

Autor's reply to reviewers' comments

I would to thank the reviewers for their insightful and constructive comments that improve the manuscript. I want also to thank Dr. Ziveri for her editorial handling of the manuscript.

Summary

The reviewers made three major recommendations.

Firstly, all three reviewers requested a detailed discussion of the age models.

Secondly, one of the reviewers recommended exploring the potential of $\delta^{18}\text{O}$ calcification temperature as a calibration option for the core top Mg/Ca data.

Thirdly, two reviewers requested a discussion on the possible role of atmospheric CO_2 in shaping the equatorial Atlantic surface warming.

Age model:

In revised version, we provide a detailed discussion of the age model, including uncertainties. We note that the analysis of benthic foraminiferal $\delta^{18}\text{O}$ record, as requested by one of the reviewer, is beyond the scope of this manuscript. More importantly we strongly believe that benthic foraminiferal $\delta^{18}\text{O}$ in the investigated core section would not contribute to improving the age model (for more discussion see below).

$\delta^{18}\text{O}$ calcification temperature:

Following the reviewer's recommendation, we analyzed $\delta^{18}\text{O}$ in test of *G. ruber* pink from 30 core top samples and calculated the so-called $\delta^{18}\text{O}$ calcification temperature. We compare the $\delta^{18}\text{O}$ calcification temperature with the Mg/Ca and SST. The comparison shows that $\delta^{18}\text{O}$ calcification temperatures suggest a range of 6°C (varying between 24.9°C and 30.9°C). For comparison, the largest spatial and seasonal SST contrast within the Gulf of Guinea is 3°C . Furthermore, the upper range of the $\delta^{18}\text{O}$ calcification temperature exceeds the warmest annual mean WOA09 SST by upto 2.9°C (Figure S1 and Table S1). We suggest that this large-scale overestimation arises because the $\delta^{18}\text{O}_{\text{seawater}}$ -salinity relationship in the Gulf of Guinea is unknown and the application of $\delta^{18}\text{O}_{\text{seawater}}$ -salinity relationship from tropical Atlantic is inadequate for the Gulf of Guinea. In the revise manuscript, we provide a detailed discussion about our calculated $\delta^{18}\text{O}$ calcification temperature.

Atmospheric CO_2 :

Referring to recent and relevant works on equilibrium climate sensitivity (changes of near-surface air temperature in response to a doubling of pre-industrial atmospheric CO_2), we estimate the possible contribution of atmospheric CO_2 rise to the equatorial Atlantic warming during the Heinrich events. We also discuss that our estimate might be not accurate because the contribution of atmospheric CO_2 changes under different climate boundary conditions may vary from that derived equilibrium climate sensitive estimate.

Below is our reply (bold) to the reviewers' major and minor comments highlighted in *italics*.

Anonymous Referee #1

Core stratigraphy

Indeed, the timing of SST and $\delta^{18}\text{O}$ across Heinrich Events at centennial resolution, and the demonstration of oceanic and atmospheric teleconnections between low and high northern latitudes is of outstanding importance to understand the climate system. Hence, the core stratigraphy is crucial to any conclusion drawn from the proxy data, and should be presented and discussed in much more detail, even if basics were already published previously (Weldeab et al., 2007). If I am correct, there are only 5 AMS14C-dates available for the discussed time period from 25-75 ka, with ages from ca. 27 to 42 ka BP (Weldeab et al., 2007, supplement). This is to my mind an insufficient data base to really pinpoint the timing between SST and $\delta^{18}\text{O}$ of core 2707 and N-Atlantic Heinrich Events. I would expect for one selected Heinrich Event at least, the improvement of the stratigraphy based on several AMS14C-datings. What kind of reservoir age was used to calculate calendar ages? Also, benthic $\delta^{18}\text{O}$ are essential to further support the proposed core stratigraphy. Why haven't such data been added as done in Weldeab et al. (2007) for Termination II?

Following the advise of all three reviewers, we provide a thorough discussion of the age model and address possible uncertainty. The age model is established by tuning the MD2707 $\delta^{18}\text{O}$ record to the GIPSII $\delta^{18}\text{O}$ record (Figure 4). The MD2707 $\delta^{18}\text{O}$ record reflects runoff changes of rivers that drain West African monsoon (WAM) area (Weldeab, 2012). As evidenced in the younger section of the record (< 25 kyr) (Weldeab et al., 2007), the WAM is tightly linked to northern high latitude climate. Therefore, we exploit the tight correlation between the West African Monsoon and Greenland climate, as record in the $\delta^{18}\text{O}$ records, to establish an adequate age models.

