

Thermocline state change in the Eastern Equatorial Pacific during the late Pliocene/early Pleistocene intensification of Northern Hemisphere Glaciation

- Response to Reviewer #1 -

We thank Reviewer #1 for his/her careful and thorough assessment of our manuscript. Below, we provide a point-by-point response to all comments and suggestions made by Reviewer #1.

Response to general comments

R.1.1: “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 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 *G. 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 $\delta^{18}\text{O}$ records do not show this shift but neither a clear glacial-interglacial 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?”

We thank the reviewer for this positive assessment. Substantial shoaling of the thermocline as documented by our data from ~2.64 to 2.55 Ma suggests that major changes in thermocline depth occurred shortly before the final phase of the late Pliocene/early Pleistocene intensification of Northern Hemisphere Glaciation (iNHG) and that (sub-)tropical thermocline shoaling,

perhaps, was a precondition to allow the development of large ice sheets in the Northern Hemisphere. Our observation is in line with studies from Fedorov et al. (2006) and Dekens et al. (2007), but contradicts the results of Wara et al. (2005), Steph et al. (2010) and Ford et al. (2012). The latter have documented most prominent changes in thermocline depth only prior to ~3.5 Ma (see Fig. S2.1 in our Response Letter to Reviewer #2).

A brief discussion on what caused what, i.e., whether thermocline shoaling favoured Northern Hemisphere ice build-up or *vice versa*, as asked by the reviewer, has already been included in the original version of the manuscript (Section 5.4.2, p. 12, lines 6–10). To account for the reviewer’s comment, however, we will elaborate this discussion in more detail, also in comparison with other studies as also suggested by Reviewer #2 (for details see our response to comment R.2.2 by Reviewer #2).

R.1.2: “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 $\Delta\delta^{18}\text{O}$ and $\Delta\text{Mg}/\text{Ca}$ 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?”

We highly appreciate this suggestion. Our new records ($\delta^{18}\text{O}$ and Mg/Ca gradients) from Site 849 indicate a general shoaling of the thermocline across the entire target interval (~2.75–2.4 Ma), and therefore can be termed a “long-term” shoaling trend, although the most prominent thermocline shoaling is restricted to the ~2.64–2.55 Ma period (Fig. 6b). The overall shoaling trend observed at Site 849 across the Plio-Pleistocene transition matches results from previous proxy records and modelling efforts (Wara et al., 2005; Fedorov et al., 2006; Dekens et al., 2007; Steph et al., 2010; Ford et al., 2012) that consistently document a “long-term” shoaling of the thermocline in the Eastern Equatorial Pacific (EEP) and other (sub-)tropical upwelling regions throughout the Plio-Pleistocene.

Upon closer inspection, however, the geochemical (Mg/Ca and $\delta^{18}\text{O}$) records of the thermocline-dwelling species *G. tumida* from Site 1241 in the East Pacific Warm Pool reveal a short-term, i.e., “transient” thermocline deepening from ~2.7 to 2.5 Ma that is superimposed

on the overall “long-term” shoaling trend observed in this study for ~6–2 Ma (Steph et al., 2010; see Fig. S2.1 in our Response Letter to Reviewer #2)).

“Transient” thermocline deepening from ~2.7 to 2.5 Ma as shown for Site 1241 (Steph et al., 2010) contradicts our proxy records for thermocline depth at Site 849 that rather indicate a “long-term” thermocline shoaling trend for this time interval; unfortunately, other records used to infer thermocline evolution in the tropical east Pacific for this interval lack the required temporal resolution (i.e., ~12.5 kyr [Site 847; Wara et al., 2005], ~10–20 kyr [multiple east Pacific sites; Dekens et al., 2007], and ~30 kyr [multiple EEP sites; Ford et al., 2012] *versus* ~750 yr [Site 849; this study] to ~3 kyr [Site 1241; Steph et al., 2010]) to resolve the “transient” thermocline state change during the ~2.7–2.5 Ma period as observed for Site 1241, if existent. Nevertheless, we hypothesize that records from Site 1241 reflect a more local signal than Site 849 and other sites located within the equatorial “cold tongue”. Moreover, Site 1241 was possibly temporally affected by glacioeustatically induced openings and closures of the Central American Seaway around ~2.5 Ma (Marine Isotope Stages [MIS] 100–96): High-resolution (~1 kyr) data (surface-to-thermocline, i.e., *G. sacculifer*-to-*N. dutertrei*, Mg/Ca-based temperature gradient) from this site reveal changes in thermocline depth on glacial-interglacial timescales for MIS 100–96 (Groeneveld et al., 2014), while our records from Site 849 indicate no glacial-interglacial variability in thermocline depth for the same time interval (for details see our response to comment R.1.3 and Fig. S2.1 in our Response Letter to Reviewer #2).

