

Interactive comment on “Parallelisms between sea surface temperature changes in the western tropical Atlantic (Guiana basin) and high latitude climate signals over the last 140 000 years” by O. Rama-Corredor et al.

Anonymous Referee #1

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General comments

This is a review of “Parallelisms between sea surface temperature changes in the western tropical Atlantic (Guiana basin) and high latitude climate signals over the last 140 000 years” by Rama-Corredor et al. submitted for publication in Climate of the Past. I truly appreciate the effort of the authors in generating a long (ca. 140 ka) sea surface temperature (SST) record in sub-millennial temporal resolution. This record has a great potential given the small amount of already available SST records from the area with a similar or better temporal resolution. The manuscript addresses a relevant scientific

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question that is within the scope of Climate of the Past. The novel and remarkable aspect in the manuscript is definitely the great SST data presented.

Having said that, I must also express my concern on some general aspects of the manuscript, as listed below.

1. In a back of the envelope calculation, the mean temporal resolution of the SST record for the 14C-dated portion of the core is ca. 400 yr. Since most Heinrich Stadials (HSs) that occurred during the last glacial lasted for ca. 1.3 kyr (HS1 excluded; Sarnthein et al., 2001. In: The Northern North Atlantic: A Changing Environment), in the best case HSs are characterized by three values. Dansgaard-Oeschger (DO) events are even shorter than HSs, and thus would be represented by an even smaller number of values. Additionally, the authors used six 14C ages to constrain the age model of the core for the whole 14C-datable portion of the core (ca.50 ka; Reimer et al., 2013. Radiocarbon). The relatively low temporal resolution of the SST record associated to the relatively coarse 14C-based age model render the core appropriate for orbital-scale investigations, but not ideal for the identification of millennial-scale events. In the Abstract, the authors themselves state that (at least for MIS3) it is difficult to unambiguously identify either DOs or HSs. Still, significant portions of the Introduction, Results and Discussion are devoted to millennial-scale events. I would urge the authors to provide an in-depth evaluation of the ability of their record to investigate millennial-scale events. I have no doubts that, if point 2 mentioned below can be convincingly satisfied, the new SST record presented by the authors is very well suited to assess orbital-scale changes, but this is not the case for millennial-scale changes. By focusing on orbital-scale changes, the conclusions would be substantial and perfectly supported by the results.

2. SST records based on alkenones may result in inaccurate values for situations of limited growth of open ocean haptophyte algae (e.g., Versteegh et al., 2001. Organic Geochemistry; Harada et al., 2003. Geochimica et Cosmochimica Acta). Typical situations of limited growth of open ocean haptophyte algae include low salinities and light

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limitation. Both conditions could be present under the influence of the Amazon River plume (Lentz and Limeburner, 1995. *Journal of Geophysical Research*; DeMaster and Pope, 1996. *Continental Shelf Research*). During the last glacial, sea level lowstand would have shifted the Amazon River plume offshore potentially affecting site MD03-2616 more intensively than today, also considering potential increases in the discharge of the Amazon River. I would appreciate to see this topic addressed by the authors. This is my main remark regarding the validity of the laboratorial methods applied by the authors.

3. Some portions of the text deserve in-depth restructuring. For instance, in section 3 Methods, the authors describe some results (e.g., the age model), while in section 4 Results the authors discuss some results (e.g., SST trends). The manuscript would profit from a stricter compartmentalization.

4. In the ^{14}C -dated portion of the core there is an age reversal. The authors only briefly mention that the ^{14}C result obtained for core depth 176 cm has not been used to produce the age model. Still, the reasons for choosing depth 176 cm as not valid instead of 148 cm, for instance, are not clear. That portion of the age model would be significantly different if the authors discard the age at 148 cm. I would urge the authors to provide a detailed rationale supporting the elimination of the age obtained at 176 cm core depth.

5. In order to correlate their SST record to the strength of the Atlantic meridional overturning circulation (AMOC), it would be of great relevance to show a record of AMOC strength spanning the period investigated in this manuscript. Such correlations are performed in different portions of section 5 Discussion (e.g., page 13, lines 16-19; page 16, lines 8-11) but no AMOC record is shown in Figs. 2-5.

