Author responses to Anonymous Referees for "Millennial scale sea surface temperatures of the western Arabian Sea between 37 - 67 ka BP"

For ease we have copied the referee comments above the author responses.

RC3 – https://doi.org/10.5194/egusphere-2024-865-RC3

Sorry for this long review. I really enjoyed the article, but there are several critical problems. Perhaps some of my comments can't be dealt with, and that's completely OK but please explain why you don't agree with my comment.

The article presents a high-resolution, high-quality SST and isotopic data from a marine sediment core collected offshore Somalia. The new data focus on MIS3, and are discussed along previously published data from the same core (but focusing on different time intervals) and other cores collected in the region. The analysis attempts resolving winter and summer SST variability using Mg/Ca measurements performed on planktonic foraminifera with contrasting seasonalities. And that's a difficult task - so I commend the attempt. The authors conclude that the ITCZ latitudinal movements have differential impact on the overall SST signals w.r.t. the hemisphere that controls the signal (northern vs. southern hemisphere in boreal winter vs. summer, respectively) that alter the possibility to read a clear northern vs. southern signal in the seasonal SST regimes reconstructed using both foraminifera species.

I think the article and its interpretation should eventually be published, and that EGUSphere is a perfect journal to publish in. However I think a deep and thorough revision needs to be done before.

First, in a region where such a complex seasonality (summer being colder than winter) without a description of modern climatology in a full chapter is simply not possible. Figure 1 is not helpful in that regard. Please add a full paragraph and figures to describe clearly it.

Second, a deep and thorough description of the age model, with the mandatory uncertainties and an appropriate and fair discussion on the likely very much larger uncertainties for MIS3 has to be added. There not even mention of the s.d. of the radiocarbon dates... In particular in articles dealing with MIS3 the uncertainties of the age w.r.t. the ones applying to ice cores are just so much different that you can't hide this limitation.

Third, the way you present the winter and summer equations have to be fully described in the article. It is not possible to simply refer to other articles. The reader has to have in hands a minimum of background, and certainly with an extra figure showcasing the seasonality in G. ruber and bulloides to evaluate the winter and summer equations without downloading a series of articles.

I really think those three items need to be seriously dealt with prior to any attempt to discuss the science.

Response to general comments in RC3

We thank Referee 3 for their valuable comments and time spent reviewing our manuscript. We thank them for the detailed feedback and insightful comments.

We agree with Referee 3 that a more in depth description of the modern climatology (including air sea interaction would be helpful. Our manuscript we will be updated accordingly. As touched on by all referees we will better describe the uncertainty of our age model, and as suggested by Referee 3 ensure we describe the age model methodology and robustness in more detail. Additionally, we will include a more detailed description of the winter and summer temperature equations and the foraminifera flux dynamics data that this is based on to ensure that the methodology we have used is clear and accessible to the reader.

This being said, I now start the formal review in the order it appears directly in the article.

2.3. (Age model): I don't think the authors can justify a fine-tuning of d18O to NGRIP as long as they conclude that SST don't look to any of the ice cores. Please discuss a bit more this processing. Also, please show the original d18O values and the result of the filtering on the same figure and Y axis. It is unclear which dataset is already processed.

Thanks for these suggestions. We will show original d18O values before filtering and make clear in the figures what data is being shown (filtering on it etc). We will also address the reasons for our tuning approach in the revised age modelling chapter.

2.5. (W and S SST): I find the r and b values strange and arbitrary in Saher. Please discuss this issue here. In general, I don't like the way to assign some weight in the W equation with both species and the W into the S SST estimation equation. It doesn't make any mathematical sense to me. Playing around with this set of equations could drive the math towards uncomfortable solutions. For example, very small changes in the numbers of the parameters lead me to estimate S SST uniquely with ruber... but it is mean-annual. Also, the S SST as it stands depends partly on the W SST, which doesn't make any sense to me either, etc. So, please, even if I won't agree with your interpretation in the end (I'm fine with that, and will accept your way of interpreting), at least please elaborate more discussion on the choice of your parameters because the resulting W and S SST depend crucially on the exact value of these parameters.

We will include a more elaborate assessment of the seasonal *G. ruber* and *G. bulloides* flux values from Saher and the choices we made in the winter and summer temperature estimates.