In overview study (Weldeab et al., 2007), we used 5 AMS datings to establish an age model for sub-orbitally resolved record. With an age model uncertainty up to 2400 and 4200 years in the section of our interest, the 5 AMS datings are not suitable for the multi-decadally and centennially resolved record that we present in this study.

We would like to stress that the demand for a benthic foraminiferal $\delta^{18}\text{O}$ record is beyond the scope of this study and has no bearing on solving of possible uncertainties of the age model. Siddall et al., (2004) seal level record and Shackleton benthic foraminiferal $\delta^{18}\text{O}$ (2000) show that the pace of changes in ice volume and deep-sea signature are very different (gradual) than that of sea surface and atmospheric imprints of the H- and DO-events. Therefore instead benthic record that may follow the gradual changes Antarctic climate, we believe that it is logical to use the monsoon record to establish an age model of MD2707 via correlation with Greenland ice core. In the revised version, we addressed the limitation and uncertainty of the age model.

Foraminiferal Mg/Ca

The author presents Mg/Ca from G. ruber (pink). It should be mentioned in this respect that there are absolute Mg/Ca differences between G. ruber (pink) and (white), and that most Mg/Ca vs. temperature calibrations were established for G. ruber white. Why did the author measure the pink variety? That should be briefly discussed. Also, different morphotypes of G. ruber (sensu stricto and sensu lato) reveal measurable differences in both Mg/Ca and $\delta^{18}O$ and point to different living depths. This topic should be discussed briefly, and the according references should be acknowledged in the reference list (e.g. Wang, 2000; Steinke et al., 2005). I may have missed it, but the authors forgot to mention the assumed living depth of G. ruber (pink)?

We focus on *G. ruber* pink (sensu strito) that makes up 85% of planktonic foraminiferal assemblage in the MD2707 material. The dominance of this species is due its tolerance for low salinity surface water (Ufkes et al.,1998). We used the global calibration curve that includes *G. ruber* pink (sensu strito) to convert the Mg/Ca data into SST estimates. In the revised version, we provide details of this species and its habitat preference with reference to works that significantly contributed to a better understanding of *G. ruber* morphotypes and their chemical/isotopic signatures (Wang, 2000; Steinke et al., 2005)

Results and Discussion:

In particular, I like the discussion on G. ruber Mg/Ca with respect to the calcite saturation state of the ocean. Also, the discussion of the salinity effect on foraminiferal Mg/Ca is very conclusive and again, stresses the large potential of foraminiferal Mg/Ca for paleoceanography.

*There is apparently a lead of Mg/Ca over $\delta^{18}O$ during the Heinrich events, with SSTMg/Ca becoming warm, and subsequently *G. ruber* $\delta^{18}O$ becomes lighter. Such a lead/lag pattern was described earlier for the deglaciation (Termination I and II) by Lea et al. (2000), Nürnberg et al. (2000), and most deeply discussed by Visser et al. (2003) for the tropical Pacific and Atlantic oceans, with a temporal offset of up to several thousand years. Even for core 2707 (as presented in Weldeab et al., 2007), such temporal lead of Mg/Ca over $\delta^{18}O$ seems to hold, most explicit for Termination II. How, in this respect, behave the benthic $\delta^{18}O$? Are salinity changes due to riverine freshwater input or strengthened evaporation at times of significant warming affecting the planktonic $\delta^{18}O$? How prominent would be such effects in $\delta^{18}O$?*

We care fully checked the timing of rapid SST rises relative to those of changes in the $\delta^{18}O$ record during Heinrich events. Taking age model uncertainty into account, we note that the only episode during which the SST rise clearly predates the changes in $\delta^{18}O$ is H-5a. Other observation is that the SST rise that started with the onset of H events and persists increasing or stays at high level when $\delta^{18}O$ starts to decrease. We addressed this observation in the original manuscript.

Further:

Fig. 1: This figure is to my mind redundant. So, if the author is asked to cut down the length of his manuscript, the very simplified ocean current pattern in Fig. 1 could be easily included into Fig. 2. The core position of the reference site GEOB3910-2 could be left out.

Since the paper focuses on cross equatorial Atlantic thermal changes and refers to SST reconstruction from western equatorial Atlantic and zonal currents, we think Figure 1 is useful for the readers and we would like to keep it.

