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

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We thank Stephen Gallagher for reviewing our manuscript and for acknowledging the value of our dataset. His annotations to our manuscript showed us that some sections lack clarity. In particular the last part of section 4.2, which involves the "thought experiment", and section 4.3 that discusses the reconstructed temperature variability. Also, considering the comments of Referees #1 and #2, we have decided to significantly restructure those sections, placing more emphasis on the role of paleoceanography (polar fronts), and to refrain from quantifying ice volume changes. In the revised manuscript we will discuss several scenarios that can explain the differences and similarities between our TEX86-based temperature record, the benthic δ18O records and the Mg/Ca-based bottom-water temperature record. We believe that this approach will

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improve the structure and clarity of the manuscript.

ORIGINAL COMMENT:

This is very good new organic proxy dataset from offshore Wilkes Land. The authors present a near field palaeotemperature record that although is much lower in resolution compared to other proxy datasets it sheds light for the first time consideration of the long term sea surface temperature evolution of this Wilkes Land margin.

I have made extensive comments and suggestions in the attached annotated text to this paper.

REPLY:

All comments and suggestions in the annotated text were clear, mostly they were related to the choice of words or to incorrect English and suggestions will be included in a revised manuscript. The reviewer's major comments are addressed below.

Comment: In the annotated manuscript, in line 238, the reviewer asks for clarification on why TEX86 would not have been influenced by subsurface temperatures in the absence of low-salinity waters due to sea ice melt.

Answer: It has been shown that the sea-ice influenced, low-salinity surface waters of today's Southern Ocean contain virtually no Thaumarchaeota in the top layer of the water column (0-45 meter below sea level (mbsl), Kalanetra et al. 2009 Environ. Microbiol.). Instead, the GDGTs are derived from Thaumarchaeota in the deeper water column (45-105 mbsl, Kalanetra et al., 2009 Environ. Microbiol.) and, therefore, TEX86 does not represent a true surface water signal (see also Kim et al., 2012 Geophys. Res. Let.). Dinoflagellate cysts in the same sediments suggest that oceanographic conditions were similar to today's Subtropical Front and that no sea ice was present. Hence, we conclude that TEX86 at Site U1356 does reflect a surface water temperature. We will clarify this in a new version.

Comment: In line 251 Stephen Gallagher has placed a question mark at the word 'prior'

in the sentence “The prior for site U1356 is obtained from recent clumped isotope measurements (Δ47) on planktonic foraminifers from Maud Rise (ODP Site 689) (Petersen and Schrag, 2015), which show early Oligocene temperatures of 12°C.”

Answer: the BAYSPAR method is based on Bayesian inference and therefore requires a prior distribution of temperature (i.e. the prior) in order to predict sea surface temperatures from the observed TEX86 values. In general, the prior is our initial belief or scientific understanding of the unknown quantities to be estimated, in this case sea surface temperature (Tierney & Tingley, 2014). As for deep time temperature reconstruction this prior cannot be based on modern-day annual mean sea surface temperatures, the BAYSPAR method requires a user-specified mean and variance for this prior (Tierney & Tingley, 2014). Therefore, we use previous estimates of southern high-latitude early Oligocene seawater temperatures (12°C based on Δ47) as a prior for our TEX86-based sea surface temperature reconstruction. We will better explain this in the revised manuscript.

ORIGINAL COMMENT:

I would like to add the following to the discussion: Reference to EAIS volume changes in line 70 page 3: As I iterated in my review of the Bjil et al submission to this volume: I appreciate the utility of using isotopes to interpret Antarctic Ice Sheet variability as summarise by Liebrand et al (2017) (www.pnas.org/cgi/doi/10.1073/pnas.1615440114) and this approach is used extensively when discussing the Cenozoic greenhouse ice-house transition. However, there are other sections that have been interpreted using backstripping and stratigraphic data in the Gippsland and New Jersey margins that reflect glacio-eustasy in the Oligocene and relative ice volume (Gallagher et al., 2013), it would be useful to consider the significance of these near field and far field sections in any section reviewing ice volume variability. This paper also considers the apparent instability of the EAIS during the Oligocene and presents a sea level curve with Oi events (Figure 6 in Gallagher et al; at slightly higher resolution that the present study) that bears striking similarity to the temperature curve presented in this paper (Figure 4

in this submission).

Gallagher, S. J., G. Villa, R. N. Drysdale, B. S. Wade, H. Scher, Q. Li, M. W. Wallace, and G. R. Holdgate (2013), A near-field sea level record of East Antarctic Ice Sheet instability from 32 to 27 Myr, Paleoceanography, 28, doi: 10.1029/2012PA002326.

REPLY:

We agree that the sea level curve reconstructed in this paper shows the same long-term trends as our temperature record. In our new version of the manuscript, we will sketch several scenarios to explain the differences and similarities between our TEX86 record and the benthic δ18O record in a more qualitative way. As this will likely involve ice volume changes and therefore global sea level changes, the paper by Gallagher et al. (2013) will be a nice addition to this discussion, providing a framework for our theories.

ORIGINAL COMMENT:

More specific comments are below:

Line 95: The core recovery in the Wilkes Land section is certainly not “complete”

REPLY:

The reviewer is correct in this and we will revise the sentences where we give the impression that Hole U1356A is without hiatuses.

ORIGINAL COMMENT:

Line 145 page 5: These are modelled plate tectonic reconstructions.

REPLY:

We will correct this. Indeed tectonic reconstructions show that Australia and South America were closer to Antarctica. We meant to say that numerical modeling of ocean currents shows that the strength of the circum-polar current was limited by these narrow

gateways (Hill et al. 2013).

ORIGINAL COMMENT:

Line 160 reference to Bijl et al paper in Jl Micro to be cited?

REPLY:

We will cite Bijl et al. (2018, J. Micropal.) at the end of this line to refer to the position of M. escutiana.

ORIGINAL COMMENT:

Line 315 I agree to a certain extent about the lack of identification of Oi events due the gaps in the record (not unexpected during glacials near Antarctica), however, Oi2 is not near 32 Ma (Figure 4) it is actually near 30 Ma and it is possible there is core of this event in the section (see possible correction of Figure 4).

REPLY:

We will correct for this mistake and place Oi-2 near 30 Ma in Figure 4. Although it might be possible that Oi-2 is recorded in U1356A, age model limitations prevent us to be certain.

ORIGINAL COMMENT

Pages 13 and 14: This section is very interesting yet requires significant clarification, I have suggested ways to enhance the message and tone down the “speculation” in this section hopefully these suggestions help.

REPLY:

We will reduce the speculation by refraining from a quantitative comparison between TEX86 and benthic δ18O.

ORIGINAL COMMENT:

In conclusion, once the text has been clarified and the suggestions considered this will be another useful addition to the relatively sparsely documented Antarctic (palaeo)climate and oceanographic records.

REPLY:

We thank the reviewer for these positive comments.

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