To account for the reviewer’s comment, a more detailed comparison of our new records on thermocline evolution at Site 849 to other thermocline data in the tropical east Pacific will be included in Sections 5.3 (“Stable-isotope and Mg/Ca records of *G. crassaformis* and *G. ruber* at Site 849”) and 5.4.1 (“Geochemical evidence”), focussing on both the “long-term” trend and also on the “transient” or glacial-interglacial variability. In this context, we also plan to modify Figure 1 by showing (in addition to the two maps already presented) a global map indicating the location of sites that will be mentioned in the text. This will further enhance clarity for the readers.

R.1.3: “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.”

We thank the reviewer for this comment. The records of Groeneveld et al. (2014) suggest that water-column stratification (defined as the temperature difference between the surface-dwelling species *G. sacculifer* and the thermocline-dwelling species *N. dutertrei*) at east Pacific Site 1241 was higher during interglacials (temperature difference [ΔT] of $\sim 3\text{--}4$ °C) than during glacials (ΔT of $\sim 1\text{--}2$ °C) for MIS 100–96, thereby implying thermocline-state changes on glacial-interglacial timescales (see Fig. S2.1 in our Response Letter to Reviewer #2). This contradicts our new data from Site 849 where both surface-to-thermocline $\delta^{18}\text{O}$ and temperature gradients indicate no change in thermocline depth on glacial-interglacial timescales for MIS G6–96 (Fig. 6b). Together, data from Sites 849 and 1241 suggest that thermocline-state changes have not occurred homogeneously across the entire tropical east Pacific, possibly due to the following reasons:

- (i) Sites 849 and 1241 are located in different areas of the highly heterogeneous tropical east Pacific (i.e., cold tongue upwelling region [Site 849] *versus* warm pool [Site 1241]). Therefore it appears reasonable to hypothesize that changes in thermocline depth in- and outside the cold tongue upwelling regime simply underwent a different evolution on glacial-interglacial timescales. This hypothesis is supported by faunal data (calcareous nanofossil counts) from EEP cold tongue Site 846, which, like Site 849, do not indicate any variations in thermocline depth for MIS 101–95 (Bolton et al., 2010).
- (ii) It is likely that Site 1241 reflects a more local signal due to influences by glacioeustatically induced openings and closures of the Central American Seaway during MIS 100–96 (as suggested by Groeneveld et al. [2014]), while Site 849 records a more open-ocean, quasi-global signal due to its position west of the East Pacific Rise (Mix et al., 1995). This is, for example, highlighted by the fact that global climate-ocean ecosystem model experiments (Schneider and Schmittner, 2006) indicate that a closure of the Central American Seaway helps to promote upwelling in nearby EEP regions (including Site 1241) – in contrast, Site 849 appears to be mostly unaffected by such changes as shown by sand-accumulation-rate-based primary productivity data (Jakob et al., 2016). However, it is likely, although speculative, that temporal closures of the Central American Seaway from MIS 100 through 96 (Groeneveld et al., 2014) might have contributed to generally increasing upwelling and thus primary productivity rates that have been observed at Site 849 since MIS 100 (Jakob et al., 2016; this study; Fig. 6c–d).

To account for this comment, we will include a discussion into Sections 5.4.1 (“Geochemical evidence”) and 5.4.2 (“Faunal and sedimentological evidence”) on (i) thermocline data derived from Sites 849 and 1241 (also in comparison to other datasets from the east Pacific; for details see our response to comment R.1.2 by Reviewer #1 and to comments R.2.2 and R.2.3 by Reviewer #2; see also Fig. S2.1 in our Response Letter to Reviewer #2), and on (ii) the impact of glacioeustatically induced openings and closures of the Central American Seaway to these sites.

R.1.4: “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 MnCO_3 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 $\delta^{13}\text{C}$ as variations in productivity would change the intensity of the oxygen minimum zone.”

