**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 #2

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Ganssen et al. present a novel and original dataset designed to reconstruct changes in seasonality in the Somali margin upwelling system over the last 20 ky. Their method uses oxygen isotope data of individual tests of G. ruber and G. bulloides to resolve short-term extremes in sea surface temperature as captured by d18O. Their approach is generally sound and their results appear to be both statistically significant, and interesting in terms of paleoclimatic implications. The main result points to reduced maximum temperatures and overall seasonal temperature range in the glacial and deglacial interval compared with the Holocene. The paper is methodologically novel and stimulating and could pave the road for more systematic analyses of individual foraminifera to address issues of seasonality in paleoclimate studies. I recommend the paper be accepted for publication in Climate of the Past, after providing further clarifications and responses to the following queries.

1. Calibration issues

I have been unable to replicate the paper's claim that sea surface temperature at the core sites has a 14 degree seasonal range (16-30 degrees C). My first figure below shows the NCDC monthly SST data for 52E and 10N from the link provided in the paper (page 2801, line 8). The monthly range is about 6 degrees (24-30 C) not 14. I therefore ask the authors to clarify exactly how they derived the 14 degree estimate for the range, and to show the SST data they used as a supplementary figure. This point also relates to the paper's Figure 1 where the authors provide a histogram of the monthly SST values from 1960-1993. This histogram ranges from 21-30 degrees, a 9 degree range, again different from the 14 degrees stated earlier. Can you clarify how and why these estimates are different?

The authors state that the salinity variation is <0.5 psu corresponding to <0.1 per mil in d18O. Are there hard constraints on the regional seawater d18O-salinity relationship in this region that this estimate is based on? If so provide the appropriate references. A quick glance at World Ocean Atlas 05 data for this site actually shows an annual salinity range closer to 0.8 psu (see my figure 2 below), and since this does not account for interannual excursions I would take this is a minimum value. It's not very different from 0.5 psu but we ought to be as precise as possible. Are there data to substantiate your estimate of <0.5 psu?

Figure 2 shows that G. bulloides has a greater calcification temperature range than G. ruber and this appears to hold in all the samples over the last 20 ky (Fig 3 right). How is this reconciled with the earlier statement that ruber is present year-round while bulloides is predominant in the May-Oct upwelling season (page 2799, line 5)? If that's true wouldn't we expect ruber to have the larger range?

2. Outliers

The paper goes through great length to identify data outliers and reject them from the analysis. The entire supplemental section is devoted to that. However there is no com-

pelling justification for why this is necessary. Looking at the raw data there is nothing immediately suspicious about these "outliers". They appear to be valid members of the dataset and convey important information.

I can understand the concern about possible outliers because the approach here is to quantify maximum-minimum ranges, which is essentially the difference between two outliers: the lightest ruber and the heaviest bulloides d18O value (Fig 3 right). On the other hand this concern reinforces the view that this particular metric (max-min range) is not a very robust index of seasonality for two reasons: (1) the data undersample the true climatic variability (60-80 monthly estimates within about 100 years) so that the probability of capturing the true maximum and minimum is fairly low; and (2) because the estimation of the max-min range relies on only 2 values within each sample and disregards the information in the rest of the data. A more unbiased indicator of seasonality would be the standard deviation (1 or 2 sigma), which is calculated from all the data. In this case outliers have less of an impact and don't need to be excluded unless there is compelling reason such as analytical error. Using standard deviation ranges is a little trickier in this case because we are dealing with two distributions (ruber and bulloides) in each interval. One way to do it is this: replace the max-min temperature range for each species shown in Fig 3 right, with the (1 or 2) standard deviation range and then calculate the full range spanned by the two species' standard deviations combined. That would be a good alternative way to quantify the spread of values in each sample. Using standard deviations of course assumes normal distributions, but on the other hand it is less sensitive to outliers and utilizes all the data, not just the extreme values.

I am not suggesting that the authors necessarily substitute this approach for what they have done, but I do suggest that they do these calculations and check whether the results are sensitive to them. Looking at the raw data I suspect not. I think the main result of a smaller overall range in the glacial samples will remain valid, but at least this way it will be reassuring to know that it is insensitive to "outliers".

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3. Error of the mean

The estimate of the max-min range is sensitive to the mean temperature values of each species. If the ruber/bulloides means are closer together then the total range is shorter and vice-versa. So it is important to discuss briefly the error associated with the mean temperatures. There are three main sources of error: (1) the standard error of the Mg/Ca calibration, (2) the Mg/Ca analytical error, and (3) the standard error of the mean, which arises from the fact that the mean is estimated from a subsample (∼30 shells) of the true population, and is given by $\sigma/\sqrt{n}$ (standard deviation divided by square root of sample size). These aggregate errors apply to each of the two species means and I suspect they will turn out to be significant when combined. I don't think this is sufficient reason to invalidate the results because they appear systematic (at least in the glacial versus Holocene timescale). However these error considerations should be discussed in the paper.

