Brest, 22th of July 2016

Dear editor and reviewer,

We wrote the response and we corrected the manuscript at the same time. Then, this letter addresses all reviewer remarks: main issues that needed clarifications as well as minor points (words or sentences corrected/re-written, typos..) that have easily been corrected in the revised manuscript.

For the moment, you will find below our detailed responses to each of the reviewer comments. Comments have been grouped when they addressed same issues. For the reader convenience, all our responses are in red in the following text.

We hope to have satisfied / clarified all remarks.

Sincerely yours,

Aurélie Penaud and co-authors

Anonymous Referee #1
Received and published: 31 May 2016

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 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)*

Also:

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 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.

Response: this is something we are definitely conscious about, see for instance the paper our team has published in 2012 (Eynaud et al., GRL 2012) where we stated:

(1) In the main text, regarding the age model: "…Taking advantages of the recent discussion cautioning "marine event-based chronostratigraphies" (Austin and Hibbert, 2012), especially regarding phasing issues, **we thus prefer to avoid any artificial tuning to the Greenland ice-cores. This approach thus generated a fully independent chronology**.

(2) In the Supplementary Figure 1 caption: "Stratigraphy of core MD95-2002, with the comparison of the initial published age model (in blue; see Ménot et al, 2006 and Eynaud et al., 2007) with the NGRIP (orange) tuned age model. Arguments to validate this tuning are not sufficiently strong to require a new age model. **Furthermore, such tuning assumes synchronicity of abrupt millennial-scale air-temperature changes over Greenland with sea-surface temperature / planktonic foraminifera population events in the temperate North Atlantic and, thus by definition is unable to detect latitudinal intra-hemispheric propagation of climatic changes**

which have a direct impact on the duration of the perturbation in the surface ocean and therefore on planktonic communities (see Eynaud et al., 2009 or Scourse et al., 2009 - Figure 5; for an illustration of this question regarding the expression of the regional impact of Heinrich Stadial on *N. pachyderma* s. records)…**"**

Here we decided to adopt such a tuning based on $\delta^{18}O$ stratigraphy thanks to the already published material on the MD99-2339 core and furthermore on proximal reference cores (e.g. MD95-2042) where this methodological choice was supported by complementary indicators (see Shackleton et al. 2000, for a review).

To avoid any ambiguity on our choice we have thus added in the text:

"It was also supported by previous works conducted on the southern Iberian margin where the stratigraphy of paleoclimatological reference sites were constructed using a similar tuning on Greenland ice records (see Shackleton et al., 2000). This event based stratigraphy (i.e., Austin and Hibbert, 2012) however prevents to establish any definitive conclusion about latitudinal leads and lags, and to evaluate intra-hemispheric propagation velocities of climatic perturbations. "

Also:

Page 54, Line 1260:

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 stayed in the text, in order to a better contextualization of your interpretations.

Response: Idem above

Furthermore, the model study by Charles et al. 1994 showed a strong linkage between the subtropical gyre and Greenland snow / ice in the form of the subtropical gyre being a significant moisture source for Greenland precipitation.

Charles, C.D., Rind, D., Jouzel, J., Koster, R.D., Fairbanks, R.G., 1994. Glacial-Interglacial Changes in Moisture Sources for Greenland - Influences on the Ice Core Record of Climate. Science 263, 508-511.

***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.*

This is already what was done with the age model revision. See line 156-155 of the initial manuscript.

Also:

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?

Response: This not really our point here as we have built an age model and thus have attributed an age for each studied sample. The referee seems precisely to not support this assumption "We **do not have enough time marker points to calculate flux rates** for every single short event separately, but at least on a multi-millennial timescale…" Our aim here was to avoid miscalculations with fluxes rather forced by the number of tie points than by natural shifts.

***C**) 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*

Response: This topic is outside of the scope of the current manuscript. The last deglaciation/ Last Glacial Interglacial Transition off western Iberia has been discussed in several previous publications. As such Skinner & Shackleton, 2003 (*Rapid transient changes in northeast Atlantic deep water ventilation age across Termination I*) discussed this in details and it was also the main topic of Bard et al., 1981; Rogerson et al., 2004; Rodrigues et al., 2010 and more recently the review by Naughton et al., in press. The studies of Turon et al., 2003 and of Penaud et al., 2010, 2011 focused on this transition with dinocyst proxies with Penaud et al. (2010 and 2011) covering the waters between southwestern Iberia and Morocco (including core MD99-2339).
Our choice was then to focus on the data from MIS3. We have added a sentence to overcome such a view for future readers**: "Different configurations of Mediterranean-Atlantic exchanges were also taken into account regarding their potential impacts on MD99-2339 dinocyst surface proxies. For this paper, we focused our discussion on the paleohydrographical response of the Gulf of Cadiz during Marine isotopic stage 3 to complement previous studies which have extensively documented the last glacial termination (e.g. Bard et al., 1981; Rogerson et al., 2004; Turon et al., 2003; Penaud et al., 2010; Naughton et al., in press)."**

Rodrigues, T., Grimalt, J.O., Abrantes, F., Naughton, F., Flores, J.-A., 2010. The last glacial-interglacial transition (LGIT) in the western mid-latitudes of the North Atlantic: Abrupt sea surface temperature change and sea level implications. Quaternary Science Reviews 29, 1853-1862.