6. In different portions of section 5 Discussion (e.g., Page 12, lines 23-25; page 13, 22-24), the authors refer to changes in salinity in their study area, the western tropical North Atlantic. Still, by only providing a SST curve, any discussion or conclusion on

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salinity is based on already published records or conceptual models. I would urge the authors to focus the Discussion on SST or to show a salinity record from MD03-2616.

7. The large number of specific comments and technical corrections listed below (please consider that Fig. 1 needs in-depth restructuring, see my comments below) makes the reading of the manuscript very demanding. The manuscript would greatly benefit from a detailed revision.

Specific comments / Technical corrections

Page 2, line 3: Use “37” as subscript, and rewrite the alkenone symbol, since it is not correct.

Page 2, lines 8-9: Not clear, please rephrase.

Page 2, line 11: Since MD03-2616 was also collected in the NA, this should, in principle, be expected. Thus, please rephrase for more accuracy (i.e., mentioning a specific region of the NA).

Page 2, line 13: Delete “with SSTs reaching as low as 25.1oC.”

Page 2, line 14: If the term “reminder” characterizes signals that are not very clear, delete the sentence. If not, rephrase it for more clarity.

Page 2, lines 15-17: Why not making a more direct comparison by looking into marine sediment cores (e.g. Nace et al., 2014. Palaeogeography Palaeoclimatology Palaeoecology)?

Page 2, line 25: Which hemisphere of “polar variability”.

Page 3, line 1: The Introduction is completely focused on millennial-scale events. Change the focus to orbital-scale modifications in climate if the millennial-scale information in the core is not conclusive and the more conclusive signals relate to orbital-scale changes.

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Page 3, lines 13-14: Consider including Barker et al. (2015).

Page 4, lines 10-11: In general terms, many studies assessed this issue. State a specific area if you still want to keep this sentence.

Page 4, line 17: Add “and” before “(iii)”.

Page 4, lines 24-25: Delete “a tropical region confined between Arctic and Antarctic oceanographic influence”.

Page 5, line 19: Substitute “was formed by” by “is composed of”.

Page 5, line 21: Section 2.1 is too short. Please provide some more details (e.g., mechanism controlling the position of the Intertropical Convergence Zone).

Page 6, lines 1-5: Move run-off related issues to section 2.3.

Page 6, line 10: Section 2.1 is too short. Please provide some more details (e.g., seasonal SST, annual and seasonal sea surface salinity (SSS)).

Page 6, line 11: Are the authors referring to SST? Additionally, provide SSS annual, summer and winter means (either here or in section 2.3) (also in Fig. 1). The proximity of the Amazon River mouth may generate extremely low SSS that hamper appropriate growth of open ocean alkenone producing algae. Also, consider that past periods of enhanced Amazon precipitation and Amazon River discharge (e.g., Mosblech et al., 2012. Nature Geoscience; Govin et al., 2014. Climate of the Past) coupled with lower sea-level may have resulted in significantly lower SSS at site MD03-2616, affecting the growth of alkenone producing algae (see y comment above).

Page 6, line 14: Delete “main”.

Page 6, line 20: Add “retroflexion” after “NBC”.

Page 6, lines 25-27: Provide the mean depth of the boundary between these two water masses. This is a relevant information to be considered together with the core depth.

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Page 6, lines 26-27: uNADW is placed beneath AAIW! Lankhorst et al. (2009. DSR), for instance, assigned the pressure range of 600-1050 dbar for AAIW, and 1200-2050 dbar for uNADW. Please rephrase.

Page 7, lines 7-9: Seems not to be relevant for this manuscript. If this is indeed the case, delete it.

Page 8, line 19: Add the error of your SST reconstruction.

Page 8, line 20: It is important to state in this section (and not only in the caption of Table 1) that (i) the authors performed a linear interpolation between age pointers, (ii) if any DR was used, and (iii) which software was used to calibrate the raw 14C ages.

Page 8, lines 21-22: Delete this sentence.

Page 8, line 24: Substitute “shelves” by “shells”.

Page 8, line 25: Delete “Table 1”.

Page 8, lines 26-27: “. . .identifying the biozone with the Y interval of Pulleniatina obliquiloculata disappearance. . .” reads awkward. Please rephrase.

Page 9, line 2: Delete “Fig. 2e”.

Page 9, line 3: Delete “Fig. 2d; Table 1”.

Page 9, lines 6-15: Move this whole paragraph to section 4 Results.