*Fig. 2: Although very colorful, the different symbols in the xy-diagrams can hardly be differentiated. This should be improved. For completeness, the *G. ruber* Mg/Ca vs. temperature calibrations of McConnell & Thunell (2005), Lea et al. (2000), and Regenberg et al. (2009) could be included and acknowledged.*

We modified the symbols and included the *G. ruber* Mg/Ca vs. temperature calibrations of McConnell & Thunell (2005) and Lea et al. (2000) into Figure 2. We note that there is no a calibration curve available in Regenberg et al. (2009) work.

Fig. 3: The figure is informative, but could be easily included into Fig. 4. The stratigraphy of core MD2707 was already presented and discussed in Weldeab et al. (2007) in much more detail, which should also be done in this paper as the stratigraphy is essential for all ongoing interpretations. At least, the AMS14C-dates should be marked. As pointed out above, the only 5 AMS14C-datings within the period ~27-42 ka are not convincing, when trying to relate the eastern equatorial Atlantic SST variations to N-Atlantic Heinrich events and to resolve for leads and lags.

Figure 3 shows a plot of sediment depth versus sedimentation rate. So technically it is not possible to incorporate Figure 3 in Figure 4 because the latter shows a plot of age versus Mg/ Ca and d18O. In the overview study (Weldeab et al 2007), the 5 AMS datings were used to age model of sub-orbitally resolved record. In the present study, the 4 AMS datings (>30 kyr) with error estimates of 2200 and 4200 years are less suitable for an age model that focus on centennially and multi-decadally resolved record. Therefore, we tuned the rapid transition in the d18O record to those of the GISPII ice core record. See above more discussion in this matter

Fig. 4: The author should clarify also in the figure caption whether the presented data were already published elsewhere.

Done.

*Fig. 5: The authors compare their *G. ruber* Mg/Ca-temperature record, which apparently reflect annual summer temperatures from 25 m (?) water depth, to alkenone- derived SSTs from the western equatorial Atlantic and conclude that the SST development in the western and eastern equatorial Atlantic was different during the Heinrich events. It should be added in this respect, when (season?) and where (water depth?) the alkenone-derived temperature signal was most likely formed. Is it justified to directly compare temperature reconstructions derived from different proxies? Are there Mg/Ca-temperature records from the western equatorial Atlantic available, which could be preferentially consulted?*

Referee #2 raised similar questions and comments. We address these comments together.

First to the seasonality issue: Within the Gulf of Guinea, the strongest seasonal contrast is centered in the area where a shoaling of nutrient rich and relatively cold subsurface water occurs in summer (Figure 2a). Most of the core top samples are recovered from this area, and therefore we believe that the production of planktonic forams may be skewed toward summer. The core top Mg/Ca data support this interpretation. The site of MD2707 is located out side or at the periphery of the area of strong shoaling of subsurface water (see Figure 2a). Seasonal contrast of SST over the core site is the relatively weak as compared to those from the center of subsurface shoaling. The above discussion focuses on modern conditions. In previous study (Weldeab, 2012), we analyzed $\delta^{18}\text{O}$ of individual *G. ruber* pink to investigate seasonal contrast. The $\delta^{18}\text{O}$ data show that during Heinrich event and DO-stadial the seasonal contrast in monsoon strength was strongly reduced (Weldeab, 2012). The reduced seasonal contrast was due to weakening of summer monsoon. Keeping in mind that a weak West African Monsoon is accompanied by a weak Gulf of Guineas current, the weakening of the latter leads to warmer surface water during the summer, thereby reduce the winter and summer SST contrast. So one can argue that the surface warming during Heinrich events was in PART due to weak seasonal contrast and the increase of annual SST.

The second question was whether the spatial heterogeneity across the equatorial Atlantic, as observed in our Mg/Ca-based and alkenone-based SSTs (Jaeschke et al., 2007) estimates, is due to the proxies and the seasonality they reflect. Deglacial SST reconstructions based on Mg/Ca (Weldeab et al. (2006), EPSL, v. 241, p.699-706) and alkenone-unsaturation index (Jaeschke et al., 2007) from closely located sites in Western equatorial Atlantic reveal warming trend during the Younger Dryas and Heinrich events. While the results are limited to the deglaciation, they show that both methods capture the same direction of thermal changes. In the revised manuscript, we discuss this issue.

