

Clim. Past Discuss., referee comment RC2
<https://doi.org/10.5194/cp-2021-184-RC2>, 2022
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Comment on cp-2021-184

Anonymous Referee #2

Referee comment on "Sea surface temperature evolution of the North Atlantic Ocean across the Eocene–Oligocene transition" by Kasia K. Śliwińska et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-184-RC2>, 2022

Summary

In this study, the authors generated high-resolution sea surface temperature (SST) records spanning the Eocene Oligocene Transition (EOT) using two independent organic proxies, namely algal lipid-based UK'37 and archaeal lipid-based TEX86. The study site ODP 647 is located in the North Atlantic (NA), and is thus far the most northerly NA location with a known EOT sequence. The authors interpret both UK'37 and TEX86 as reflecting SST. Comparing their records with other published NA records show that the temperature trend across the EOT is spatially heterogeneous. The authors further calculated average temperature values for 37.0–34.5 Ma and 34.5–32.0 Ma for comparison with previously published climate model output from Hutchinson et al. (2018). Proxy-derived latitudinal gradient is substantially flatter than that derived from the model output. Comparing site-specific SST anomalies further highlight data-model discrepancy; the climate model output indicates warming in the subpolar gyre in stark contrast to the cooling suggested by proxy data. The authors discuss possible factors leading to this data-model mismatch by considering uncertainties in numerous aspects of data and model output.

General comments

The topic investigated fits the remit of the journal. There is a dearth of high-resolution data across the EOT, thus the data presented by this study will make an important and timely contribution to the community. The paper is generally well-written and accessible. I did spot numerous typos in the discussion though, a thorough proof-reading before resubmission would be appreciated. Some arguments are unclear and could be further strengthened. In the following I list my major concerns which I invite the authors to consider and clarify when revising their manuscript.

(1) Does the age model support the interpretation of narrow time windows like Step 1 and EOIS?

The age model is based in part on the visual correlation between the low-res benthic d18O record from ODP647 and the high-res d18O record from ODP1218. Due to the sparse temporal coverage of the 647 d18O record, it is unclear to the reader how the authors correlated the two records. It might be helpful to mark the age tie-points in Figure 2 or add a table listing the tie-points so that the reader can judge how robust the age model is. It would also be great to add some discussion on whether the uncertainty of the age model allows any interpretation of events across the EOT. I am also slightly baffled by what the authors wrote in Line 231 "...no change in SST at Site 647 concurrent with the Step 1 or EOIS events..." as I do not see any SST data point in these time intervals (Fig. 2). Some clarification regarding the basis of this statement would be appreciated.

(2) Are TEX86- and UK'37-temperatures similar to each other?

TEX86 is a very useful proxy especially in deep-time climate reconstruction, but it is not always clear from which water depth these lipids originate and thus reflect. The authors argue that TEX86 reflects annual mean SST at their study site because they find the trend and absolute values of TEX86 temperature similar to those of the UK'37 temperatures.

- Robustness of UK'37 temperatures: Alkenones were only detected at the very end of the EOT and not for the Eocene. Does this mean that at this study site alkenone producer only started appearing during the Oligocene? What (which species) might they be? The fact that this is not the same species as modern-day precursor (*E. huxleyi*) on which the UK'37 calibration is based would in theory introduce some uncertainty in the UK'37 temperature estimates especially when interpreted quantitatively (absolute values). How does this uncertainty affect the interpretation of TEX86 that is based on the assumption that UK'37 temperatures are robust?
- Different Oligocene trends in UK'37 and TEX86: for the interval wherein both UK'37 and TEX86 data exist, UK'37 data show a strong cooling of > 5°C whereas TEX86 data shows little to no change in trend. Zooming in, one would see that UK'37 temperatures are higher than TEX86 temperatures during the early Oligocene but lower during 33.5–33.0 Ma. Wouldn't these different trends argue against the authors' assumption that both proxies are similar in values and trends?
- UK'37 calibration choice: The authors applied both Kim et al's linear regression-based calibration and Bayspar for TEX86, but only Müller et al's linear regression for UK'37. Why not also Bayspline? I think with Bayspline the abovementioned UK'37 Oligocene trend would be amplified, and further increasing the discrepancy between UK'37 and TEX86.

If the authors agree that the absolute values of UK'37 temperature are uncertain and the trends in UK'37 and TEX86 are in fact different, then the interpretation of TEX86 as SST would be unsupported. As this is a very critical point for the study (see also general comment (4) on data-model comparison), some in-depth discussion is warranted to

strengthen the conclusion of the study.

