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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.

Julian D. Hartman et al.

j.d.hartman@uu.nl

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Firstly, we would like to thank Referee #2 for his/her thorough feedback on the manuscript. We recognize that the major concerns of R2 are the same as those of R1, for which we have written a more detailed reply letter.

ORIGINAL COMMENT:

In this paper Hartman et al., present Southern Ocean TEX86-derived temperature estimates from the Oligocene. They then use the TEX86-derived temperature reconstruction to de-convolve a δ18O of benthic foraminifera from an equatorial Pacific site into the δ18O of seawater component to infer relative stability of the Antarctic ice sheet during the Oligocene. I think the TEX86 record is a useful contribution to our under-

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Discussion paper

standing of temperature conditions around Antarctica during the Oligocene. However, I found the “thought experiment” inferring Antarctic ice volume stability (lines 419-420), particularly given the limitations of TEX86, unconvincing. The authors are very mindful about the limitations of this "thought experiment" but inferring ice volume stability feels over-reaching. I think a revised manuscript should focus on the regional oceanography and polar frontal systems. I also found it difficult to comprehensively review this manuscript because it references two submitted articles Salabarnada et al., and Bijl et al.,) related to the lithology and surface water conditions.

REPLY:

Similarly to our reply to R1, we agree with R2 and will focus on the relation between temperature and δ18O in a more qualitative way, and redirect our discussion more towards the nature of the high variability in the SSTs in a revised version of our manuscript. In addition, we agree with Referee #2 that the discussion is too much focused on the one scenario that links the TEX86 variability to the δ18O variability, while the discussion on the role of oceanography (polar front shifts) and its link to temperature changes is limited. Indeed, the role of oceanography is also very important for the stability of the cryosphere (Sangiorgi et al. 2018, Nature Comm.) and it is therefore highly interesting to analyze our temperature data in light of possible oceanographic changes. In the revised manuscript we will therefore explore scenarios that involve the potential role of shifting polar frontal systems over Site U1356 to explain the TEX86 variability.

We are sorry that Referee #2 felt unable to comprehensively review this manuscript because it refers to two submitted papers (Salabarnada et al. and Bijl et al.), both in review in CP. By submitting the three manuscripts back-to-back in Copernicus journals, we hoped to enable all reviewers to openly access them for the purpose of their own review. We understand that submitting 3 back-to-back papers implies that the reviewers should read and evaluate all three papers to comment on one of the them. Salabarnada et al. submitted their rebuttals at at https://www.clim-past-discuss.net/cp-2017-

152/#discussion and the reviews for Bijl et al. can be found here (https://www.clim-past-discuss.net/cp-2017-148/#discussion). We believe that none of these reviews suggest a major reconsideration of our conclusions.

ORIGINAL COMMENT:

There are many fundamental assumptions the authors make and explore within the manuscript with respect to relative contribution of temperature and ice volume in the δ18O of benthic foraminifera from Site 1218:

1) Temperature bias in TEX86 is to summer and deep water production (in the modern) is to winter. The authors note this throughout the manuscript but this is a difficult temperature disconnect to constrain. What is the seasonal range in temperatures from summer to winter today? The winter temperatures during deep water formation are constrained because seawater freezes at -2C. Summer temperatures, particularly in a warmer world, could vary by a lot. In particular, the TEX86-derived temperatures are nearly twice that of the Mg/Ca bottom water temperature record.

REPLY:

We agree with the reviewer that this temperature disconnect is difficult to quantify for the Oligocene, based on our data. In general, quantification of temperature disconnect for past periods before monitoring became available is always difficult and has at best to rely on proxies (and their uncertainties). Because of this, we do agree we should refrain from quantifying the temperature signal in δ18O using our TEX86 results. However, we would like to point out that the temperature at the locus of deep-water formation could have changed profoundly in the past. Deep-sea δ18O changed considerably over the ice-free Eocene (Zachos et al., 2008), which can only result from changes in deep-water temperature in the absence of ice sheets. Although on Oligocene glacial interglacial time scales we expect SST changes to be in part affected by the migration of polar frontal systems, we cannot rule out that winter temperatures at the locus of deep water formation changed as well. After all, the locus of deep water formation is to some extent predefined by geographic boundaries, next to the physical properties of the water that cause the water to sink. Therefore, the possibility that a significant part of the benthic foraminifer δ18O variation of Site 1218 is related to glacial-interglacial winter temperature variability remains. We shall focus the discussion more towards this point in a revised version of our manuscript.