Fig. 6: Curves are difficult to distinguish! Use better colors (e.g., green) and open symbols.

Supplement download: Only one figure with SST reconstructions, no figure caption!

The abstract should start with the motivation of the work, not with mentioning the results of the core-top study. The phrasing of titles of Chapter 4.1 and 4.2 – although being appropriate - lack some fantasy and deserve improvement.

We changed the symbols, added the missing figure caption, improved the chapter titles

Referee #2

Main points:

- Mg/Ca equation derived from core-tops

The core-top database allows the author to assess the seasonality in the Mg/Ca signal recorded by foraminifera and the potential influence of salinity. However, I do not think it is necessary to generate a calibration equation, because I wonder how significant/useful is a calibration that has been derived from a SST range of less than 4°C (3.16°C, as stated in P.1743, L.20). Therefore, I think it should be presented with caution and I would not use it for the time series (P.1745, L.13 and Supplementary Figure 1). It is of course useful in order to show the low influence of salinity because it is derived from a relatively wide salinity range (compared to temperature), but the author should acknowledge that previous studies (e.g., Arbuszewski et al., 2010, EPSL) have already pointed out that salinity is not expected to have a significant effect below 35 psu in the Atlantic

I agree with the reviewer. In fact, we applied the global calibration equation (Anand et al., 2003) to convert the Mg/Ca time series into SST estimated, as stated several times in original submission (page 1742, line 18-20 and page 1745, line 20-23). In the revised version, we emphasize this fact by restating: "... the seasonal as well as spatial SST variation of 3.1°C is too small for a robust Mg/Ca–temperature calibration."

Following the reviewer's recommendation, we clarify that the findings of Arbuszewski et al., (2010) apply to salinity >35 psu. Page 8, line 8-11:

I would recommend emphasizing: - The fact that the Mg/Ca data fall within the global calibration curve and uncertainty (Figure 2c), which is very reassuring. - The smaller Δ Mg/Ca produced by the general equation compared to equations that consider salinity (although similar to the Kisakürek equation) (Figure 2d) - The unreasonable temperatures reconstructed in the time series using the Kisakürek equation.

In the revised manuscript we emphasize those aspects our findings.

On a different note, I think it will be very interesting for potential readers to see Figure 2c-d plotted with mean annual and winter SSTs and SSSs, and to be able to check how good/bad the fit is. They could be included in the Supplementary Information.

This is an excellent idea. In the revised version, we provide a supplemental figure (Figure S2) that shows annual and seasonal SST and SSS plotted versus Mg/Ca and DMg/Ca, respectively.

- Discussion about seasonality and other records:

I think there is the need for some comment on seasonality and differences with other records. The author goes through a validation process using the core-top samples in order to figure out whether salinity is playing a role and which season is being recorded by foraminifera, but then there is no reference to this latter aspect in the discussion (or at least it is not clear to me). Can the climatic interpretation be biased by the summer preference of foraminifera in this region? Can the differences with the study by Jaeschke et al. (2007, Paleoceanography) be due to the different proxies used (i.e., Alkenones and Mg/Ca recording different depths, seasons, etc.). The author should comment on these issues.

See reply to Referee #1 who raised similar comments.

Other points:

- Page 1740, Lines 15-18: Could the author provide a citation for this statement?

The discharge data were compiled from the web site (www.fao.org) of the Food and Agriculture Organization of the United Nations. A reference is included in the revised version of the manuscript

P. 1741, L. 8: Need to explain here the meaning of ΔCO_2 . Lines 3-4 in page 1742 should be moved to this section.

We followed the recommended re-arrangement (page 4, line 18-19)

- P. 1741, L. 12: It would be interesting to introduce some discussion about the cleaning method. Some authors have suggested that the full reductive-oxidative cleaning artificially lowers Mg/Ca by preferential dissolution of high-Mg calcite (e.g., Yu et al., 2007, G3). The author should explain why this cleaning was chosen instead of the less aggressive oxidative cleaning (e.g., Barker et al., 2003), which may have been enough for this region.

The depositional setting of MD2707 is dominated by high amount of terrigenous input, temporally varying between 28 and 12 g/cm²/kyr (Weldeab et al. 2011, GRL, L13703) and 35-20 cm/kyr (this study). Nd isotope study in MD2707 material show that the isotope signature of coating materials is very similar to that the terrigenous components of the MD2707 sediment (Kraft et al., (in review) GCA), indicating that high accumulation rate leads to pore water and element mobilization and precipitation of authigenic materials (Fe-Mn-coatings). Therefore, the application of reductive step is necessary to remove the coating and other terrigenous components, as shown in Weldeab et al. (2007, G-cubed).