Naughton, F., Sanchez Goñi, M.F., Rodrigues, T., Salgueiro, E., Costas, S., Desprat, S., Duprat, J., Michel, E., Rossignol, L., Zaragosi, S., Voelker, A.H.L., Abrantes, F., 2015; in press. Climate variability across the last deglaciation in NW Iberia and its margin. Quaternary International, doi:http://dx.doi.org/10.1016/j.quaint.2015.08.073.

Also:

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 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.

Response: Millennial-scale climate variations during the Holocene are not the topic of the current manuscript and cannot be resolved with the current resolution in the Holocene section of core MD99-2339. None of the western Iberian margin surface water records show a strong response to the 8.2 ka (see core compilations in Voelker and de Abreu, 2011; Salgueiro et al., 2014). The best impression of a 8.2 ka related surface water cooling is probably in core D13882 from the Tagus pro-delta (Rodrigues et al., 2009) and even here the cooling is just in the range of 1.0-1.5°C. Furthermore, in the Gulf of Cadiz, any climate change related to the 8.2 ka event is overprinted by the impact of

the African monsoon signal / Sapropel 1 climate conditions. Such processes are not easy to discriminate from each other.

Voelker, A.H.L., de Abreu, L., 2011. A Review of Abrupt Climate Change Events in the Northeastern Atlantic Ocean (Iberian Margin): Latitudinal, Longitudinal and Vertical Gradients, In: Rashid, H., Polyak, L., Mosley-Thompson, E. (Eds.), Abrupt Climate Change: Mechanisms, Patterns, and Impacts. AGU, Washington D.C., pp. 15-37.

Salgueiro, E., Naughton, F., Voelker, A.H.L., de Abreu, L., Alberto, A., Rossignol, L., Duprat, J., Magalhães, V.H., Vaqueiro, S., Turon, J.L., Abrantes, F., 2014. Past circulation along the western Iberian margin: a time slice vision from the Last Glacial to the Holocene. Quaternary Science Reviews 106, 316-329.

***D**) 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.*

Also:

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.

Response: Our discussion on methane is related to the atmospheric methane concentrations and the related climate forcing. Methane emissions from deep-sea floor source could contribute to the atmospheric methane levels, but such processes are outside of the scope of the current manuscript. Furthermore, we are discussing productivity and biodiversity in the surface ocean and not at the ocean floor. Methane sequestration / emission in the Gulf of Cadiz is related to particular features such as mud volcanoes and/or tectonic faults and core MD99-2339 was not retrieved from an area close to mud volcanoes (which would have been counterproductive for paleoceanographic/climatic studies as methane related diagenetic processes could alter proxy signals (such as of the foraminifer carbonate shells).
These CH4 seafloor emissions are actually important component of the bottom water chemistry and geology (e.g. Pinheiro et al., 2003, 2006; Maldonado & Nelson, 1999) but rather occur on the eastern Gulf of Cadiz, close to the horseshoes structure of the Gibraltar strait where a lot of mud volcanoes are actually sustained by CH4 releases. **These phenomenons however do not interact with climatic processes at the scale of our study and furthermore do not impact surface marine proxies (which are the focus of our topic).**

Pinheiro, L.M., Ivanov, M.K., Sautkin, A., Akhmanov, G., Magalhaes, V.H., Volkonskaya, A., Monteiro, J.H., Somoza, L ., Gardner, J., Hamouni, N., Cunha, M.R., 2003. Mud volcanism in the Gulf of Cadiz: results from the TTR-10 cruise. Marine Geology 195, 131-151.

Pinheiro, L.M., et al. 2006. MV Seis: Tectonic control, deep crustal structure and fluid escape pathways in the Gulf of Cadiz Mud Volcano Field. EuroMargins Interim Report, ESF Project 01-LEC-EMA24F.

Other minor specific comments are listed below. Specific Comments:

Page 4. Line 73:
Also in form of CH4.
Response: done

Page 4, Line 89:
This sentence is a bit unclear for my taste. Some specific references could be useful here to illustrate your idea.
Response: We have added a reference (Bahr et al., 2015).

Bahr, A., Kaboth, S., Jiménez-Espejo, F.J., Sierro, F.J., Voelker, A.H.L., Lourens, L., Röhl, U., Reichart, G.J., Escutia, C., Hernández-Molina, F.J., Pross, J., Friedrich, O., 2015. Persistent monsoonal forcing of Mediterranean Outflow Water dynamics during the late Pleistocene. Geology 43, 951-954.

Page 4, Line 92:
Insert a spacebar between HYDROGRAPHY and OF, please
Response: done

Page 7, Line 151:
Figure 2 should be cited before Figure 3. May be you refer Figure 2 but not Figure 2? Reordered the references or rename the Figures, please.
Response: this has been corrected. The order of citation is now correct and old Figures 2 and 3 have been inverted and are now new Figures 3 and 2, respectively.

Page 8, Line 181:
If some non- Quaternary specimens in your record come from reworking, anyone might wonder if any other Quaternary specimens have been reworked too. Clarify, please.
Response: Few reworked specimens have been identified, we then have deleted the end of the sentence.

Page 12, Line 268:
Insert a comma, please: just after HS5, during GI 12.
Response: the sentence has been modified

Page 13, Line 273:
Perhaps be useful to indicate the MIS intervals also in Figure 4.
Response: done

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?
Response: yes, corrected

Page 15, Line 329:
Indicate its position in Fig. 1, please.
Response: done

Page 16, Line 337:
Use lower case (gC/m2)

Response: done

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, 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.
Response: re-phrased

Figure 7. Might be % Grain size <0.063_m? Revise if necessary, please.
Response: corrected