We appreciate this comment. As highlighted in our manuscript, it is important to note that if Mn-rich overgrowths existed on *G. crassaformis* tests, they might change absolute temperature estimates by on average ~ 0.5 °C, but this would not affect the overall shape of the Mg/Ca-based temperature record (Fig. 4c, d). Therefore it appears reasonable to assume that early diagenetic Mn-rich overprinting has no significant impact on our interpretation regarding relative changes in the surface-to-thermocline gradient. Hence, we argue that for the purpose of our study the reductive cleaning step, which might remove such overgrowths, is not required. However, to account for the reviewer’s comment, we have re-checked the character of Mn in/on *G. crassaformis* tests at our study site as follows:

(i) The reviewer suggests that Mn-oxyhydroxides might perhaps explain enhanced Mn/Ca ratios (~ 0.2 – 2.1 mmol/mol; Fig. 4) in *G. crassaformis* tests at our study site. We have tested this possibility through a modification of the cleaning procedure for element analyses via including an additional reductive cleaning step for selected samples (e.g., Barker et al., 2003). As a reductive reagent, a mixture of hydrazine, ammonium hydroxide and ammonium citrate was used. The results show by ~ 40 – 45 % lower Mn/Ca values for samples that were cleaned reductively compared to samples that underwent only oxidative cleaning

(Fig. S1.1). This indicates that at least two different Mn phases coexist on *G. crassaformis* test surfaces – perhaps Mn-oxyhydroxides that can be removed by reductive cleaning and another phase that can neither be removed by oxidative nor by reductive cleaning. To comply with the reviewer’s comment, we will include this information into Section 5.2 (“Assessment of contamination and diagenetic effects on Mg/Ca ratios of *G. crassaformis*”).

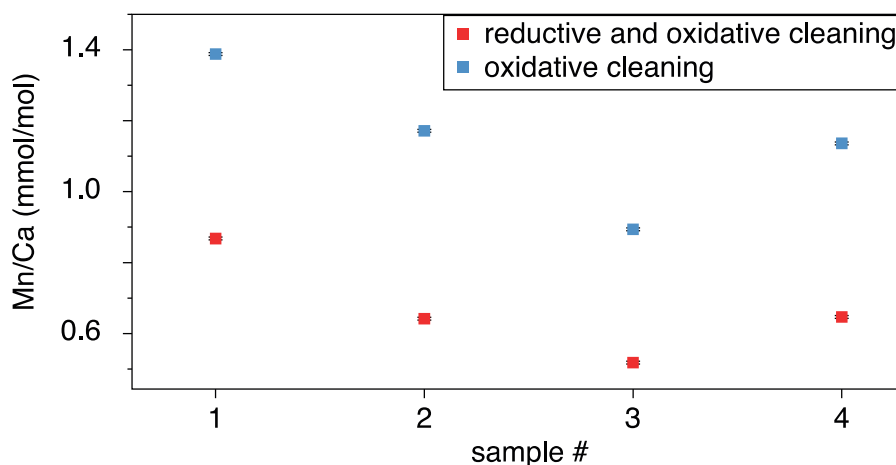


Figure S1.1: Evaluation of potential contaminations on *G. crassaformis* test surfaces from Site 849. Mn/Ca ratios of selected samples (1: 849D-7/3-77–79cm, 2: 849D-7/3-105–107cm, 3: 849D-7/4-51–53cm, 4: 849D-7/4-67–69cm) that underwent reductive and oxidative cleaning (red) or oxidative cleaning only (blue).

(ii) The reviewer is correct in stating that if foraminiferal Mn/Ca ratios trace bottom-water oxygen conditions (e.g., McKay et al., 2015; Koho et al., 2017), our Mn/Ca record should follow the glacial-interglacial cyclicity given by the productivity proxy record from the same site and time interval (Fig. 6d; Jakob et al., 2016). To identify coherencies between these two records (Mn/Ca values *versus* sand-accumulation rates; note that we use the latter record as a primary productivity proxy instead of commonly used foraminiferal $\delta^{13}\text{C}$ proxy data since $\delta^{13}\text{C}$ at Site 849 has been shown to not simply trace *in-situ* changes in productivity, but rather reflects the $\delta^{13}\text{C}$ signature imprinted on high-southern-latitude waters that are transported to the EEP [Jakob et al., 2016]) we performed Blackman-Tukey cross-spectral analyses with a 30 % overlap using the AnalySeries software package version 2.0.8 (Paillard et al., 1996). Data for cross-spectral analyses were linearly interpolated, detrended, and prewhitened.

Our results indicate that indeed both records yield a 41-kyr (i.e., glacial-interglacial) cyclicity (Fig. S1.2). However, the Mn/Ca and the productivity proxy records do not fluctuate in phase; instead, they are shifted by approximately -102° (equal to -11.5 kyr) for the 41 kyr period. If Mn/Ca reflects bottom-water oxygen concentrations, the oxygen

(Mn/Ca) and productivity (sand-accumulation rate) proxy records are, however, expected to fluctuate in phase. Therefore we exclude that *G. crassaformis* Mn/Ca data at Site 849 record bottom-water oxygenation. We decided not to include this discussion into the manuscript as it goes far beyond the scope of our study, and we hope this finds approval of the editor.