We included a statement emphasizing the necessity of the reductive cleaning approach and refer to Barker et al (2003) al and Lea et al (2005).

- P. 1741, L. 23-26: Some references regarding “contamination thresholds” need to be introduced for reference.

We would like to comment that there is no globally accepted “contamination thresholds”. In our view, it very much depends on the Mg content of the silicates and post-depositional precipitates that vary from a depositional setting to another one. Though not without short-coming, one way to assess whether the presence of silicate/coating/infill, as indicated by elevated Al, Mn, and Fe, significantly contributes to the analyzed Mg is to look at the correlation of the diagnostic elements (Al, Mn, and Fe) to Mg. We find no significant correlation.

P. 1742, L.4-7: This repeats what has already been said before (P.1741, L. 9-11). Also, I cannot see any Table S1 in the Supplementary Information, there is only Figure S1 (without caption) (see also reference to Table S1 in P1743, L. 2).

Indeed, it repeats what was already said. We removed it. The caption of Figure S1 got somehow lost during formatting by the editorial office, and the loss escaped my attention during proof reading, we will include it in the revised version the manuscript

- P. 1742, L. 8 onwards: Most of this text should be in the Results section and not in Methods.

We moved this paragraph to the result section

- P. 1742, L. 25: This sentence needs to be rephrased. A Mg/Ca range cannot be compared to an SST range. I guess something like “The range of Mg/Ca-derived T using the global calibration equation is much larger. . .”. Also, this sentence sounds a bit obvious here as the author already said in Methods that core-tops represent summer SSTs. Once parts of the text that are currently in Methods are moved to the Results section this would be fixed. For example, Lines 3-7 in P. 1743 repeat what has already been said in Methods.

We have rephrased and re-arrange the sentence and paragraph, respectively

P.1743, L.10: I guess it refers to Figure 2, not 3. Also, I see no reference to Figure 3 anywhere in the manuscript, and there is no section discussing the age model.

We change the reference to figure and add a discussion to the age model

- P.1743, L.15: Yes, it is a problem inherent to core top studies that tends to be addressed using ^{14}C or Rose Bengal-stained benthic foraminifera.

- P.1743, L.17-18: Again, it is strange to compare Mg/Ca to SST. It would be better to refer to Mg/Ca-derived T or similar. Also, the sentence “The comparison. . . (Fig.2)” needs some revision as it is a bit confusing. I suggest removing the “to” before “the spatial” and before “sampling sites”.

We fixed the typos and grammar issue that indeed was confusing.

P.1744, L. 14: Figure 2d: In the literature, $\Delta\text{Mg}/\text{Ca}$ is usually calculated as $\Delta\text{Mg}/\text{Ca} = \text{measured} - \text{expected}$, so it is confusing to have it the other way round. I suggest changing this so the plot is comparable to other plots in the literature.

Following the reviewer’s recommendation, we changed the way the $\Delta\text{Mg}/\text{Ca}$ is calculated (see Figure 2c, and Figure S1)

- P. 1744, L. 16: I suggest adding “respectively” after “underestimate and overestimate”, because it is confusing otherwise. The author needs to better explain what he means by “ranges between 2.05 and 2.3 mmol/mol”.

We rephrased those sentences.

P. 1745, L.15-23: I agree that it makes sense to use the global calibration curve without a salinity correction in this area. However, the author should consider that the salinity values used for this correction are “Ba/Ca-based runoff-induced SSS estimates” and therefore there is uncertainty on them and this may (or may not) be causing part of the “overestimation” of the salinity effect. The author should comment on this issue.

In fact, we addressed the possible contribution of uncertainty in our Ba/Ca-derived SSS estimates to SST estimates in the caption of Figure S2. Following the reviewer’s advice, we will address the caveat in the main text

TECHNICAL CORRECTIONS

- P. 1739, L. 21: “to isolated” should read “to isolate” or “to be isolated” - P. 1740, L. 19: “during” should be substituted by “to”.

- P. 1740, L. 19 and others: The author should be consistent with the decimal figures used for temperatures and salinities throughout the paper. For example, there is one decimal figure in line 9 (32.2 psu), no decimal figures in line 14 (29 psu) and two decimal figures in line 19 (32.16 psu).

