

Interactive comment on “Oligocene TEX₈₆-derived seawater temperatures from offshore Wilkes Land (East Antarctica)” by Julian D. Hartman et al.

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We would like to thank Referee #1 for his/her extensive and constructive review. The major concern of R1, which is also that of R2, is that our attempt to quantitatively disentangle temperature and ice volume from the benthic $\delta^{18}\text{O}$ record requires too many assumptions. We understand this concern and agree with the reviewers. We agree that the most important message of this paper is to provide the first long-term Oligocene SST record from close to the Antarctic margin, and we will place the focus on this particular aspect in the revised version of our manuscript. In addition, we will discuss the TEX₈₆-based temperature reconstructions, related uncertainties, and their oceanographic and climate implications in a qualitative way. We will also discuss multiple scenarios that might explain the differences and similarities between the

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TEX86-based temperature record, the benthic $\delta^{18}\text{O}$ record and the Mg/Ca-based bottom temperature record, instead of focusing only on the link between temperature and ice volume. Below, we give a point-to-point response to the comments of R1.

ORIGINAL COMMENT:

1) Temperature and ice volume In Section 4.2, the authors attempt to use TEX86 SST estimates to disentangle temperature and ice volume from the deep-sea delta ^{18}O signal. However, there are several caveats to this approach. These include: (1) a possible summer bias in TEX86 estimates, (2) uncertainties in the location of deep-water formation, (3) a "poor age model" (the authors words, not mine!) and (4) low sampling resolution (line 416 to 421). As such, I remain unconvinced by the discussion that follows. This is problematic given how much time the paper devotes to it.

Unless you can present additional lines of evidence which support your conclusions (e.g. GCM simulations c.f. Liu et al., 2009), these results are highly speculative.

REPLY:

We agree that our quantitative estimates of the ice volume effect in $\delta^{18}\text{O}$ records based on independent temperature reconstructions from our TEX86-based SST record involves many assumptions. Although we feel that we had objectively discussed these assumptions and associated speculation in our manuscript, the shared concerns of R1 and R2 on this matter indicate that we should revise this part of the discussion. In our revised manuscript, we will limit the discussion to a qualitative assessment of our new temperature record and its influence on the benthic foraminifer $\delta^{18}\text{O}$ record. Irrespective of all the assumptions involved, we remain confident that our TEX86 record convincingly shows more profound temperature changes both on orbital and on long-term time scales than previously appreciated (e.g., Lear et al. 2004), in an area that is close to the region of deep-water formation (the Wilkes Land-Adélie Coast margin has been identified as a region of deep-water formation during the Eocene by Huck et al. 2017, Paleoceanography). This would imply that a larger portion of the high-amplitude

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benthic $\delta^{18}\text{O}$ can potentially be explained by southern high-latitude climate change and a smaller fraction by ice volume changes. However, we will refrain from quantifying the amount of variation in the benthic $\delta^{18}\text{O}$ record that could be explained by our data. Instead, we will focus the discussion on how other aspects, such as the moving of the Southern Ocean polar fronts and the position of deep-water formation, could explain the differences and similarities between the benthic $\delta^{18}\text{O}$ record, the TEX86-based temperature record, and the Mg/Ca-based bottom-water temperature record.

Numerical model simulations of adequate time interval (i.e. geographical boundary conditions of the Oligocene) and spatial resolution are currently not available, although we have initiated collaboration with modelers to produce such simulations in the near future. We choose to leave the model outcomes for a future paper, and solely focus on the data in this current manuscript.

ORIGINAL COMMENT:

In this paper, the authors show an intriguing temperature offset between two contrasting lithologies. They argue that the laminated carbonate-rich marls reflect glacial cycles, whereas the bioturbated carbonate-rich deposits reflect interglacial cycles. However, I have two concerns: Firstly, this relies heavily upon Salabarnada et al. which is currently in review.

REPLY:

We are aware that it would have been difficult for reviewers to assess the veracity of papers that are still under review. This is also one of the reasons why we chose to submit our papers (Hartman et al., Bijl et al., and Salabarnada et al.) to *Climate of the Past*, as Copernicus enables all reviewers (in fact everybody) to access papers under review (and join in on those discussions) for the purpose of their own review.