4. Discussion

My final comment has to do with the implications of this work. This paper essentially lacks a "discussion" section. Sections 3 and 4 present the "results" and are then followed by section 5, the "conclusions". What are the implications of the findings? Why is it important to know if and how seasonality changed in this region? What are the dynamical implications for the upwelling system? How are the results potentially linked to orbital forcing or to the background climate conditions? I urge the authors to put some thought into these questions.

On a related point, the last paragraph of section 4 (page 2804, lines 16-18) is somewhat of a stretch. I don't think it is possible to "conclude that severe changes . . . occur. . ." on the basis of a single sample. I suggest using softer language and appropriate caveats to make this point.

In summary, I like this paper very much. It is a fresh way of looking at seasonality issues in the paleoceanographic record and I am sure it will have an important impact

in the field.

Interactive comment on Clim. Past Discuss., 6, 2795, 2010.

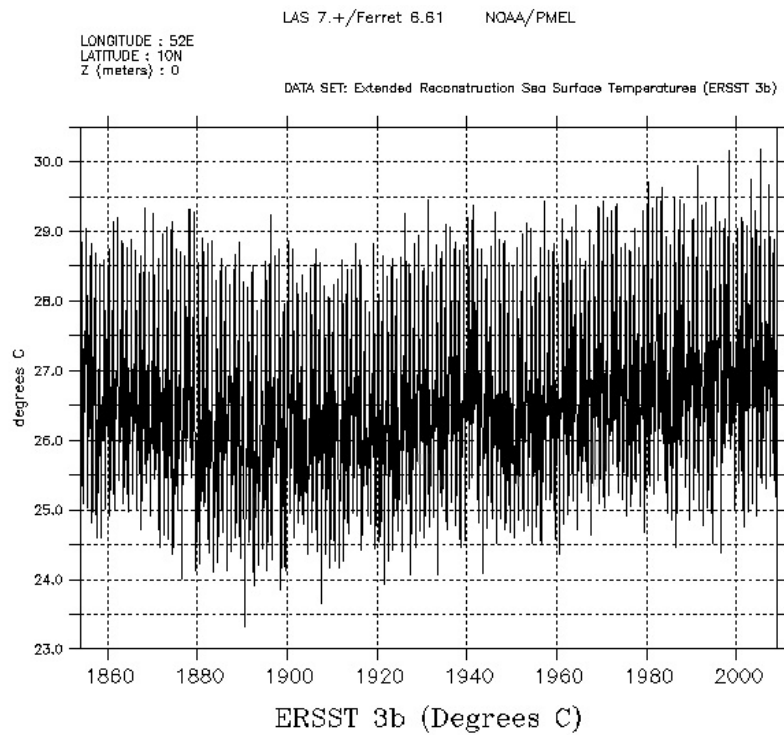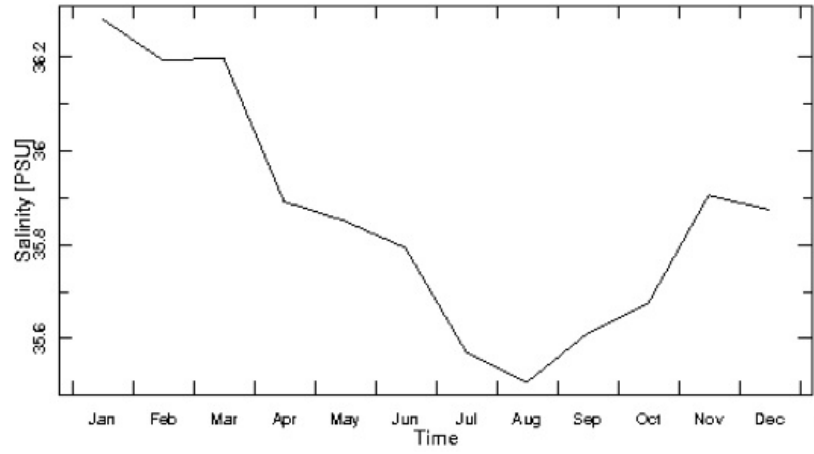**Fig. 1.** Monthly instrumental SST for the core site

Fig. 2. Annual cycle of salinity at core site

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