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Climate of the Past
Discussions



# *Interactive comment on* "Oligocene $TEX_{86}$-derived seawater temperatures from offshore Wilkes Land (East Antarctica)" *by* Julian D. Hartman et al.

**Anonymous Referee #1**

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In this paper, Hartman et al. use the TEX86 proxy to reconstruct sea surface temperatures (SST) along the East Antarctica margin during the Oligocene epoch. TEX86-derived SST estimates range from 10 to 21°C and are broadly consistent with the dinocyst assemblage (Bijl et al., in review; this volume). However, TEX86 values can fluctuate significantly (up to 5-8°C) over relatively short time intervals (i.e. < 500kyr), suggesting additional controls on the GDGT distributions. Part of this variability could be related to glacial/interglacial climatic variability (Salabarnada et al; in review); however, it cannot explain all of the variance. The authors then compare TEX86-derived SST estimates to published deep-sea delta18O estimates and argue that a significant portion of the deep-sea delta18O record can be attributed to temperature change. This implies that variations in Antarctica ice volume may have been less severe than previ-



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ously thought.

The development of a long-term Oligocene SST record is a welcome contribution to the Cenozoic paleoclimate community. However, the TEX86 is quite complex and highly variable, suggesting additional controls on the GDGT distributions. The paper is also quite speculative, especially when discussing the potential interplay between temperature and ice volume (e.g. lines 421-439). Understanding the link between temperature and ice volume is a very important question; however, I do not think that this low-resolution (and complex) dataset can answer this question satisfactorily.

Finally, several key conclusions (e.g. interglacial/glacial cyclicity) rely upon two companion papers which are currently in review (Salabarnada et al; Bijl et al). Until these are published, I unable to fully assess the veracity of this paper.

I have explored some of these comments in more detail below:

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 delta18O 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.

2) TEX86 offsets and high variability

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.

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.

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

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?

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







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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.

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.

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 dissolution bias and should not be used.

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?

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

Minor comments:

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

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.

66: state $CO_2$ estimates for the Oligocene here

93: specifically, ISOPRENOIDAL glycerol dialkyl glycerol tetraethers

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?

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

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

141: space needed between oceanography and are

140: should this read "Palaeoceanographic setting"?

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)

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

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.

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

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.

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

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

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

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., 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.

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.

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

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

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

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