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Climate of the Past Discussions

# *Interactive comment on* "Seemingly divergent sea surface temperature proxy records in the central Mediterranean during the last deglacial" *by* M.-A. Sicre et al.

**M.-A. Sicre et al.**

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GENERAL COMMENTS In this manuscript (Text, 3 Figures), authors compile their previously published work [Siani et al., 2001, Sci.; 2004, QSR; 2010, JQS; 2013, CP– this issue; Genty et al., 2006, QSR; Essallami et al., 2007, G3; Rouis-Zargouni et al., 2010, PPP; 2012, CR GEOSCI.; Bout-Roumazeilles et al., 2013, CPD–this issue; Desprat et al., 2013, CP– this issue] with some new results published here for the first time, and with records already worked and published by others [GISP2 record... Cuffey



Clow, 1997, JGR?]. Hence, authors are familiar with the study area.

The two datasets chosen (sites MD04-2797 and MD90-917) are well suited for the special issue Holocene changes in environment and climate in the central Mediterranean as reflected by lake and marine records by M. Magny, N. Combourieu-Nebout, D.-D. Rousseau, and M.-F. Loutre'. A number of aspects of the manuscript are of particular interest: - The connection between the Siculo-Tunisian Straits, the Ionian basin and the Adriatic shelf, that is, climate sensitive areas where western-eastern water exchange takes place (including eastern Mediterranean deep water formation) and the tropical (monsoonal) climatic system of Northern Africa interacts with the North Atlantic climatic system. - The dating control for the deglaciation and mid-Holocene is remarkable, consisting of 13 14C for MD04-2797 and 21 14C dates for MD90-917, together with a previous in-depth evaluation of radiocarbon reservoir age changes and ash layers. Thus, in general the age control is enough for the discussion (exception: H2; see below for constructive comments). The dating of late Holocene (ca. from 5k to present) is less well constrained, though the authors do not attempt to explain this section. - The multiproxy approach enriches the description of events: _18OG.bulloides, sea surface temperature reconstructions using alkenones, and planktonic foraminifera assemblages for comparison purposes with _18Ospeleo, _13Cspeleo, _18Oice The results presented in the paper generally support the interpretations and conclusions. They provide reference patterns for the central Mediterranean during the interval studied and this should certainly be of interest to the reader. In this regard, the paper merits publication in Climate of the Past. In order to make the manuscript more suitable for publication, my only advice is that the authors attempt to add complexity to their discussion and achieve slightly better clarity on the figures. Below are specific and technical constructive comments intended to improve the manuscript.

SPECIFIC COMMENTS References are made to the text by giving [page number, line number: "text quote"]. I group the specific comments around three main aspects: ONE Clarify FIG. 2 and FIG. 3; uncertainties; NEW TABLE 1, NEW TABLE 2 TWO

Deglaciation disimilarities: seasonality only? proxy and location? THREE Holocene Hypsithermal/ Holocene thermal maximum missing?

ONE Clarify FIG. 2 and FIG. 3; uncertainties; NEW TABLE 1, NEW TABLE 2 Please indicate clearly the original publication of every profile (i.e. the references which the data shown in the figures belong to) and the new contribution/proxies presented in this study. If not, as the manuscript is currently written, it is hard to distinguish who has done what, what the authors are actually contributing in this study and what improvements are made to previously published profiles. Below some suggestions; please confirm they are correct. MD90-917 (South Adriatic Sea, 41N 17E, -1010.0 m) - _18Ocalcite determined in G. bulloides calcite (‰ $[Sianietal., 2004, QSR; 2010, JQS] - SSTsderivedfromplanktonicforaminiferaassemblages; April - -MayandOctober - -November(AM-SSTforamandONSSTforamrespectively, \_C); [Sianietal., 2004, QSR; 2$ $alkenones(SSTalk, \_C)[thisstudy?]MD04 - 2797(Siculo - -TunisianStraits, 36N, 11E, -771m) - \_18Ocalcitedetermin edinG.bulloidescalcite( [Rouis-Zargounietal., 2010, PPP?ifso, pleaseincludethisreference] - SSTsderivedfromplanktonicforaminiferaassemblages; April - -MayandOctober - -November(AM - SSTforamandON - SSTforamrespectively, \_C); [Rouis - Zargounietal., 2010, PPP?ifso, pleaseincludethisreference] - alkenones(SSTalk, \_C)[Essallamietal., 2007, G3; thisstudy?]$