ORIGINAL COMMENT:

2) No subsurface temperature bias in TEX86. Given that the temperatures vary by >10C and this assumption relies on a submitted manuscript (Salabarnada et al.,), I found this assumption difficult to evaluate. My main concern is that “interglacial” and “glacial” temperatures are related to lithology. The packaging and flux of TEX86 to the deep ocean is likely very different during these times: “interglacial” temperatures are during bioturbated carbonate-rich periods and “glacial” are during laminated silty periods. Also, are there post-depositional processes that might influence TEX86 estimates due to the change in lithology?

It seems given the uncertainty and variability in the temperature reconstructions, all calibrations should be discussed, including the subsurface ones. The BAYSPAR calibration itself (Tierney and Tingley, 2015), which is discussed in some detail, uses regional factors such as the vertical temperature gradient and related subsurface TEX86 influence to reconstruct temperature.

REPLY:

The sedimentation of Site U1356 during both glacial and interglacial periods is characteristic for a deep-water distal setting, dominated by fine-grained turbidite overbank and hemipelagic depositions, that are reworked by bottom currents of different intensities (stronger during the interglacials). There is therefore lithologic variation between glacial and interglacial time periods, but we believe the processes responsible for the lithologic variability are not the ones that typically change GDGTs. Post-depositional processes, such as oxic degradation, do not affect the ratio between the various isoGDGTs

(Huguet et al. 2009, OG). To investigate if there is any relation between lithology and archaeal community changes, we checked if there is any correlation between lithology and GDGT2/3 ratio, which can identify potential contributions of subsurface GDGTs. However, no relation between the lithological facies and the GDGT2/3 ratio has been found. We agree that this should be more adequately discussed, and will change this in the revised manuscript.

As Referee #2 mentions, there is significant variability in the temperature reconstructions across the various calibrations to (sub)surface temperature (Figure S2). However, in our materials & methods section we thoroughly discuss why most of these calibrations cannot be applied at Site U1356.

ORIGINAL COMMENT:

3) The authors mostly dismiss the Lear et al., 2004 Mg/Ca bottom water temperature record. This is odd because the Mg/Ca record is from the same site as the δ18O of benthic foraminifera (Site 1218) so it should be discussed in some length. The authors note changes in Mg/Ca of seawater and carbonate ion may influence the Mg/Ca-based temperature reconstruction. There are many uncertainties about the Mg/Ca paleotemperature sensitivity to changes in Mg/Ca of seawater (Evans et al., 2016 and for a nice discussion see Lear et al., 2015 for ice volume estimates for the Miocene to present) but the relative direction in bottom water temperatures shouldn't be an issue. The fact that the TEX86 and Mg/Ca-derived temperature estimates have different trends can't be explained by Mg/Ca of seawater changes. Additionally, the benthic foraminifera used in the Lear et al., 2004 study are an infaunal species, largely insulated from longterm changes in in carbonate ion (Lear et al., 2015, Ford et al., 2016).

REPLY:

We agree with Referee #2 that we too easily dismissed the Lear et al. (2004) Mg/Ca bottom water temperature record. We thank Referee #2 for pointing out to us that the benthic foraminifera Oridorsalis umbonatus used for reconstructing the Mg/Ca record of Site 1218 is infaunal and therefore to some extent, but not completely insulated from long-term changes in carbonate ion concentrations, and also for citing Lear et al. (2015), which states that this species is insensitive to changes in the Mg/Ca ratio of the seawater. We shall revise the discussion of the Lear et al. (2004) benthic Mg/Ca record in the revised manuscript. Instead of focusing mostly on the relation between our TEX86 record and the benthic δ18O record of Site 1218, we shall discuss several scenarios that will try to explain the differences and similarities between the long-term trends and the variability of our TEX86-based surface water temperature record, the Mg/Ca-based bottom-water temperature record of Lear et al. (2004), and the δ18O record more extensively.

