Comments Anonymous Referee #1:

The article by Maier et al. presents a 43 ka-long alkenone-based SST record in the NE corner of Oman peninsula. The core location is hence potentially under the influence of the Mediterranean region, and the Asiatic and African landmasses, so many different climatic regimes could interfere, manifest, or be masked by a multitude of processes. Then it is not surprising, and very interesting, to see a SST record that look anything like what has been already published in the literature in the broader NW Indian ocean sector.

I find the authors have done a good job in interpreting their curious SST record, and I liked their choice of showcasing their record along with other alkenone-based records that are used in Figure 4 to appreciate the potential contrasted influences that surely played some role in shaping their SST record. I think the article could be published after minor revisions.

First, I suggest the authors to introduce more clearly the complexity of their SST record that comes from such a locality, and develop more on that in the conclusion. As it stands the conclusion only summarizes the main findings, but I think there is room to finish the conclusion within a broader regional context than the Arabian Sea and the Gulf of Oman only.

Response: First, we want to thank the anonymous referee for his/her helpful and thoughtful comments to improve the manuscript. To introduce the complexity of our SST more clearly we will add a further paragraph at the beginning of the discussion and the conclusion. We agree that this important aspect is indeed missing in the conclusion. Further, we will rearrange the conclusion and add an additional paragraph to bring our SST record into a broader regional context as suggested.

Other remarks:

What I found really missing was a stronger description of the age model, perhaps through adding an additional figure. Before starting to read the article the only d15N record looks so much like a series on Bond cycles for the MIS3 that I first thought that the Heinrich events were mislabeled. I had to check in the original Burdanowitz paper to be convinced by your age model, and I think it is really missing in your own article.

Response: We agree with the reviewer. To describe the age model of SL167, we will add the age-depth model based on AMS ^{14}C dates of planktic foraminifera published in Burdanowitz et al. 2024, where we first described the age model. Further, we will add the measured ages as points with its uncertainties into Figure 4. However, as a detailed description is already available in Burdanowitz et al. 2024, we will only add the reference link to the age-depth data (Pangaea data base) and will not republish the table in the current manuscript.

On your SST interpretation, with which I have no problem, it is sometimes hard to follow when you describe your curious SST record with other ones in chapters 5.1.2, 5.1.3 and 5.1.4. Particularly, for example, when you discuss the seasonality of climate patterns such as the monsoon and ITCZ along with other SST records in the northern Arabian Sea. I think the authors will find guidance if you also show the monthly SST for all individual sites that you show in Figure 4, to better highlight how changes in other SST could be partly driven by

changes in seasonal/atmospheric processes that you describe. For example, the paper by Bassinot et al. (https://doi.org/10.5194/cp-7-815-2011) shows how seasonal changes in wind could enhance/dampen upwelling in regions situated in the western/eastern parts of the Arabian Sea. Even if you consider your own alkenone as being reflective of mean-annual SST I think more discussion on how seasonal features can deeply affect the regional dynamics could be more apparent in your discussion.

Response: The reviewer raised a justified concern about the seasonal variability of atmospheric patterns. First, we will extend Figure 2 showing now the mean SST for a) January-March, b) April-June, c) July-September and d) October-December. Second, we will add the individual sites from Figure 4 into Figure 2 to show the seasonal SST variability of each site. Third, we will discuss the differences and seasonal variability in more detail at the beginning of the discussion and include this critically in the further discussion.

The combined proxy-model study on the Holocene changes in the wind and productivity patterns in the Arabian Sea area by Bassinot et al. (2011) nicely underlines the hypothesis of stronger SW monsoon impact on our SST record during the early Holocene. They found stronger SW monsoon induced winds during the summer months at the Oman Margin inducing stronger upwelling at around 9 ka compared to 6 ka. It is plausible that strong upwelling and winds increase the water mass transport from the colder upwelling region into the Gulf of Oman (Watanabe et al., 2017) and significantly lowering SST at site SL167. Further, the decreasing strength of SW winds from 9 to 6 ka (Bassinot et al., 2011) could have reinforced the rapid SST increase at site SL167 as less colder water masses from the Oman upwelling arrived at the core site.

However, we are aware that the core site is strongly affected by the SW monsoon, the NE monsoon and also NW winds. Strong changes of each of them may bias the, reconstructed annual SST towards slightly more seasonal SST.

I liked the proposition that the Persian Gulf outflow waters could have played a role on your Holocene record. On that, perhaps you could be interested in reading the article by Naderi et al. (https://doi.org/10.1002/jqs.3614) could help having an illustration of what has been happening there and in the surrounding land.

Response: Thanks for the hint to the interesting new study about the postglacial flooding and Holocene climate shift in the Persian Gulf by Naderi et al. (2024), which also underlines our findings.

Finally, I am not sure the whole discussion on the wavelet analysis really adds a value to your discussion. I am not sure whether the sun is something had really a discernible impact on your SST record, given the other processes you list during the discussion, but I don't have strong recommendation to remove it either. Anyway listing the 525, 505, 493 etc. periodicities does not add something the reader will really focus on, and there is still the possibility that your sediment sampling could add wavelet artifacts on this long list of periodicities. I find it is a shame to discuss it, it dilutes your discussion.

Response: We understand, that the spectral- and wavelet analyses may not add a big value to our discussion part. We are also aware that the resolution can bias the analyses. Especially during the time period between 15 and 21 ka BP, the resolution of the SST reconstruction is not that high, due to the low organic content in this part of the core. However, also for other measured parameters, like $\delta^{15}N$ or total organic carbon (see Burdanowitz et al. 2024), we do not see strong changes during that period. In total, the findings of the spectral and wavelet analyses of the SST record are similar to the findings in Burdanowitz el al. (2024). However, we agree that a detailed discussion of this shifts the focus of our discussion. Therefore, we will condense this part in the discussion and delete the paragraph about the spectral and wavelet analyses in the conclusion.

Other minor remarks:

There is a series of typos (and bugs at reporting the chapter numbering). Pleaser get a profound last read over the manuscript prior to submitting your revised version.

Response: We will check the manuscript thoroughly for typos.

Productivity-mediated records in your core highlighted in Figure 4 could perhaps be better used while discussing the dynamics of your core.

Response: We will bring productivity reconstruction into the discussion in more detail and link SST reconstruction more closely with each other.

Thank you again for your time and effort. Your comments and suggestions helped us to improve our manuscript.

On behalf of all co-authors,

Jan Maier