Indeed, the lithological interpretations are not part of this paper and are entirely from Salabarnada et al. Crucially, the interpretation of the lithofacies as being representative

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of glacial versus interglacial deposits in Salabarnada et al. are based on the integration of the facies (characterized on the basis of sedimentological data, physical properties, and geochemical data) independently of our TEX86 results, and merely stem from bottom current and pelagic sedimentation variations. We want to point out that Salabarnada et al. already submitted replies to their reviews (see the reviews and rebuttals at <https://www.clim-past-discuss.net/cp-2017-152/#discussion>). The consistent offset between TEX86-based SSTs between the lithologies supports the independent interpretation of the lithofacies by Salabarnada et al. very nicely. This is to us additional support for a SST signal preserved in TEX86.

ORIGINAL COMMENT:

Secondly, there are other mechanisms which could account for this variability. These include: a) oxic degradation and differential degradation of core GDGTs and/or b) changes in the Thaumarchaeotal community (e.g. deep vs shallow ecotypes). The latter is particularly important to consider as“ : :the abundance of ‘shallow’ versus ‘deep water’ Thaumarchaeotal communities at deep water sites, like Site U1356, could be affected by the presence of sea ice and the relative influence of (proto-)Component Deep Water upwelling” (lines 220-222). One way to assess this would be to compare GDGT-2/3 ratios for glacial and interglacial deposits (see Taylor et al., 2013 but also Kim et al., 2015; GCA). You may also want to revisit Littler et al. (2014; P3), as they observe a similar offset in TEX86 values between laminated and homogenous marls during the Cretaceous.

REPLY:

a) The effect of oxic degradation has been studied by Huguet et al. (2009, OG) on turbidites from the Madeira Abyssal Plain. Although differences between TEX86 values from unoxidized and oxidized sediments have been observed, these are not consistent and it seems therefore that differential degradation of isoprenoid GDGTs does not play a role (Huguet et al. 2009, OG). Instead, oxic degradation may lead to an increased

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relative influence of soil-derived isoprenoid GDGTs, which could bias the TEX86 in different ways depending on the composition of the soil-derived isoprenoid GDGTs (Huguet et al. 2009, OG). An increased relative contribution of soil-derived organic matter to marine sediments can be identified using the BIT Index (Hopmans et al., 2004; Weijers et al., 2006). Following this approach, we have discarded nine samples with a BIT Index above 0.3, indicating that the terrestrial contribution could potentially have affected TEX86 values. We acknowledge that this is currently not mentioned in the manuscript and we will add this discussion in a revised version of the manuscript.

b) GDGT2/3 values are available in Table S1 in the Supplementary Information. For the revised manuscript, we will add a figure displaying the GDGT2/3 ratios to facilitate easy comparison between glacial and interglacial lithologies. Although there is considerable variability in GDGT2/3 ratios throughout the record (which is why we refrain from using TEX86-L), it cannot explain the differences in TEX86 between glacial and interglacial sediments. Nor can it explain the long-term trends in our data.

ORIGINAL COMMENT:

Finally, you also have quite large variations in TEX86 values even within the same lithofacies (e.g. 31 Myr ago). Is this a true climate signal? Or are there additional controls on the GDGT distribution?

REPLY:

Considering that we have excluded all known biases due to soil-derived input, methanogenic and methanotrophic input, and oxic degradation, and the fact that there is no relation between the TEX86 values and the GDGT2/3 ratio, we interpret the variation in TEX86 as a true temperature (climate) signal. We agree that there is indeed quite some variation within the bioturbated facies around 31 Ma, and will provide and discuss possible explanations for this variability in the revised version of the manuscript.

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3) Summer Bias The authors argue that TEX86 values are biased towards summer SST (lines 353-361). Although this observation has also been made for other high-latitude sites during the Oligocene (e.g. ODP Site 511), it is quite speculative and is based upon the assumption that ancient high-latitude GDGT export is similar to the modern. This is quite an assumption! Therefore, do you have any other evidence for a summer bias?

REPLY:

The potential summer bias in high latitude TEX86-based SST reconstructions has been discussed extensively in other papers (e.g., Sluijs et al., 2008; Bijl et al., 2009; 2010; 2013). Like in these warm past climates, we expect that primary productivity in the Oligocene Southern Ocean was in sync with seasonal availability of light irrespective of the presence of sea ice. Transport of isoGDGTs likely requires fecal pelleting to sink effectively to the sea floor, and therefore depends on the presence of larger zooplankton that feed on the phytoplankton. This means that, like today, TEX86 temperature reconstructions are likely skewed towards the season with highest primary productivity, i.e. the summer. We will explain this more clearly in a new version of the manuscript.