ANSWER Figure captions have been modified accordingly -

The reader always appreciates it if authors point out precisely in which part of the figures one can find whatever is being discussed (as far as possible, a figure must be self-explanatory). If figures are clearly worked, the reader will be able to recognize at first glance what is going to be discussed in the text; thus, I earnestly request that authors include in FIG. 2 and FIG. 3 the name of the core and the proxy used; this information must be clear either in the title axis and/or above every profile. This will have the added benefit of simplifying legends and the text of figure captions, without losing information (e.g. the original publication of every profile). In addition, if I'm not



mistaken, labels for the alkenone profiles in FIG. 3 are mixed up, i.e. label 'Siculo-Tunisian Strait' must read 'Adriatic Sea' and viceversa.

ANSWER This has been corrected in the revised version

[689,13-16: "The higher resolution SSTalk signal also reveals imprint of millennial-scale Heinrich event coolings, i.e. H1a (15.5kyr)... H2a off the Iberian margin (Bard et al., 2000)"] [700,6: " Shaded areas in grey indicate the Younger Dryas (YD), the Heinrich stadials H1a, H1b and H2a. "] Is it H2a or H2b? or simply H2? the label in FIG 2 does not agree with the figure caption.

ANSWER The shaded areas are H1a, H1b and H2a.

In any case, I'm afraid around the H2 interval, the dating is not precise enough to comment on it; please delete the sentences accordingly.

ANSWER We modified the sentence to be more cautious on this point - we feel that, at least in the alkenone SSTs in MD04-2797, that colder SSTS are well expressed and should thus be mentioned. This is not so clear in the MD90-917 because of continental influences and possibly also lower temporal resolution.

Apart from that, it must be emphasised that Heinrich events cannot be recognized in the SST profiles or _18Ocalcite record of different species. I'm aware that everybody does it, but technically speaking, this is not correct. We assume that the events are synchronous, but it is no more than an assumption. It is necessary to honour originally published designations wherever possible [Rousseau et al., 2006, QSR]; that is, Heinrich events" [Broecker et al, 1992. CD] were described solely as marine sediment layers containing a large concentration of ice-rafted debris and a scarcity of foraminifera or high N. pachyderma sinistral (Nps) percentages at particular northeast Atlantic mid-latitudes. Nps percentages for core MD04-2797are shown in [Rouis-Zargouni et al., 2010, PPP – Fig. 6]; are Nps percentages available for core MD90-917?; Can Nps percentages be added in FIG. 2 and FIG. 3 and name "Heinrich events" only to them?

ANSWER Nps are available for core MD90-917 (Siani, JQS 2010) but do not show a clear pattern as expected somehow from the proxy records we show here where HE are now very distinct. As mentioned above, this is possibly due to impact of from continental water runoff (Po and other rivers discharging in the Adriatic sea) - HE coolings are better seen in Modified Atlantic waters (MD04-2797) but we think that this is anyway out of the main goal of the paper and would lead to overload the figures.