(3) Limitations of climate model

The authors mention that the 400 ppm Arctic closed simulation is not in equilibrium, and that the modern-day orbital forcing parameters were used to simulate the Eocene and Oligocene simulation. Some discussion on whether this has any bearing on the results would be helpful to convince the reader of the robustness of the conclusions (i.e. they are not affected by limitations in the model output).

(4) Data-model mismatch: Does TEX86 really reflect SST?

The authors discuss at length the discrepancy between proxy data (based largely on TEX86) and model output. The data-model comparison is based on the premise that TEX86 reflects SST, which hinges on whether TEX86 resembles UK'37 in absolute value and trend – the latter is not unequivocal (see my general comment (2)). Another curious observation is Figure 4 – proxy data based largely on TEX86 suggests a much flatter latitudinal gradient compared to that derived from the model output. A similar data-model discrepancy has been reported for the early Eocene – see the rather controversial study by Ho and Laepple (2016, *Nature Geoscience*). Ho and Laepple argue that TEX86 reflects subsurface temperatures not SST, thus an improved data-model match can be obtained when proxy data are compared to temperatures from comparable depths in the model. Might this also be the case for the EOT data-model comparison? Recently, TEX86-derived estimates at site 959 (one of the sites in the SST compilation) have been interpreted as subsurface temperatures (van der Weijst et al., *ClimPast Discussion*), at odds with the authors' interpretation. As the conclusion of this study hinges on interpretation of TEX86 (in other words the depth origin of sedimentary GDGTs), I invite the authors to carefully consider these points and present a more detailed analysis in the manuscript.

Line 25: "... This step in SST values..." briefly mention how the "step" is determined. Eyeballing? Change point analysis?

Line 112–114: Unclear how the correlation was established. Please provide more details, e.g. age tie-points or statistical technique used.

Line 127: UK'37 proxy was proposed by Prah1 and Wakeham 1987, by modifying the UK37 proxy proposed by Brassell et al 1986. Please cite the original papers instead of later studies that applied this proxy.

Line 130: Bayesian statistics-based calibration for TEX86 (Bayspar) was used, so why not

also Bayspline? As the UK'37 values are rather high in this record, the choice of calibration might matter. Using Bayspline may yield higher SSTs with a larger magnitude of change compared to those obtained using the Müller et al 1998 calibration.

Line 181–183: See my general comment (2).

Line 183–187: I find this argument a little confusing. Based on the results of a culture experiment, Qin et al. proposed that archaea may change their GDGT distribution in response to changing oxygen concentration in their living environment. Since the authors interpret TEX86 temperature as SSTs, the implicit assumption is that the sedimentary lipids must necessarily come from planktonic archaea living in the mixed layer. It thus follows that it would be more logical to assess the O₂ concentration in the habitat of the archaea in the upper water column rather than that of the depositional environment of the lipids after cell lysis.

Line 189–190: On average, <10% of the organic matter that is produced in the photic zone ends up in marine sediments at the seafloor. All organic matter in the marine system is subject to degradation, GDGTs included. As for no sharp increase in BIT – this would only be apparent if there is a large change in O₂ in sediments, e.g. in turbidite sequences. But the fact that we do not see it does not mean there is no degradation of OM. Please rephrase the sentence to improve clarity.

Line 198 & 207–208: See my general comment (3).

Line 216–219: See my general comment (2).

Line 231–232: See my general comment (1).

Line 242: "...decreased more permanently." The usage of "permanent" here is a bit confusing. Please reword or clarify. Would "substantial" or "prolonged" work better in this context? Also correct it throughout the manuscript.

Line 272–276: See my general comment (2). Just because the global core-top data fits better with annual mean SST does not mean that the alkenones at site 647 reflect the annual mean too. Previous studies have reported a better fit between North Atlantic core-top UK'37 and seasonal SST (e.g. Tierney and Tingley, 2018 *Paleoceanography and Paleoclimatology*). Also, might it be an idea to compare the TEX86 temperatures to the summer SST in the model?

Section 5.2 and 5.3: Spotted numerous typos. Please proof-read before resubmission.

Line 318–320: The temperature maximum and minimum mentioned here is based on one or a few data points. Are these statements supported by the data presented, given the uncertainty in age model and proxy noise?

Line 323–324: It IS really based on only one data point. Please provide more robust evidence or rephrase the sentence.

I think Section 5.1 and 5.3 can be merged, or at least 5.3 follows 5.1, before the discussion on data-model comparison.

Line 332–336: Is it possible that the EOT cooling at site 647 is also caused by a long-term shift in the gyre boundary?

Figure 3: It is difficult to tell apart the colors in panel B and to match the lines to the site locations. Perhaps try a different color palette? It might also be helpful to add a legend.