ORIGINAL COMMENT:

4) Comparison with CO₂ records You observe significant temperature fluctuations in your record (up to 10 °C). This may be partly related to glacial/interglacial variability. However, is there also a potential role for CO₂? (see Zhang et al. (2011) and Anagnostou et al. (2016) for recent Oligocene CO₂ estimates). It might be worth showing these CO₂ records in Figure 4 too.

REPLY:

We agree with the reviewer that atmospheric CO₂ concentrations were likely the driving factor of Oligocene glacial-interglacial variability (DeConto et al. 2008; Liebrand et al. 2017). However, locally, surface water temperature variability can respond very

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sensitively to glacial-interglacial climate change and in a non-linear way, e.g., via the migration of ocean fronts. The discussion about the forcing mechanism for Oligocene high-latitude glacial-interglacial climate variability lies beyond the scope of this paper. Moreover, the resolution of the existing CO₂ reconstructions for the Oligocene (Zhang et al. 2013) (as well as those for the Eocene (Anagnostou et al. 2016)) does not capture the glacial-interglacial variability seen in our record. We will therefore refrain from adding a CO₂ reconstruction to Figure 4, as it would suggest a correlation between the two, which we cannot say based on our results.

ORIGINAL COMMENT:

5) Branched GDGTs: Although the authors have analysed branched GDGT (see Supp. Table), MBT/CBT values were not reported. However, this could provide important insights into continental air temperatures during the Oligocene and would be an interesting addition to this paper.

For example, how do MBT/CBT values compare to TEX86 estimates? Do they exhibit the same temporal trends? Are they offset? Do they max out? etc etc.

REPLY:

Although we agree that data on Oligocene air temperatures would be a great addition to our understanding of Oligocene Antarctic climate evolution, we fear that branched GDGT (brGDGT) data obtained from Site U1356 are unable to provide reliable information on this front. The reason for this is the very low absolute and relative abundances of soil-derived brGDGTs (BIT<0.3), which is probably caused by the too large distance to shore and/or limited soil development and subsequent transport from the land. In addition, a substantial portion of the samples analyzed has #rings-tetra values higher than 0.4 (see Table S1), which indicates that temperature estimates for these samples are likely affected by a contribution of in situ produced brGDGTs (Sinninghe Damsté, 2016). All of the above makes it difficult to infer a reliable long-term atmospheric temperature trend at this stage.

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ORIGINAL COMMENT:

6) Calibrations This study uses the linear calibration of Kim et al. (2010). However, it is important to note that Kim et al. (2010) plots SST on the y-axis (see Kim et al. 2010; Fig.5). As such, this calibration will suffer from a regression dilution bias and should not be used.

REPLY:

Referee #1 is correct in stating that the calibration of Kim et al. (2010) suffers from a regression dilution bias caused by the uncertainty in the measured TEX86 values plotted on the x-axis, and we shall add this potential complication in the methods section. This bias causes flattening of the slope (Hutcheon et al. 2010, BMJ) and therefore affects TEX86-based temperature reconstructions at the lower and upper end of the calibration range. However, because TEX86 in our record lies around the middle of the total TEX86 range used for the linear calibration of Kim et al. (2010), reconstructed temperature values for Site U1356 will not be severely biased.

Indeed, the BAYSPAR method is the only TEX86 calibration that is not affected by the regression dilution. However, by applying the BAYSPAR method, only core tops within a selection of 20°x20° grid boxes are used, thereby excluding all of the low-temperature Southern Ocean core-top calibration values (Fig. 3). Considering that Site U1356A is a high-latitude Southern Ocean site, we did not want to only use BAYSPAR and, therefore, plotted the linear calibration of Kim et al. (2010) for comparison. Although this linear calibration shows large scatter at the lower temperature end, it has been shown that all Southern Ocean core-top values in the Kim et al. (2010) dataset fall within the standard error ($\pm 5.2^\circ\text{C}$) of the linear calibration of Kim et al. (2010) (Ho et al. 2014, GCA). In addition, the fact that the temperature reconstruction based on the Kim et al. (2010) calibration compares very well with the BAYSPAR-based temperature record (Fig. 3) indicates that the effect of regression dilution bias is, in our case, relatively minimal.

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In the revised manuscript we will clarify our choice to use both the BAYSPAR and the Kim et al. (2010) linear calibration.

ORIGINAL COMMENT:

This paper also flips between different calibrations and there needs to be some consistency in the text and figures. For example, in Figure 3 you calculate SSTs at IODP 1356 with TEX86 (Kim 2010) and BAYSPAR (T&T 2015). However, ODP Site 511 is only shown using TEX86 (Kim 2010). Why not also recalculate with BAYSPAR?