ORIGINAL COMMENT:

4) Given the changes in lithology and the offset between the glacial and interglacial LOESS curves is constant, I'm not sure resampling the "glacial (values above average _18O) and interglacial (values below average δ18O) δ18O trends at Site 1218" (lines 403-405 is the best approach. In fact, I think much of the discussion in the section "4.3 sea surface temperature variability at glacial and interglacial time scales" is poorly supported given the uncertainty in the age model, lithology, and TEX86-based temperature estimates. A more thoughtful approach would a comparison figure of mean δ18O of seawater estimates from 1) LOESS TEX86 and a LOESS δ18O of benthic foraminifera and 2) the high-resolution Mg/Ca and δ18O of benthic foraminifera.

REPLY:

Alternations between laminated and bioturbated carbonate-rich sediments allow us to identify orbital glacial-interglacial cyclicity. Although we cannot identify each orbital cycle within our record due to core gaps, the age model of U1356 is definitely robust enough for reconstructing long-term trends in the SST reconstructions. If we would not distinguish between TEX86-derived temperatures from glacial lithologies and those obtained from the interglacial lithologies, the temperature trend would flatten out due to the larger internal temperature variation. In addition, we would like to remind R2 that our TEX86 record is of relatively low resolution, and sample location is guided (in part) by avoiding sediments that are known not to be in situ (i.e., distal mass transport deposits). For this reason, samples are in places predominantly obtained from glacial sediments, while in other places they are predominantly obtained from interglacial sediments. Running a LOESS curve through the entire dataset could therefore potentially establish non-existing trends due to our irregularly-spaced sample distribution. In order to resolve this uneven distribution of samples from the two different lithologies, we apply a LOESS curve on the (independently separated) glacial and interglacial data(sub)sets. We are confident that this better reflects the actual temperature trends, because both the glacial and interglacial LOESS curve show the same long-term trend despite the fact that they are based on separate data(sub)sets. ISince it would be an uneven comparison to compare these to the mean of δ18O, we have chosen to compare glacial SSTs to the above-average δ18O and the interglacial to the below-average δ18O.

Secondly, Referee #2 suggests comparing δ18O of the seawater calculated by using TEX86-based temperatures versus δ18O of the seawater calculated by using Mg/Ca-based temperatures. Upon reviewing our initial approach, and supported by the comments of both R1 and R2, we will no longer quantify δ18Osw changes from our TEX86 record. Since we will discuss matches and mismatches between the trends of the δ18O record and the TEX86-based temperature record more qualitatively in the revised manuscript, resampling of the benthic δ18O record is no longer valid.

ORIGINAL COMMENT:

5) The Site 1218 δ18O of benthic foraminifera is used because it covers the entire record. However, are the trends in δ18O of seawater different when the other high resolution Site 1264 δ18O of benthic foraminifera is used? Any one location can be influenced by changes in hydrography.

REPLY:

The long-term (million-year) trend of Site 1264 is very similar to that of Site 1218 (Liebrand et al. 2017, PNAS) and there is therefore no difference in the reconstructed δ18O of the seawater. In fact, globally all Oligocene δ18O records follow the same trend, except for the δ18O record of Maud Rise (Hauptvogel et al. 2017, Paleoceanography), which we believe is indeed influenced by changes in hydrography.

ORIGINAL COMMENT:

Minor comments: The authors should include changes in paleolatitude and whether that might influence the temperature record. Are there large changes in sedimentation rate that might influence preservation and/or these records?

REPLY:

We shall include the paleolatitudes of Site U1356. Site U1356 shifted from 58.86°S at 30 Ma to 59.43°S at 22 Ma (using van Hinsbergen et al. 2015, PloS One). We believe that this shift to higher latitudes could be at least partly responsible for the increased glacial-interglacial temperature variability in the late Oligocene. We acknowledge that this is not part of the manuscript in its current state and we will include this discussion in the revised manuscript in the part that focusses more on the potential role of Southern Ocean fronts.

Changes in sedimentation rate in general will not affect the temperature reconstruction, as the TEX86 (i.e. the relative abundance of GDGTs) relatively unaffected by oxic degradation (Kim et al., 2009, GCA) and if so, this would result in substantially elevated BIT indices something we do not observe (Huguet et al. 2009, OG). Changes in sedimentation rate at Site U1356 are mainly determined by the deposition of mass transport deposits. These could contain allochthonous GDGTs, which is why samples from this type of deposits were not used for reconstructing the sea surface temperature record.

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