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Reply on RC2

Kasia K. Śliwińska et al.

Author 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-AC3>, 2022

We would like to thank reviewer 2 for her/his time and effort reviewing our manuscript. Our replies to the comments are marked as bold text.

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 heterogenous. 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. **Thank you for the overall positive assessment. The spelling errors will be corrected, and the concerns are addressed below.**

(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 $\delta^{18}\text{O}$ record from ODP647 and the high-res $\delta^{18}\text{O}$ record from ODP1218. Due to the sparse temporal coverage of the 647 $\delta^{18}\text{O}$ 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.

We thank the reviewer for their attention to the age model. The stratigraphic model across the EOT in the ODP 647 is the same as presented by Coxall et al. 2018.

Below, we include some explanation of the age modelling approach for clarification. We then made some text revisions that we hope both clarify age model issues in the ms text and also address the reviewer's questions about uncertainties, especially in relation to identifying the narrow events EOT-1 and EOIS.

There are few biostratigraphic datums at Site 647, and age constraints rely mostly on P-mag tie points (see Firth et al., 2013). The discontinuous coring at the site also creates relatively large sampling gaps (and thus depth uncertainties on P-mag reversal tie points). This is most extreme close to the E/O boundary with Core 29R having the greatest sediment disturbance eliminating any coherent P-mag signal. Thus there was a depth uncertainty of max +/- 9 m on the position of the Top C13r/Base C13n. Despite this, an age model has been produced (Firth et al., 2013). Coxall et al., 2018 adjusted this age model with reference to their benthic foraminifera $\delta^{18}\text{O}$ stratigraphy. The $\delta^{18}\text{O}$ data help 'test' and slightly refine the age model as follows: (from Coxall et al., 2018 SI section)

"In other EOT deep sea sequences, combined $\delta^{18}\text{O}$ and magnetic reversal stratigraphy has shown that high $\delta^{18}\text{O}$ values diagnostic of the EOGM $\delta^{18}\text{O}$ increase ('Step-2' of Coxall et al., 2005; Oi-1 of Coxall and Wilson, 2011, Zachos et al., 1996; Katz et al., 2008) reach a peak close to the base of magnetochron C13n, while the prior and first phase of the EOT transition (Step-1 of Coxall et al., 2005, and 'EOT-1' Coxall and Wilson, 2011, Zachos et al., 1996) occurs in the previous reversed polarity zone C13r. Firth et al., 2013 use 270.93 mbsf as the age tie-point for the C13r/C13n reversal boundary at Site 647. Due to the sampling limits of the paleomagnetic analysis, there is a +/- 9 m uncertainty associated with this horizon (See Supp. Table S2 in Coxall et al. 2018). Our benthic $\delta^{18}\text{O}$ sample from 269.79 mbsf falls within the zone of P-mag uncertainty. Since it has (what we interpret as) a 'pre-EOGM' $\delta^{18}\text{O}$ value (and, thus pre- C13n value), it most likely occurs within C13r. We can therefore shift the C13r/C13n reversal depth up to 265 mbsf, which is a revised estimate of the P-mag reversal position after (Firth et al., 2012) that is consistent with the P-mag constraints and $\delta^{18}\text{O}$ chemo-stratigraphy."

This also reduces the depth uncertainty on the C13r/C13n reversal to ca. +/- 4 m. This revision was integrated in the Coxall et al., 2018 age model and used here.

While there is room for small differences in interpretation, we feel that the main

position of the $\delta^{18}\text{O}$ shift, and therefore the EOT, constrained by both P-mag, and $\delta^{18}\text{O}$ chemostratigraphy, is robust. There is apparently no major change in sedimentation rate at Site 647, suggesting the section is continuous (Firth et al., 2013). Independent comparison of the $\delta^{18}\text{O}$ to the Site 647 benthic $\delta^{13}\text{C}$ is also consistent with the P-mag / $\delta^{18}\text{O}$ age framework, both locally and globally (see Coxall et al., 2018), increasing our confidence in the chemostratigraphic approach.

With respect to the new TEX_{86} SST records therefore, importantly we have confidence in our interpretation of the relative positions of the signals discussed, i.e.

- the prominent cooling signal detected is pre-EOT

-there is no apparent temperature change straddling the acute phases of Antarctic glaciation comprising EOT-1 and EOIS (although these are not discernible in the Site 647 records directly, only by comparison to the Site 1218 $\delta^{18}\text{O}$ records).

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.

Our point was that we do not see any permanent temperature shifts related with these events. It is true that we do not have any data point corresponding exactly with these events, but even if there was a SST change/decrease related with these events, it was short-lasting. We will clarify this.

(2) Are TEX_{86} - and UK'37-temperatures similar to each other?

TEX_{86} 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 TEX_{86} reflects annual mean SST at their study site because they find the trend and absolute values of TEX_{86} 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 TEX_{86} that is based on the assumption that UK'37 temperatures are robust?

It is well known that alkenones produced in these times are not from *E. huxleyi* as this species has been around for only a few 100 kyrs. Many studies have shown, however, that the ancestors show a quite similar response of the UK37 to temperature compared to modern day producers (e.g. Villaneuva et al., 2002). Naturally, this introduces some uncertainty, similar to using extinct foraminifera or dinoflagellates to reconstruct palaeoceanographic conditions or GDGTs to reconstruct temperatures, but generally this uncertainty is considered minimal compared to other proxy errors. The fact that the alkenone producer started to appear since the Oligocene is also a common observation, i.e. alkenones really became more ubiquitous starting in the late Eocene at the end of a long term cooling out of the Eocene hothouse (Brassell et al., 2014). We now

briefly mention these uncertainties in the manuscript.

