

Interactive comment on “Magnitude and timing of Equatorial Atlantic surface warming during the last glacial bipolar oscillations” by S. Weldeab

Anonymous Referee #3

Received and published: 13 July 2012

I am pleased to review the manuscript authored by S. Weldeab and entitled “Magnitude and timing of equatorial Atlantic surface warming during the last glacial bipolar oscillations”. Weldeab’s manuscript targets the relationship between the Equatorial Atlantic surface ocean temperature changes and the “out-of-phase” bipolar/interhemispheric climate oscillations during marine isotope stages 4 and 3 of the last climatic cycle. Surface ocean temperature reconstructions spanning the interval between 75 and 25 ka BP are derived from Mg/Ca measurements performed on the calcite tests of the planktonic foraminifer *Globigerinoides ruber* (pink variety) along a sediment core (MD03-2707) taken at 1295 m in the Gulf of Guinea. Previous studies led by the S. Weldeab have shown the high quality of this core as a paleoceanographic archive and the suitability of the foraminiferal species and geochemical proxies used here to reconstruct the hydrographic evolution of the surface ocean in the Gulf of Guinea (Weldeab et al.,

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2007a; Weldeab et al., 2007b; Weldeab, 2012). In the present study, the Author enhances the resolution of the *G. ruber* Mg/Ca record that was previously presented by Weldeab et al. (2007a) and supports the interpretation of the down-core results with new Mg/Ca data obtained from a large number of core-tops retrieved throughout the Gulf of Guinea. Weldeab’s study indicates that the surface ocean in the Eastern Equatorial Atlantic warmed during H-events 6 to 3 and that warm conditions persisted after the end of each of these H-events. The latter evidence is at odds with both model results and paleoceanographic data from the Western Equatorial Atlantic, making this manuscript an important contribution to our understanding of the high-to-low latitude climate variability during glacial times. Weldeab’s manuscript is well written for most parts and of potential interest to the readership of *Climate of the Past*. However, in order to be accepted for publication in *Climate of the Past* the Author should convincingly address three central points, such as (a) the selection of an appropriate “calcification temperature” used to calibrate the *G. ruber* (pink) Mg/Ca core-top data from the Gulf of Guinea, (b) the robustness of the chronology upon which the MD03-2707 records are placed, and (c) the relationship between changes in the atmospheric CO₂ concentrations and variability of the surface ocean temperatures in the Eastern Equatorial Atlantic under glacial boundary conditions.

Main Points

Re: *G. ruber* (pink) Mg/Ca vs. calcification temperature. I personally find more appropriate to calculate the calcification temperature of planktonic foraminifera against which Mg/Ca calibrations are performed by using the so-called “isotopic calcification temperatures” (see e.g., Anand et al., 2003). Furthermore, this approach also allows to infer the depth of calcification of the planktonic foraminiferal species of interest (e.g., Anand et al., 2003; Friedrich et al., 2012) and thus may be helpful to improve down-core data interpretation. The $\delta^{18}\text{O}$ can be in fact measured on an aliquot of the same (homogenized after crushing) foraminiferal calcite used for the Mg/Ca measurements (see e.g., Elderfield and Ganseen, 2000; Anand et al., 2003). Hence, $\delta^{18}\text{O}$ and Mg/Ca

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data are co-registered in one and the same signal carrier and are thus a reflection of both ocean water temperature and $\delta^{18}\text{O}$ at the same time and depth in the water column (i.e., during calcification). On the other hand, it is worth noting that the use of the “isotopic calcification temperatures” relies on the assumption that the ocean water salinity (and in turn the ocean water $\delta^{18}\text{O}$ that is derived from it, e.g., by following LeGrande & Schmidt, 2006) at the time of calcification of the planktonic foraminifera found in the core top samples was the same as today. I do not want to impose my view on this, but I feel that the manuscript would greatly benefit from a discussion (if needed in the form of supplementary material) aimed at informing the reader of the suitability of the World Ocean Atlas temperatures as opposed to “isotopic calcification temperatures” for calibration purposes in the present study. As the Author states in page 1743 there is a weak correlation between Mg/Ca-derived sea surface temperatures (SST) and the ones indicated by the World Ocean Atlas. This further underscores the need for such a supplementary discussion on the selection of an appropriate “calcification temperature” estimate.