- P.1741, L. 9: write “out” after “58”.

- P.1742, L. 14: I think “annual or winter” would be better.

- P. 1743, L. 7: “... showing an r^2 of 0.22.”

- P. 1744, L. 20: units needed after “1.7”

- P. 1744, L. 25: “is much closer”

- P. 1745, L. 2: Why the “a” after the reference? There is only 1 paper in the reference list by Weldeab et al. in 2007.

- P. 1745, L. 10: No need to repeat “due”.
- P. 1745, L. 19: A caption/legend is needed for Supplementary Figure 1. - P. 1749, L. 13: I think “under- or over-estimate” might be better.
- Figure 1: Bigger symbols for core locations will be useful.
- Figure 3: “Depth” should be added at the end of the caption

We took care of all the suggested technical corrections

Referee #3

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 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.

In the revised manuscript, we present 30 paired measurements of Mg/Ca and $\delta^{18}\text{O}$ in *G. ruber* pink from Gulf of Guinea core top samples. Using these data we calculated “ $\delta^{18}\text{O}$ calcification temperature” and discuss its relationship to SST and Mg/Ca. The data enable us to address the reviewer’s points:

A calibration of foraminiferal Mg/Ca versus the water temperature in which the forams calcify can be established using the so-called “ $\delta^{18}\text{O}$ calcification

temperatures”. This approach exploits the relationship between temperature, $\delta^{18}\text{O}_{\text{foram}}$, and $\delta^{18}\text{O}_{\text{seawater}}$: $T(^{\circ}\text{C})=16.5-4.81*(\delta^{18}\text{O}_{\text{foram}} - \delta^{18}\text{O}_{\text{seawater}})$ Bemis et al., 1998). For this approach, the knowledge of regional $\delta^{18}\text{O}$ -seawater is a key requirement. $\delta^{18}\text{O}$ -seawater measurement is not available for the Gulf of Guinea. Instead, we calculate $\delta^{18}\text{O}$ -seawater over the core top sampling sites using WAO9 salinity data set and $\delta^{18}\text{O}$ -seawater–salinity relationship in the tropical Atlantic ($\delta^{18}\text{O}$ -seawater= $0.15*\text{SSS} -4.61$; $r^2=0.55$, $n=285$) (LeGrand & Schmidt, 2006).

Based on the calculation we find that:

- firstly, the use of $\delta^{18}\text{O}$ -seawater to calculate “ $\delta^{18}\text{O}$ calcification temperatures” in the Gulf of Guinea is not independent from WAO9 data set. Therefore, it does not present a real alternative to the WOA09 SST data set
- secondly and most importantly, the applicability of the $\delta^{18}\text{O}$ -seawater–salinity relationship of the tropical Atlantic ($\delta^{18}\text{O}$ -seawater= $0.15*\text{SSS} -4.61$) is not suitable for the Gulf of Guinea, as discussed below and the revised manuscript.

The Figure S1 in the revised manuscript shows a plot of Mg/Ca versus “ $\delta^{18}\text{O}$ calcification temperatures”. We obtained $\delta^{18}\text{O}$ calcification temperatures that suggest a range of 6°C (varying between 24.9°C and 30.9°C). For comparison, the largest spatial and seasonal SST contrast within the Gulf of Guinea is 3.1°C . Furthermore, the upper range of the $\delta^{18}\text{O}$ calcification temperature exceeds the warmest annual mean WOA09 SST by up to 2.9°C (Figure S1 and Table S1). We conclude that $\delta^{18}\text{O}_{\text{seawater}}$ -salinity relationship of tropical Atlantic surface water is not adequate for the Gulf of Guinea. Instead, we use WOA09 annual and seasonal SSTs to explore the Mg/Ca-temperature relationship. In revised manuscript, we provide a detailed discussion of this issue.

*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*

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

See our reply to the comments of referee #1.

*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*.*

Referring to recent and relevant works on equilibrium climate sensitivity, we estimate the possible contribution of atmospheric CO₂ rise to equatorial Atlantic warming during the Heinrich events. We also discuss that our estimate might be not accurate because the effect of atmospheric CO₂ changes under different climate boundary conditions may vary from that derived from the finding of equilibrium climate sensitive estimate.

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’ C842 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).

We took care of all minor correction and suggestion.

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