Page 9, lines 11-15: Please, reconsider this in light of Govin et al. (2014. Climate of the Past).

Page 9, line 16: Include an introductory subsection where you describe the age model. In this subsection, you should include the last paragraph of section 3.2 and refer to Table 1 and Figs. 2d, e and f.

Page 9, lines 19-23: In the Results, do not discuss your results, but only present them in a clear and synthetic way. Move these sentences to section 5 Discussion. Why

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"subtle"? Delete this characterization or justify it. Compare the difference in SST between the Last Glacial Maximum (LGM) and the Holocene to MARGO (2009. Nature Geoscience).

Page 10, lines 6-7: Not clear. Please rephrase clearly stating the relationship between the mentioned orbital parameters and warming/cooling trends in the western tropical North Atlantic. If therefore you are not only describing the results but need to discuss them, move this sentence to section 5 Discussion.

Page 10, lines 11-21: Move these sentences to section 5 Discussion.

Page 10, line 22: Start this section stating that first you determined an objective way to identify abrupt changes, and then describe the putative abrupt changes. Still, consider my general comment #1 above.

Page 10, lines 23-25: This belongs to the Discussion. Please move this to section 5 Discussion.

Page 10, lines 25-27: Already mentioned above. Incorporate to item 4.1, and delete from here.

Page 11, lines 1-2: Fits better to section 4.1. Please consider moving to that section.

Page 11, line 2 "...shows a maximum fall of..." Did you calculate the SST change between two adjacent samples to get to this value? Not clear, please rephrase.

Page 11, lines 6-8: Section 4.2 could start here. I urge the authors to consider deleting all sentences above in section 4.2.

Page 11, line 10: Text is not appropriate, since $\pm 0.50\text{C}$ is not a rate. Please reformulate for more accuracy.

Page 11, line 18: Define acronyms in their first appearances in the text.

Page 11, line 26: Use "variability".

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Page 12, lines 12-14: What is the mechanism therefore?

Page 12, line 16: Reads awkward. Please rephrase.

Page 12, lines 17-20: If the authors do want to discuss millennial-scale events of the last deglaciation, I would urge them to provide a figure limiting the x-axis to the last deglaciation and comparing their SST record to other appropriate records (e.g., Ruhlemann et al., 1999. Nature; Schmidt et al., 2004. Nature; Jaeschke et al., 2007. Paleoceanography; Weldeab et al., 2006. Earth and Planetary Science Letters).

Page 12, line 21: "... North Atlantic..." Be more specific, by citing a region in the North Atlantic, since MD03-2616 was also collected in the NA. Also, quantify the difference and cite the records to which you are comparing your record.

Page 12, line 23: But in this manuscript the authors are only showing temperature data. Please rephrase limiting on temperature.

Page 13, lines 5-6: Peterson and Stramma (1999. Progress in Oceanography) suggested that the SEC is primarily fed by the South Atlantic Current. Please consider this and other similar references to rephrase this sentence. Of key importance here is to cite original references, in this case, studies of physical oceanography, and not secondary references like paleoceanographic papers.

Page 13, lines 10-12: Please state possible reasons for the absence of similarity in these SST records.

Page 13, line 14: In many situations the authors claim that there is a coupling between SSTs at site MD03-2616 and "North Atlantic" records. Citing one or more of these records in the text is of prime importance. Also, including one of these records in Figs. 2-5 (or at least in one of them) is of prime importance to support the suggested coupling.

Page 13, line 17: Please be more accurate. It is not clear what the authors mean with "... ocean processes in Guiana...".

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Page 13, lines 22-24: Recurrently the authors make reference to changes in salinity off northeastern South America but one such record is not shown. I would urge the authors to base their discussion primarily on the shown SST record.

Page 14, lines 3-4: Not appropriate. Please rephrase. The way the text reads now, suggests that the end of MIS3 is a deglaciation period, but the deglaciation only starts after the LGM.

Page 14, line 5: Which events? Be more specific by citing, for instance, the section where the discussion is to be found.

Page 14, lines 5-6: Please, be more specific. This is a broad statement that would benefit from a more specific formulation as well as from citations to support the statement, since it is probably not based on the results shown here.

Page 14, line 21: Delete “or vice versa”. From page 14 line 28 until page 16 line 3: The way it is formulated, it suggests that DO oscillations, for instance, necessarily left an imprint in SST in the western tropical North Atlantic. Since this is not necessarily, I would suggest the authors to rephrase this sentence.