Re: chronology. Page 1746, lines 8-10 the Author states that “. . . the timing of abrupt EEA SST rises is synchronous, within the age model uncertainty, with the onsets of the Heinrich events . . .”. I think the age model uncertainties should be mentioned somewhere in the manuscript. In addition, some more details on the chronology adopted in the present manuscript should be provided. I am well aware that establishing a firm chronology for paleoceanographic records aimed at resolving millennial-scale climate relationships over large spatial scales it is all but an easy task. Some assumptions are unavoidable even in the ice core studies, which notably benefit from a wealth of dating tools. I am also aware that chronology for core MD03-2707 has been published elsewhere (Weldeab et al., 2007a; Weldeab, 2012). However, if I combine the information from a recent paper published by the Author (Weldeab, 2012) with what I derive from the present manuscript I get fairly convinced that some clarification and/or corroboration of the correlation approach between the MD03-2707 *G. ruber* $\delta^{18}\text{O}$ profile to the Greenland $\delta^{18}\text{O}$ is very much needed. The *G. ruber* $\delta^{18}\text{O}$ data reflect the in-

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terplay between the temperature of calcification and the $\delta^{18}\text{O}$ of the ocean water in which *G. ruber* calcifies. The ocean water $\delta^{18}\text{O}$, in turn, reflects the interplay between a local ocean water $\delta^{18}\text{O}$ composition (linked to salinity changes and/or to variable inputs of freshwater with potentially different sources and isotopic compositions) and a more global ocean water $\delta^{18}\text{O}$, which is conceivably modulated by changes in sea level (e.g., Waelbroeck et al., 2002).

- According to Siddall et al. (2003), the sea level changes across marine isotope stage 3 follow an Antarctic rhythm of variability. Assuming that the releases of freshwater to the ocean during millennial-scale episodes of sea level rise (of up to ~ 35 m) promote synchronously global decreases in the isotopic composition of the ocean water of 0.0085‰ per meter of sea level (e.g., Waelbroeck et al., 2002), then Antarctic warming events coincided with decreases of the $\delta^{18}\text{O}$ of ocean waters as large as ~ 0.3 ‰ also in the Gulf of Guinea.

- By using the chronology presented by Weldeab in his manuscript, virtually contemporaneous SST increases (e.g., during H-event 5, Figure 5D) of 1.5 deg. C displayed by the *G. ruber* Mg/Ca results in MD03-2707 would result in an overall *G. ruber* $\delta^{18}\text{O}$ decrease ~ 0.6 ‰ (i.e., sea level + temperature effect) across this interval.

- During H-event 5 the MD03-2707 *G. ruber* $\delta^{18}\text{O}$ increases by ~ 0.5 ‰ implying a considerable salinity change at the core site and/or a ~ 1.1 ‰ $\delta^{18}\text{O}$ increase in the ocean water in which *G. ruber* calcify (see discussion in Weldeab, 2012 concerning the potential causes of the $\delta^{18}\text{O}$ increase during H-events).

I feel that these factors collectively complicate the use of *G. ruber* $\delta^{18}\text{O}$ to tie MD03-2707 and Greenland chronologies. The above does not necessarily imply that the chronology adopted by Weldeab in his study is wrong, but it does suggest that a thorough assessment of the potential biases involved in this approach is in order here. I could also suggest that to use the benthic $\delta^{18}\text{O}$ (see e.g., Weldeab et al., 2007a) to test the robustness of the chronology adopted here. In either case, a robust chronol-

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ogy will allow the Author to base his relevant findings (and conclusions) on far more unambiguous grounds.

Page 1746, Lines 13-15. “. . . Furthermore, Gulf of Guinea SST rises were paralleled by a rise in atmospheric CO₂ concentration ranging from 12 to 20 ppmv (Ahn and Brook, 2008). We suggest that this magnitude (12–20 ppmv) of atmospheric CO₂ changes is too small to account for the observed rise of SST in the EEA . . .”. This statement should be supported by a reference and possibly further elaborated. The sensitivity of the glacial climate to CO₂-related radiative forcing could have well been larger than under present-day boundary conditions (see e.g., van de Wal & Bintanja, 2009). Accordingly, the 15-20 ppmv pulses in atmospheric CO₂ concentrations could have contributed to the millennial-scale SST increases at the Gulf of Guinea core site. I suggest adding a few lines to concisely discuss this issue with the help of key references, as I am sure it will be of great interest to the readership of *Climate of the Past*.

Minor Points

Page 1738, Lines 11-12: “. . . indicating that the Eastern Equatorial Atlantic responded very sensitively to millennial-scale bipolar oscillations of the last glacial and marine isotope stage 3. . .”. This statement appears in the abstract and aims at informing the reader of the main finding of Weldeab’s study. I think that it should be revised in the interest of clarity. Specifically, the Author has just mentioned the relationship between Eastern Equatorial Atlantic SST, North Atlantic H-events, and Greenland temperatures but the link with the “bipolar climate variability” could be missing to (at least) part of the interdisciplinary readership of *Climate of the Past*. Somewhere the relationship between Greenland/North Atlantic climate variability and Antarctic climate changes should be mentioned in the abstract. Also, if the Author decided to use the acronym “EEA” for Eastern Equatorial Atlantic – as I gather he did – that acronym should be then used throughout the manuscript.

Page 1742, Lines 4-20: I think this entire paragraph could be moved into the results’

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section. The core-top data are new results and they would be best presented in the appropriate section of the manuscript.

Supplementary Information: the supplementary figure lacks the caption, while I could not locate the Table 1 mentioned in the manuscript (page 1742, line 7).

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