**Climate
of the Past
Discussions**

Interactive comment on "Global and regional sea surface temperature trends during Marine Isotope Stage 11" *by* Y. Milker et al.

Anonymous Referee #2

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This paper compares a compilation of temperature proxy data spanning MIS-11 to modeled temperature anomalies, in order to detect the importance of orbital and greenhouse gas climate feedback processes during this interglacial. I found the paper reasonably well written, and rich in detail. However, the downside to this style of writing is that the central point of the paper is sometimes lost in the myriad of data discussion and permutations. If I could suggest one improvement, it would be for the authors to concentrate more on how their study has expanded what we know about MIS-11. For example, we know that orbital forcing had an influence on middle-Brunhes climate, and it is not clear what this study really adds to that.

I agree with much of what the other reviewer has already said, but would add a few other points:

1. Age-model: Tie-points are notoriously difficult to identify during the middle-Brunhes period due to the low amplitude orbital cycles that are expressed in oxygen-isotope records. I therefore wondered why the authors chose to use different tie points for each of their records, some of which (e.g. Figure 2) are very subjectively defined. Would a more objective approach have been to identify a more limited number of tie-points common to all records based only of the most easily identifiable features, such as the steepest part of the TV/IV de-glacial trends? I would have liked to see a table listing all age-control points for each record, and also some indication of whether individual tie-points create spurious sedimentation rate trends; these can be a good indicator of when a record has been over-tuned.

2. SST records: Many of the SST records are attributed to mean annual temperature. However, for many of these records, especially UK'37, this attribution is empirical only, based on a good correlation to modern mean annual SST. In reality the production of these biomarkers can be heavily skewed towards a particular growth season, especially at higher latitudes. While the authors address the issue of varying SST errors in their sensitivity test for the EOF analysis, the issue of misattribution of seasonality is not addressed in the same manner. Would this alter the results?

3. Model-data comparison: Some big-picture points are missing from the data-model comparison sections. These include why it is necessary to use a data-model comparison to detect regional and global trends in SST evolution in response to orbital forcing? The proxy SST data, especially expressed in an EOF, should be enough to detect these variations themselves, minus the additional uncertainty introduced by comparing to a simulation with its own sources of uncertainty. Additionally, no mention is made of why the chosen model is appropriate for this sort of study; would a different model create a totally different set of data-model comparisons, particularly given the very small range in SST values (see next point)? Lastly, and most importantly, a central theme of this paper is to compare proxy and modeled SST data for three time slices through MIS-11, relative to a long-term baseline calculated for each dataset. What is couldn't see is a

blunt justification of why this approach (comparing anomalies rather than absolute values) is needed. Presumably it is to hide systematic differences in the two approaches and concentrate only on trends, but this needs to be stated. Lastly, the comparison between model and proxy SST anomalies is to my mind greatly hindered by the very low variability in the proxy data across MIS-11. In Figure 7, most of the proxy SST data cluster around '0' and most of the scatter could be encompassed within a 1-2oC error bar (to account for SST uncertainties and the spread of the data averaged within each time slice). So can the authors make a stronger case that the approach they take is justified for such a small range of data?

Minor points: 1. Page 836, lines 8-9: The configuration of orbital parameters between MIS-11 and the Holocene is not the same; this is why they are so difficult to align. The amplitude of variation is similar.

2. Page 836, lines 14-16: Similarly, orbital alignment is not difficult due to weak orbital forcing, but rather to a different phasing of precession and obliquity.

3. Page 836, line 23: Reference needed after 'interglacial. . .'

4. Page 840, line 23: 'what' rather than 'which.'

5. Page 842, line 8: I would suggest the authors employ only raw SST data; using previously constructed stacks only propagates the uncertainties from these constructions.

6. Page 843, lines 5 and 8: 'Principal,' not 'principle.'

7. Page 843, line 11: why were records interpolated to 1000 yr intervals when the average resolution of the entire dataset is triple this (∼3000 yrs)?

8. Page 851, line 15: Could the higher range of proxy SST anomalies be due to the influence of seasonality in the SST records?

9. Page 856, line 28: although changes in plankton ecological preference are not likely to change over one glacial cycle, change in ontogeny (migration within the water

column, change in seasonal production due to food availability etc) are likely to have taken place.

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