and the Marine Environment. Cambridge University Press, 492 pp León, R. and Somoza, L. 2011. GIS-based mapping for marine geohazards in seabed fluid leakage areas (Gulf of Cadiz, Spain). Marine Geophysical Research, 32, 207-223. Mikolajewicz, U. (2011) Modeling Mediterranean Ocean climate of the Last Glacial Maximum. Clim. Past, 7, 161–180.

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