- 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 $> 5^{\circ}\text{C}$ 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?

Above 180 mbsf we observe that the SST derived from Uk37' become more similar to the TEX_{86}S , while below that depth they are similar to the TEX_{86}H . This is shown in the Supplementary figure S2. However, in the interval of our interest, i.e. in the earliest Oligocene where TEX_{86} and Uk37' derived SST values overlap, the temperatures are fairly identical. The slight increase in the offset between TEX_{86}H - and TEX_{86}S -derived temperatures, and the shift of the Uk37' derived SST record are however not the focal point of this paper, which is concerned with SST evolution across the EOT.

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.

The calibration proposed by Muller et al. (1998) is by far the most commonly used UK37 calibration and is also overlapping with the calibration for *E. huxleyi*. The calibration by Muller et al. (1998) mostly uses surface sediment data from the North Atlantic and should therefore be excellently suited to estimate UK37 SST for our core site. We will briefly discuss the reasons for choosing this calibration.

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.

See our points above. Yes, there is uncertainty with each proxy, both UK37 and TEX_{86} , as is the case with other temperature proxies applied in the Paleogene. We do observe an overall match in absolute values and trend but indeed, there are times of differences which may not be surprising considering the widely different ecologies of their source organisms. These uncertainties will be addressed in the revised manuscript.

(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). **The 400 ppm Arctic closed run was branch off from the more similar 800 ppm Arctic closed run. It is probably a little too warm still in the deep ocean, but the surface temperatures reach equilibrium quicker. We will show a time series of the AMOC to demonstrate the equilibrium of the circulation.**

(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.

There have been many studies giving extensive discussions on whether TEX₈₆ reflects SST and we do not want to reiterate this discussion here. From this literature it appears that the source organisms are not living in the uppermost surface waters and are mainly dominant in subsurface waters. Nevertheless, studies have also shown that the TEX₈₆ can reasonably predict surface temperatures and trends in deep time, in particular if there are no major differences in trends between subsurface and surface layers (the main point of the van der Weijst et al. 2021, Climate of the Past Discussion paper). Since it is not possible to predict in advance if TEX₈₆ is able to reconstruct surface conditions, we used an unambiguous surface temperature proxy, the UK37, to establish whether the TEX₈₆ can reasonably estimate past SST at our study site. We feel that, within uncertainties given and as outlined in the main document that it does. Nevertheless, we will add a more detailed discussion on this topic in the manuscript.

Line 25: "... This step in SST values..." briefly mention how the "step" is determined. Eyeballing? Change point analysis? **We will add that this step is "visually observed" in the record. Change point analysis is a statistical technique better suited for higher resolution data sets.**

Line 112–114: Unclear how the correlation was established. Please provide more details, e.g. age tie-points or statistical technique used. **Tie-points are presented in the supplementary material.**

Line 127: UK'37 proxy was proposed by Prahl 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. **Good point, we will add these references instead of Rodrigo Gamiz et al. 2015**

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. **See our comments above. The Muller et al. calibration is by far the most commonly used UK37 calibration and mostly uses surface sediment data from the North Atlantic and should therefore be excellently suited to estimate UK37 SST for our core site.**

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.

Qin et al (2015) argued that the predominant times when O₂ levels would affect the TEX₈₆ is at times of low oxygen concentrations in large parts of the water column, i.e. oceanic oxic events and oxygen minimum zones. Therefore, we discuss a potential evidence for low oxygen conditions at our core site. In fact, it is extremely rare to encounter low oxygen conditions in the surface waters in the last few 100 million years. Note that we do not argue here that the archaea are living in the surface waters: plenty of studies have shown that they predominantly come from the subsurface waters. However, organic matter transport may bias the signal towards the upper part of the water column.

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. **We will rephrase it to make it more clear**

Line 198 & 207–208: See my general comment (3). **Addressed in reply to comment (3)**

Line 216–219: See my general comment (2). **This is addressed in our reply to general comment 2**

Line 231–232: See my general comment (1). **This is addressed in our reply to 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. **This will be clarified and made consistent 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 Paleoclimatology and Paleoclimatology). Also, might it be an idea to compare the TEX₈₆ temperatures to the summer SST in the model? **We will add a comparison of the proxy data with summer SST from the model simulations to the supplementary material.**

Section 5.2 and 5.3: Spotted numerous typos. Please proof-read before resubmission. **This will be corrected**

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? **We will address this issue in the new version of the manuscript**

Line 323–324: It IS really based on only one data point. Please provide more robust evidence or rephrase the sentence. **We will in the new revision clearly admit this and take out the word “arguably” in the sentence “the variability does not rely on individual data points except arguably for the minimum at 35.7”.**

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. **All sections in Section 5 have some form of data-model comparison. Section 5.1 is all about the realism or not of the general Atlantic absolute temperatures, section 5.2 moves to general Atlantic temperature variability across the EOT, and section 5.3 moves then more specifically to the variability of the local site temperature. We will improve the logical outlay by updating the headings of the sections to clarify how they logically follow from each other. Specifically, the section will be**

5.1 Absolute SST values in the NA

5.2 SST change across the EOT in the NA

5.3 SST variability across the EOT at site 647.

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? **This is what we are suggesting: as marked by the SST drop, the site area remained under the influence of the polar gyre**

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. **We will change to a different colour palette, as suggested**