3.1. (d18O): in fact, I'm not sure that I understand it well: are the d18O are smoothed or not? Please show first the raw values.

This will be clarified.

4.1. (robustness…): please also make sure in the text and interpretation of different species and Uk'37 to be as firm as possible. You sometime state that Mg/Ca G. ruber and Uk'37 represent mean-annual SST. Then you may also elaborate why this might not be valid anymore during the MIS5, that by the way implies the season you assign to different proxies are not constant over time, that adds degrees of freedom in the interpretation to the seasonal SST changes and associated mechanisms described.

We are thankful for this comment and will streamline the discussion points on UK37 temperature data when comparing it to Mg/Ca foraminifera records (including addressing the seasonality/mean annual SST comments).

Line 210: if you have the SST estimation, a very simple calculation given the calibration equation used in Rostek will give the Uk'37 you want to calculate to investigate different calibrations. So yes, that is a very basic option.

Thanks for this suggestion, which we will consider during the revision of the manuscript. The main thrust of this manuscript is, however, to use the "seasonality" of the abundance of planktic foraminifera species to reconstruct seasonal monsoon change. Assessing different Uk'37 calibrations for data that are not our own may distract from our main aim.

Sentence lines 216-218: I don't understand your point here.

We will clarify this section.

Sentence lines 222-224: Again, a specific paragraph is absolutely needed to describe the different foram fluxes you cite here.

Please see comment above.

Line 230: it is curious: there is no foram weighting prior to foram dissolution before analyzing Mg/Ca? This should have been a very basic/classic data reported somewhere.

Previous studies (e.g. Saher 2007) have successfully used Mg/Ca thermometry in sediments from the Arabian Sea without the use of foram weights. This approach is supported by carbonate preservation studies in the region (Conan 2002). Whilst being interesting to measure foram weights, we are not sure that these data are essential for our data set.

Sentence lines 232-234: I don't agree with this statement. In this region, alkenones often/always provide a point-to-point scattering that is much lower than Mg/Ca for many reasons even when you incrase resolution.

We will re-assess our wording.

Discussions lines 244-262: please try to structure the statements, it goes a bit in any direction. For example, again, if G. ruber tolerates everything, stil its Mg/Ca fluctuates. The fact that it is mean-annual has repercussions on the meaning of your seasonal index I criticized earlier. I

also feel a larger discussion of the Ganssen data (on individual foraminifera analysis from the same species) should be detailed very much more, there is some important take-home message for you in their analysis on seasonality.

We are thankful for this comment to bring in individual foraminifera analysis (Ganssen's work) when discussing seasonality data.

4.2. (long-term…): dealing with H4, well… please be clearer on the limits of your age model. Clearly, the H4 grey bar on the NGRIP could be larger and finish right on the following D/O event, while the corresponding grey bar width on the d18O of ruber is… unknown. Specifically for H4 it has been already described that the H4 has two distinctive events, so the grey bar width could perhaps be expanded very much, having quite an implication on your discussion on the seasonality you describe for your site. Then, dealing with the SW/NE monsoon evolution, interesting model simulations available for the Holocene in Bassinot et al., 2011, Climate of the Past, could help describing the shifts in foramfluxes for different species under varying climate conditions.

We are thankful for this comment and consider placement of H4 as well as extending timeline and covering two events of H4. Thanks for pointing out the Bassinot work which we will include in our discussion.

Sentence lines 289-290: again, you may cite Ganssen and discuss his data. Also, the fact that Uk'37is even warmer than G. ruber might reveal in fact that Uk'37 is NOT the mean-annual SST.

Please see earlier comments.

4.3. (phase of…): again in the Naughton (EPSL, 2009) article there is clear 'double H4' signature. It may allow you enlarging your sedimentary sequence thickness of your own H4 and changes your interpretation.

Please see earlier comment on H4. We will also add the Naughton work to our discussion.

The ITCZ mechanism described is interesting, but already described in length in the western tropical Indian Ocean using G. ruber and Uk'37, model simulations etc. (see Wang et al., 2013, Paleoceanography). You really have to cite that article of you keep the discussion as it is (that I liked).

We will do.