Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-151-RC1, 2018

Climate of the Past
Discussions
Open Access

Interactive comment on "Thermocline state change in the Eastern Equatorial Pacific during the late Pliocene/early Pleistocene intensification of Northern Hemisphere Glaciation" *by* Kim A. Jakob et al.

Anonymous Referee #1

Received and published: 18 January 2018

Review of the manuscript "Thermocline state change in the Eastern Equatorial Pacific during the late Pliocene/early Pleistocene intensification of Northern Hemisphere Glaciation" by Jakob et al. This manuscript uses a combination of foraminiferal geochemistry (Mg/Ca and stable oxygen and carbon isotopes), abundance and sand accumulation rate to reconstruct how the thermocline in the east Pacific cold tongue area developed during the onset of Northern Hemisphere Glaciation (2.4-2.75 Ma). The comparison between foram species living at the surface and in the (sub)-thermocline gives an estimate on how deep the thermocline was and, thus, on the intensity of

Printer-friendly version

Discussion paper

upwelling and primary productivity. The new data in this study are focused on geochemistry of the deep dwelling G. crassaformis and abundance of thermocline species. These show a shift around 2.6 Ma when the intermediate dwellers decrease in abundance and the deep-dwelling crassaformis increases suggesting a switch towards a shallower thermocline afterwards. This may have played a role in the development of larger ice sheets in the Northern Hemisphere. Interestingly, the temperature and d18O records do not show this shift but neither a clear G-IG cyclicity. A longer-term trend seems to be present, although it is unclear what could have caused this.

In general, this manuscript is well-written, concise and easy to read providing new high-resolution data for an area and time interval during which a lot was happening. What I am missing a bit is the interaction with other studies dealing with this theme to come to a clear mechanism what caused what, i.e. a shallow thermocline led to more ice build-up or the other way around? A lot of work has been done already, both specifically for this time interval, but also for the full Pliocene/Pleistocene showing how long-term trends did develop. And although several studies are mentioned throughout the text as "supporting" the new data, I think the manuscript would improve if the current study is projected for example onto the longer trends and/or compared with model experiments. This would also help to determine if an apparent trend in Dd18O and DMg/Ca fits in with the overall trend, i.e. the thermocline shoaling throughout the east Pacific in Steph et al. (2010) does show a lot of variability occurring around 2.5-2.7 Ma. So could it be that the reconstructions here are more temporal variability than a long-term change?

Additionally, the impact of the final stages of closing the Panamanian Gateway could still have been involved, both concerning productivity (Schneider and Schmittner, 2006) and thermocline structure; Groeneveld et al. (2014) also show thermocline vs surface temperatures for MIS 96-100 for east Pacific Site 1241. As Site 1241 is located outside the cold tongue, modern conditions show a strong sea water temperature gradient between both locations, but the long-term thermocline shoaling during the Pliocene occurred both in and outside the cold tongue area. A comparison would therefore also provide additional evidence for a change in intensity of upwelling/cold tongue or if the full east Pacific experienced these changes.

Contamination of the samples seems to be absent concerning Mg/Ca, but Mn/Ca values are relatively high. These values, however, are not uncommon in older sediments (Groeneveld et al., 2006; Schmidt et al. 2006); and for the Galapagos area Lea et al. (2005) linked higher Mn/Ca to volcanic particles. One way to check the character of the Mn is to perform reduction cleaning to see if Mn-oxyhydroxides are involved. Another possibility may be an actual bottom water signal. Mn/Ca is recently receiving increasing attention as a recorder of bottom water oxygen conditions, either in the sediment, as coatings involving MnCO3 being formed onto the tests or in the foram calcite itself. If that is the case you may see glacial-interglacial variability in the Mn/Ca and it may be linked to d13C as variations in productivity would change the intensity of the oxygen minimum zone.

You distinguished between dextral and sinistral forms of crassaformis where possible. Based on a previous study there was no difference in the geochemical structure, but is the occurrence of both forms controlled by glacial-interglacial variability? Also, did you notice differences in the signature of more heavily encrusted specimens vs less-encrusted specimens or even between different morphological types which occur during this period (Rögl 1974)? Seasonality of ruber: Sediment trap studies often show a distinct seasonality in ruber fluxes when areas are affected by seasonal upwelling conditions. So this may mean that also in the cold tongue ruber is more inclined towards the season when upwelling decreases in intensity (Mohtadi et al., 2009; Jonkers and Kucera, 2015).

Add bars in the figures to better be able to distinguish between glacial and interglacial time periods.

Page 8, line 23: although in the case here with ruber and crassaformis living in different water masses, your d18O may also indicate a difference in salinity.

In conclusion, I think that this manuscript is fitting very well with Climate of the Past, but the discussion could use more attention by including and comparing with existence literature. Therefore, I recommend moderate revisions.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-151, 2017.