- NEW TABLE 1 horizontal uncertainties; this is of particular importance, even when it is clear that extensive research has been performed with these cores. A TABLE with the dates, raw, errors, calibrated ages, together with changes concerning the newest calibrations available if applicable, etc would be very much appreciated in order to avoid digging too much in dispersed literature [Combourieu-Nebout et al., 1998, QSR; Rouis-Zargouni et al., 2010, PPP; Desprat et al., 2013, CP; Siani et al., 2013, CP]. Authors must always keep in mind the specific purpose of their manuscript: divergent sea surface temperature proxy records in the central Mediterranean during the last deglacial. In this respect, the comparison between sites MD04-2797 and MD90-917 must be transparent, taking into account chronological uncertainties. Additionally, please justify why the GISP2 chronology is used in the paper as a reference; why not the NGRIP ice core stratigraphy - GICC05 chronology? or authors use the synchronised version? [Lowe et al., 2008, QSR; Rasmussen et al., 2008, QSR; Davies et al., 2008, JQC; Austin et al., 2012, QSR]; please smooth the _18Oice, in order to make it comparable with the resolution in the marine sediments.

ANSWER Table 1 and 2 have been added to provide dating for each core in the new version of the manuscript. However, our conclusion on apparent differences between proxies is essentially drawn form proxy record relative comparison within the same core. In the discussion we mention that these differences are seen in the two cores, but this statement refer to rather large time span (from 16 to 19 kyrs) thus beyond age model uncertainties.

- NEW TABLE 2 vertical uncertainties. A question of possible interest for the reader:



comparison between the paleo-records and the instrumental data? ... not because the paleo-records cover the most recent interval but for comments on seasonality. A thoroughly review of methodological mean error of each site and proxy would be very useful; something like AM-SSTforam and ON-SSTforam: methodological error ca. $\pm$0.7-1.4_C ?? SSTalk: methodological error ca. $\pm$0.5_C ?? ... together with inclusion of the seasonality at present at core locations [Fichaut, M., M. J. Garcia, A. Giorgetti, A. Iona, A. Kuznetsov, M. Rixen, and M. Group (2003), MEDAR/MEDATLAS 2002: A Mediterranean and Black Sea database for operational oceanography, in Elsevier Oceanography Series, edited by N. C. F. K. N. H. Dahlin and S. E. Petersson, pp. 645-648, Elsevier] Below some information from SSTs in _C MEDATLAS 2002; please confirm it is correct and discuss results in the context of the methods used (Ternois? Conte? for the SSTalk; modern analogue technique-MAT for

ANSWER This has been taken into consideration the revised version.

SSTforams? Calibration errors?). MD90-917 (South Adriatic Sea, 41N 17E, -1010.0 m) January 14.0 February 13.5 March 13.7 April 14.8 May 17.2 June 21.1 July 23.4 August 25.2 September 23.3 October 19.8 November 17.2 December 15.6 annual mean: 18.2 $\pm$ 4.2 _C MD04-2797 (Siculo–Tunisian Straits, 36N, 11E, -771 m) January 15.4 February 14.9 March 14.9 April 15.6 May 17.1 June 20.6 July 23.4 August 25.4 September 24.4 October 22.8 November 19.7 December 17.3 annual mean: 19.3 $\pm$ 3.9 _C

ANSWER Comparison between core top values for each proxy records and Medatlas present day data are now included in the result section. We emphasize the good agreement between proxy and instrumental.

TWO Deglaciation disimilarities: seasonality only? proxy and location? [689, 1: “SSTforam indicate a 2.5_C cooling... in the SSTalk record”] [689, 13-16: “The most remarkable discrepancy... South Adriatic Sea at 16.5 kyr”] [691,11-15: “Higher than ON-SSTforam values between 19 and 16 kyr (and the YD) point to preferential summer alkenone production at both sites of the central Mediterranean Sea. In contrast,

under milder BA and Holocene climates, SSTalk are close to AM-SSTforam except for the Holocene in the Siculo–Tunisian Strait region where they are similar to ON-SSTforam underlining seasonal production changes during the deglacial."] [694,5-11: "final warming to the Holocene occurs seemingly earlier in the SSTalk than SSTforam leading to an apparent shorter duration YD,... We suggest that this bias result from alkenone production shifting from spring during the BA, to summer during theYD and back to spring during the Holocene, except for the Siculo–Tunisian Strait region were Holocene SSTalk are close to ON-SSTforam.]