REPLY:

We agree that there are some inconsistencies in the use of calibrations in the text and figures. We shall recalculate the temperatures of ODP Site 511 using BAYSPAR and add them to Figure 3.

ORIGINAL COMMENT:

Similarly, you only use only BAYSPAR in Figure 4, despite the manuscript stating that Kim et al. (2010) was the preferred calibration (line 241).

REPLY:

To clarify, we do not state that we prefer the linear calibration of Kim et al. (2010) over the BAYSPAR calibration. This is why we also apply the BAYSPAR calibration. The offset between these two calibrations is, however, only 0.5°C, well within the calibration error of both regressions. Because of the small temperature difference between the two calibrations it is only for convenience that we showed only one calibration in Figure 4. In the revised manuscript we will plot both reconstructions in Figure 4.

ORIGINAL COMMENT:

Minor comments:

There are a few other TEX86 datasets which might be of interest to the authors;

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a. Wade et al, (2012) -> TEX86 values span the earliest Oligocene (ca. 34-32 Ma). b. Zhang et al., (2011) -> TEX86 values span most of the Oligocene.

REPLY:

TEX86 data of Wade et al. (2012), and Zhang et al. (2013, not 2011) clearly show that the early Oligocene did experience a latitudinal temperature gradient. Although a valid observation in itself, these low-latitude regions are not the focus of our study. Our study focusses on near-Antarctic SST estimates and trends and how these relate to our current knowledge of the Oligocene Antarctic ice-sheet dynamics.

ORIGINAL COMMENT:

66: state CO2 estimates for the Oligocene here

REPLY:

We will state CO2 estimates for the Oligocene

ORIGINAL COMMENT:

93: specifically, ISOPRENOIDAL glycerol dialkyl glycerol tetraethers

REPLY:

We will insert 'isoprenoidal'

ORIGINAL COMMENT:

94: Careful of the wording here. If you have GDGTs, you will likely have other fossil organics preserved. Do you mean that these are the only fossil "paleothermometers" which are preserved?

REPLY:

We mean specifically fossil paleothermometers.

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101-106: this should probably go into the discussion.

REPLY:

We agree with the reviewer that this should not be part of the introduction. This section will be transferred to either the methods section 2.6 on TEX86 calibrations or the discussion of the revised manuscript.

ORIGINAL COMMENT:

102: BAYSPAR is not strictly a local/ regional calibration. It searches the global coretop dataset for TEX86 values which are similar to the measured value and draws regression parameters from these modern locations.

REPLY:

We agree that BAYSPAR is not strictly a local/regional calibration. By selecting only TEX86 core-top values similar to the ones measured in Oligocene samples at Site U1356 BAYSPAR constructs a calibration based on multiple modern-day analogue sites and therefore is not regional. We shall therefore be more careful using the word 'regional'.

ORIGINAL COMMENT:

141: space needed between oceanography and are

REPLY:

A space will be added

ORIGINAL COMMENT:

140: should this read "Palaeoceanographic setting"?

REPLY:

Paleoceanographic setting would be a better heading.

ORIGINAL COMMENT:

194: they are not always minor components. For example, in arid and/or alkaline settings they can be the major components (e.g. Huang et al., 2014)

REPLY:

Indeed, isoGDGTs are major components in arid/alkaline soils when compared to brGDGTs. However, compared to in marine sediments, the concentration of isoGDGTs in soils is always low. We shall therefore not alter this statement.

ORIGINAL COMMENT:

202: why can #ringtetra discriminate between marine and soil-derived GDGTs?

REPLY:

This information is not explicitly given in the manuscript and we shall include this in the revised version. To answer the question, #rings-tetra can discriminate between marine and soil-derived branched GDGTs as the composition of soil-derived GDGTs typically show high amounts of the acyclic tetramethylated GDGT-Ia, while a dominance of cyclic tetramethylated (Ib, Ic) (and also pentamethylated) brGDGTs has been attributed to in situ production within the sediments (Sinninghe Damsté 2016).

ORIGINAL COMMENT:

208-220: it has been shown that TEX86-L does not work (e.g. Taylor et al., 2013; Hernandez-Sanchez, 2014) and I think that this discussion can be shortened significantly.

REPLY:

Agreed. A less detailed discussion is sufficient by stating earlier that TEX86L does not work due to its sensitivity to changes in the GDGT2/3 ratio as shown by Hernandez-Sanchez et al. (2014).

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ORIGINAL COMMENT:

209: these are 'assumed' to originate from Thaumarchaeota. But they can have many sources!

