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Climate of the Past Discussions

Interactive comment on "Quantifying sea surface temperature ranges of the Arabian Sea for the past 20 000 years" *by* G. Ganssen et al.

Anonymous Referee #1

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The article by Ganssen et al. entitled "Quantifying sea surface temperature ranges of the Arabian Sea for the past 20 000 years" aims at reconstructing past changes in the seasonal temperature range over a series of time slices from sediments collected within the Somalian upwelling system. The authors use repeated isotopic analysis of planktonic foraminifera measured on single tests. Such a method theoretically permits to capture new information as compared to multi-specimen analysis methods usually applied in paleoceanography – namely the intra-sample variability – that may contain some extra climatic information. The results presented in this study are sound, and nicely demonstrate that there may have new original ways to reconstruct important climatic parameters such as those related to climate seasonality or to sea surface temperature extrema. I support the publication in Climate of the Past, but I also raise some points that, in my opinion, deserve to be clarified prior to publication.

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Interactive Discussion

Discussion Paper

I will start by two general suggestions:

First, I suggest the authors to clarify what they are interested in reconstructing more precisely. In the abstract they state that they reconstruct temperature extrema. Some parts of the discussion also suggest that the data are devoted to capture temperature maxima and minima. On the other hand, the term ”seasonality” they have chosen for heading their chapter 4 rather suggests that the authors are in search for a kind of average temperature range for winters and summers (changes in te mean seasonality). I guess the data may help to resolve both issues, but it is never clearly stated nor well shown with adapted plots which kind of climate parameter (climate extreme events or mean seasonality) are looked at.

I suggest as well to not be too eager to dismiss the possibility for past changes in d18Osw. As clearly stated all along the methods chapter, the range of d18O of planktonic foraminifera is a mixed signal of temperature AND d18Osw. In such case the results are NOT straigthforwardly linked to temperature, contrarily to what is written all along the paper.

Following on these two points, I'd like to comment on some other specific points.

On the outliers:

What has exactly been done with the outliers and what does it imply in terms of seasonality? The supplementary information doesn't help much in estimating which method is best for what you want to do, and only mentions at the end that ”Method 3 was selected”. Why? Is your choice only related to the justification you rapidly made on the gaussian distribution? Or does it make the ranges fitting better with the ocean atlas data? Such a choice (method 3) seems to make some differences in the estimations of temperature extrema for some time slices, but I'm even not sure that the ncdc atlas really contains such extrema sensus stricto since it seems to be monthly-average data. The modern temperature at core location itself does not seem to be perfectly gaussian.

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Also, why these outliers would not be taken into account? Are you expecting these outliers to be due to analytical misfunctions or to post-depositional processes? I overall find it strange to remove outliers in a study that would be devoted to temperature extremes, as temperature extremes are, by definition, kinds of outliers (meaning that you eventually tend to underestimate temperature extremes). I guess that if you want to reconstruct the average seasonality, removing an outlier won't make a big difference with the dataset you have, but if you are looking for temperature extremes then I guess outliers may be kept to capture the total range (see first suggestion). Another point to briefly discuss would be whether potential changes in d18Osw can add unwanted variability into the d18O scattering of single forams. If, for some reason, seasonal changes in e.g. currentology linked to variations in monsoon over the past 20 ka can add low-salinity surface waters at the site by some local advection process, then some outliers and/or some temperature extrema can be misleadingly considered. Which of those outliers can be impacted by such eventuality and how much salinity changes you would need to invoke to make an outlier into the accepted range?

For those reasons it would be wise to show the outliers in the figures 2 and 3 and flag them, as well as to mention which figure of the supplementary information relates to which method a bit more clearly. Once these things clarified you may want to clearly discuss the reasons for why you have chosen to consider some extreme high and low d18O values as outliers, the reasons for why you made such efforts to sort the acceptable data are not clearly justified.

On the introduction:

It is a shame that the introduction turned out to be a nice but simple list of all the methods to reconstruct sea surface temperatures, and of almost all the bibliography that looked at intra-sample isotopic scattering as it is done in the paper. Although I understand why you push the originality of the method, you may at least add one or two sentences on the reasons for why the sea surface temperature of such an interesting upwelling region is important to reconstruct, otherwise the tremendous amount of isotopic data you performed is not really justified.

On the figures:

The Figure 3 is kind of hard to visualize in a climate perspective. I would personnally have liked to see a X/Y plot of potentially interesting components of your dataset such as minimum, average, maximum, variability (sigma, i.e. the average seasonality), winter, summer or whatever temperatures as a function of age together with any other reference curve to highlight important climate events such as the Younger Dryas, the Bolling Allerod, the Heinrich event 1 and the LGM.

On the discussion:

Aren't there further important implications in terms of seasonality? Some supplementary outlook on seasonality using e.g. discussion in Denton et al. from the data point of view and Flückiger et al. from the model point of view can be a nice basis to develop some ideas and concepts, such as what drives climate seasonality in the northern hemisphere (Denton et al.), to what extent such seasonality changes can have a global impact on climate change, on local upwelling processes and/or on climate records, which kinds of linkage with the indian monsoon the upwelling seasonality implies, etc. It's a shame that such aspects are not well developped, as the end of the paper is mostly descriptive.

Overall, the paper will certainly be a nice and original contribution to Climate of the Past, and perfectly fits to the scope of the journal. I feel however that the clarity and implications can be improved, probably with only minor revisions.

References cited:

Denton GH, Alley RB, Comer GC, Broecker WS (2005) The role of seasonality in abrupt climate change. Quat. Sci. Rev. 24,1159–1182

Flückiger, J., R. Knutti, J.W.C. White, H. Renssen (2008) Modeled seasonality of glacial abrupt climate change . Clim. Dyn. 31, 633-645

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