Clim. Past Discuss., 8, C829–C833, 2012
www.clim-past-discuss.net/8/C829/2012/

Climate of the Past Discussions

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

Anonymous Referee #2

Received and published: 12 July 2012

GENERAL COMMENTS

The manuscript by S. Weldeab presents a centennially resolved Mg/Ca-SST reconstruction from the Eastern Equatorial Atlantic for the period 25-75 kyr BP. The study also presents an extensive core-top data set from the Gulf of Guinea region showing that salinity does not contribute significantly to the Mg/Ca signal recorded by foraminifera in this region and that previously published general calibration equations can be applied to paleoceanographic reconstructions in this area. The record for 25-75 kyr BP shows a correlation between equatorial warming, Heinrich Events and decreasing temperatures in Greenland, highlighting the sensitivity of this oceanographic region to millennial-scale bipolar oscillations. However, while the tropical warming link with HE is consistent with previous results, its relatively long duration is not, and is at odds with

other records from the western Atlantic basin. The author puts forward the hypothesis that changes in wind-induced zonal surface currents may have caused the spatial heterogeneity in the Equatorial Atlantic. The manuscript addresses an interesting topic of relevance to the whole paleoceanographic community and I recommend its publication after some necessary reviews.

SPECIFIC COMMENTS

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 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 ΔMg/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.

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.

- 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.

Other points:

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

- P. 1741, L. 8: Need to explain here the meaning of $\Delta CO2$. Lines 3-4 in page 1742 should be moved to this section.

- 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.

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

- 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).

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

- 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.

- 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.

- P.1743, L.15: Yes, it is a problem inherent to core top studies that tends to be addressed using 14C 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".

- P.1744, L. 14: Figure 2d: In the literature, ΔMg/Ca is usually calculated as ΔMg/Ca= measured-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.

- 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".

- 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.

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 r2 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

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Interactive comment on Clim. Past Discuss., 8, 1737, 2012.