REPLY:

We shall change the sentence to "In marine sediments these lipids are assumed to originate mostly from cell membranes of marine Thaumarchaeota, because they are one of the dominant prokaryotes in today's ocean and occur throughout the entire water column".

ORIGINAL COMMENT:

221: Do you have any constraints on water depth? If the site is less than 100m depth, then the presence of "subsurface" archaea is likely to be minimal. However, if the site is >1000m, then subsurface archaea might be an important contribution to the sedimentary GDGT pool.

REPLY:

Today the water depth of Site U1356 is 3992 m (see material & methods section). We have no quantitative constraints on water depth during the Oligocene, but the sediments, characterized by hemipelagic deposits reworked by bottom currents and distal gravity flows, as well as biota suggest a deep-water setting in the Oligocene (Escutia et al., 2014 Develop. Mar. Geol.). It is therefore certainly not a shallow (<1000 m depth) site. Regardless, it is more likely that GDGTs derived from shallower waters (<1000 m depth) are more effectively transported to the sediments through a.o. fecal pelleting (Schouten et al., 2013 and references cited therein). If deep archaeal communities would have contributed to the sedimentary GDGT pool, this would result in a higher GDGT2/3 ratio for these samples, which will have only a minor effect on the TEX86 (i.e. TEX86H) index (Hernández-Sánchez et al. 2014).

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280: The Methane Index does not flag methanogens. Rather, it can identify anaerobic methanotrophs (i.e. ANME).

REPLY:

In line 280 proxies for identifying methanotrophs have mistakenly been lumped together with those identifying methanogens. We shall correct for this.

ORIGINAL COMMENT:

286: What are “non-temperature related influences on TEX86”? It might be useful to go into a little more detail here...

REPLY:

We will elaborate more on ‘non-temperature related influences on TEX86’, such as archaeal growth rates or an input from methanogenic Euryarchaeota (Zhang et al., 2016 Paleocyanography), in the revised manuscript.

ORIGINAL COMMENT:

283-284: You state that 8 had high BIT values, but only 5 were discarded? Why?

REPLY:

We meant to say that in addition to the eight samples with high BIT values, another 5 were discarded based on high GDGT-0/Cren or Methane Index values. As all samples with too high BIT values also had high GDGT-0/Cren or Methane Index values, a total of 13 samples showed indications of input from methanogens or methanotrophs. We will clarify this in the revised version.

ORIGINAL COMMENT:

317: To be clear, BAYSPAR is based upon “581 coretops, including the 426 sites from the Kim et al. (2010) calibration study and an additional 155 sites from regional coretop TEX86 studies (Leider et al., 2010; Shintani et al., 2010; Ho et al., 2011; Wei et al.,

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2011; Chazen, 2011; Shevenell et al., 2011; Fallet et al., 2012; Jia et al., 2012; Hu et al., 2012)". See Tierney and Tingley (2015) for more details.

REPLY:

We shall mention the additional 155 sites aside from the Kim et al. (2010) calibration set.

ORIGINAL COMMENT:

319: Can you explain where the 3.5°C error bars come from. I thought that BAYSPAR gave you 90% uncertainty levels rather than a definitive calibration error.

REPLY:

Referee #1 is correct in stating that the BAYSPAR program gives you 90% confidence levels for each sample. Assuming that the error is normally distributed around the mean, the lower and upper 90% confidence interval boundary can be calculated as the mean plus or minus 1.645 times the standard error. As for each sample the mean, upper and lower confidence interval are known, the standard error can be calculated. On average the calculated standard error for all of the samples is $\pm 4.2^\circ\text{C}$. The $\pm 3.5^\circ\text{C}$ standard error was miscalculated based on BAYSPAR giving the 95% confidence levels. We shall make this calculation in the methods section and correct this error.

ORIGINAL COMMENT:

331: What was the paleolatitude of the Wilkes Land section during the Oligocene?

REPLY:

We shall add the paleolatitude of Site U1356 to the manuscript, which is 58.86°S .

ORIGINAL COMMENT:

Line 404: how exactly does this resampling work? It is not clear from the text.

REPLY:

Resampling is done by predicting the $\delta^{18}\text{O}$ value from a LOESS fit through the glacial and interglacial $\delta^{18}\text{O}$ values. We shall explain this in the text.

ORIGINAL COMMENT:

Figure 5: Most of the data in Figure 5 is already in Figure 4. As such, I would recommend removing it.

REPLY:

We shall consider removing this figure. However, the variance around the mean is not always clearly visible when the long-term trend is not removed.

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