Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-153-RC2, 2018

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 #2

Received and published: 19 February 2018

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 understanding 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.

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.

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.

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 long-term changes in in carbonate ion (Lear et al., 2015, Ford et al., 2016).

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 δ18O) 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.

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.

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?

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