Page 15, line 6: Not clear why the authors referenced Fig. 5b, since b in figure 5 is the new SST record shown here for the first time. Please revise for more accuracy.

Page 15, lines 8-9: The authors are comparing their new SST record to reflectance data from Cariaco Basin, a proxy for terrigenous input in the basin (Peterson et al., 2000. Science). It would be enlightening if the authors could explain the motivation and rationale behind one such comparison.

Page 15, lines 11-14: Although the trends as calculated in the intervals suggested by the authors show the same sign between MD03-2616 and GeoB3910, these two records also present marked differences (e.g., low SSTs at ca. 40 cal ka BP in GeoB3910, high SSTs at ca. 25 cal ka BP in MD03-2616). The manuscript would greatly benefit from an in-depth evaluation of these differences as well. Also, I would

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urge the authors to compare their SST record to the one shown by Nace et al. (2014. Palaeogeography Palaeoclimatology Palaeoecology).

Page 15, line 14: What kind of terrestrial records are the authors referring to? Also here it would be enlightening if the authors could explain the motivation and rationale behind one such comparison.

Page 15, line 17: NE or SE trades? Be more specific.

Page 15, lines 18-19: Hydrological perturbations have been simulated on many other areas as well (e.g., Kageyama et al., 2013. Climate of the Past). Please rephrase for more accuracy.

Page 15, line 26: The clear changes in hydroclimate over N South America during HSs cannot be considered a "muted" reaction (e.g., Arz et al., 1998. Quaternary Research; Peterson et al., 2000. Science; Wang et al., 2004. Nature; Jennerjahn et al., 2004. Science). Also the western tropical Atlantic reacted readily to HSs (e.g., Arz et al., 1998. Quaternary Research; Weldeab et al., 2006. Earth and Planetary Science Letters; Jaeschke et al., 2007. Paleoceanography). Please rephrase for more accuracy.

Page 16, lines 1-11: This paragraph deserves special attention from the authors. The authors mention the "lack of synchrony between trends in tropical SST records and Greenland" but earlier in the text they claim that "MD03-2616 SSTs showed a remarkable parallelism with temperature changes observed in Greenland". Please make clear to which specific portions of the record you are referring to. Also, if part of the record is not showing the suggested parallelism, I would urge the authors to consider refining the title of the manuscript by using better suited terms.

Page 17, line 2: "MISb"?

Page 17, line 3: "The influence of northern waters during deglaciation..." Be more specific.

Page 17, line 7, Substitute "profile" by "record".

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Table 1: Where did the authors get the DR value from? Please include citation.

Table 1: How did you calculated these values for the different sources of information (e.g., 14C ages, *P. obliquiloculata* datum, oxygen isotopic stratigraphy)? Please provide details either here or on the main text.

Table 2: Please use “Guiana basin”, “Cariaco basin”, and “off northeastern Brazil”.

Table 3: Add a column with the probable millennial-scale event recorded in Greenland ice cores that correlate temporarily with the events identified in MD03-2616.

Fig. 1: This figure needs major restructuration. The Intertropical Convergence Zone (ITCZ) is an oceanic phenomenon; bifurcation of SEC occurs further to the S. No need to keep background world map. Crop it between ca. 0 and 90oW, and place detailed map to the right of it, saving precious publication space. Even better is to delete it, since the only information it is delivering is the location of records 1 and 2 (i.e., Greenland ice cores, that are obvious). More important is to have another detailed map with the SSS (e.g., mean annual, mean summer and mean winter) focusing on the Amazon River plume, in order to allow a more in-depth evaluation of the impact that low salinities may have had on the production of alkenones (see my comments #2 and #6 above).

Fig. 2: In the figure caption: (i) state which curve is insolation and which curve is precession (c); (ii) state what the circles in the continuous line stand for (c); (iii) for “summer solstice” specify if NH or SH (c); (iv) cite reference López-Otálvaro et al. (2009. eEarth) (e); (v) please state that the length of the colored straight lines is based on C. Why is there no trend for Greenland in MIS5e?

Fig. 3: Do not show in the figure the 14C age not used to produce the age model.

Finally, I would urge the authors to archive their data in a world data center.

Interactive comment on Clim. Past Discuss., 11, 1143, 2015.

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