ANSWER The discussion relate to broad intervals of several kyrs or early deglaciation which is thus large enough to keep us away from model uncertainties (16 to 19 kyrs)

The uncertainties mentioned above (essentially NEW TABLE 1 and NEW TABLE 2, as yet to be included in FIG. 2 and FIG. 3) are crucial to discussion of the dissimilarities observed between both paleothermometers, in particular around the H1with its two phases, H1a and H1b (please use the N. pachyderma (s) percentages to locate these events as per point one) or the BA and the YD.

ANSWER We do not discuss the detail of H1a or H1b but the early deglaciation as a whole. The grey bars represent time intervals as published by Bard et al., 2000 as an indication. However, in the Adriatic sea Nps are present from 17 to 12kyrs but do not shown higher values for H1a and H1h but these events are anyway hardly distinguishable from SSTs or oxygen isotopes. In the Siculo-Tunisian strait Nps are also abundant during this time interval they tend to follow SST alk.

For the later, the hypothesis of seasonal shift from spring to summer and then to spring again in the case of the alkenones could sound interesting but I'm afraid it is hard to prove with the tools the authors have in hand. Please include the uncertainties in the figures to help with the discussion. In any case, wouldn't be easier to invoke changes due to the different location/ proxies studied? What about the comparison between SSTalk and SSTforams? The alkenone view is not expected to be exactly the



same view as forams, even when they are measured in the same strata, because a marine plant (coccolithophores) could react differently from an animal (foraminifera); what could be lethal to plants may not be so to animals, and viceversa. Additionally, climate changes in the South Adriatic Sea are not expected to be exactly the same as events described by other proxies in relatively closer regions such as the Sicily Straits, either in intensity or rates of change. The reader would appreciate clearer discussion on that.

ANSWER The whole discussion is on these aspects (seasonality habitats of production) which are different for coccolithophorids and forams and might vary when drastic environmental changes occur -and these changes are different at the two core sites, so I am not quite sure that I understand this comment. Given that uncertainties for each proxy constant with time (and we have no information to go beyond this), the observed differences between proxy records remain but might be of different magnitude which we do not comment here.

THREE Holocene Hypsithermal/ Holocene thermal maximum missing? Why is the Holocene hypsithermal not evident in the SST records? Or at least why is this feature not discussed? I very much like the approach of the authors when they try to connect land and marine environments by means of speleothems; they use La Mine [Genty et al., 2006, QSR]; that's fine; what about the Grotta di Carburangeli [Frisia et al., 2006; QR] or others from the eastern Med? In terms of humidity, changes are observed from early to late Holocene at Lake Accesa in north-central Mediterranean [Magny et al., 2007] and Lake Preola in Sicily [Magny et al., 2011]. Hence, why no changes are evident/discussed in the alkenone profiles or the foram records? What about marine cores in the area? in [Emeis Dawson, 2003, The Holocene?] the SSTalk shows approximately -4_C decrease from the 8.2k event down to the core-top sample, from 21_C to 17_C. Concerning SSTforams, do they show a warming trend for the same time span? Do the authors have any comment on the role of the increased melting of the Laurentide Ice Sheet during the early Holocene and its influence of the central

Med climate? Please consider further references for the discussion of the seasonality forcings and the Med specific case [Renssen et al., 2012, QSR] at least to explain the long-term trends. Perhaps the authors would be kind enough to provide the reader with their hypotheses/observations on the apparently missing Holocene Hypsithermal/ Holocene thermal maximum.

ANSWER The revised MS now include a short discussion on the HTM and reference to Emeis and Dawson (2004) and Renssen et al. (2012)– Regarding the comparison with other records we found it out of scope of this paper –Furthermore a synthesis paper dealing with this has been submitted in the meantime within this special issue by Magny et al. and is currently under review –reference to this paper is now quoted in the revised version of our manuscript.

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Interactive comment on Clim. Past Discuss., 9, 683, 2013.