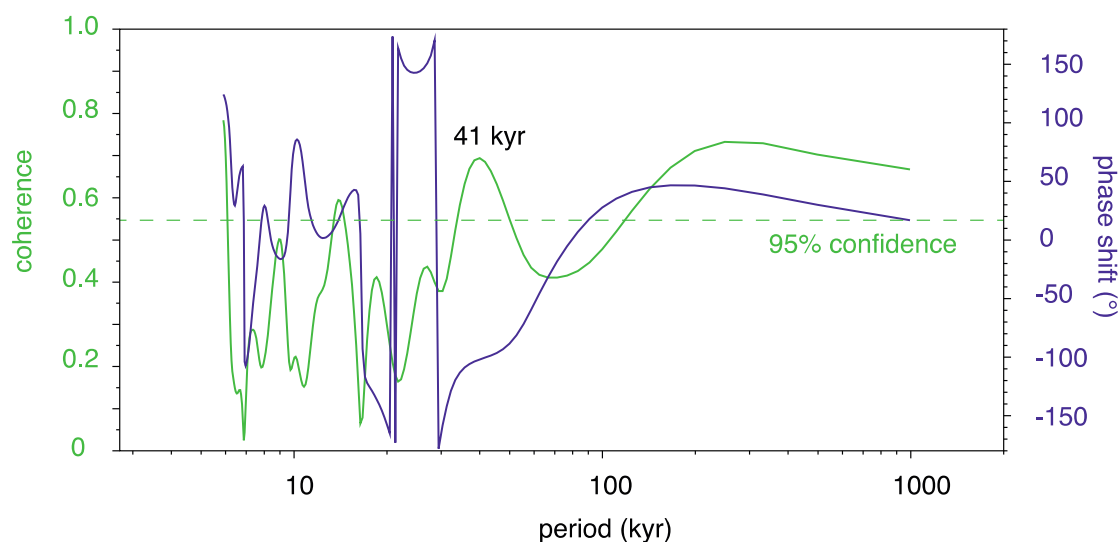


Figure S1.2. Blackman-Tukey cross-spectral analysis for the identification of phase shifts at Site 849 for the time interval from ~2.65 to 2.4 Ma. Coherence (green) and phase (purple) relationship between productivity (sand-accumulation rates [Jakob et al., 2016]) and Mn/Ca ratios are plotted on log scales. Negative values in this phase plot indicate that productivity lags Mn/Ca, i.e., by -102° (equal to -11.5 kyr) for the 41 kyr period.

(iii) Reviewer #1 suggests that enhanced Mn/Ca values in our samples could also be linked to volcanic particles (e.g., Lea et al., 2005). However, while carefully reading this study, we recognized that Lea et al. (2005) observed enhanced Fe/Ca and Al/Ca ratios (rather than increased Mn/Ca ratios) in planktic foraminifera from core intervals that were rich in volcanic debris. In contrast, they link higher Mn/Ca values to diagenetic coatings (Mn-carbonates) on foraminiferal tests. Moreover, we are unaware of any study that has shown a relation between foraminiferal Mn/Ca ratios and volcanic material. Therefore we decide to not include such a discussion into our manuscript.

R.1.5: “You distinguished between dextral and sinistral forms of *G. 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 speci-

mens or even between different morphological types which occur during this period (Rögl 1974)?”

We preferentially selected the most abundant coiling direction (sinistral) of *G. crassaformis* tests at Site 849. However, the sinistral coiling forms did not occur continuously over the sampling interval. Thus, we also used the dextral coiling tests for some intervals, (~2.46–2.43 Ma [69.84–69.04 meters composite depth (mcd)], ~2.51–2.50 Ma [70.85–70.68 mcd]) and also for some specific time points (2.40 Ma [68.10 mcd], 2.41 Ma [68.32 mcd]). The occurrence of dextrally and sinistrally coiling *G. crassaformis* specimens, however, appears to be randomly rather than following a certain cyclicity.

Furthermore, we did not notice a change between heavily and less encrusted *G. crassaformis* specimens or even between different morphotypes throughout the investigated time interval while picking. Scanning Electron Microscope (SEM) images of *G. crassaformis* specimens derived from both glacial and interglacial samples also do not reveal such changes (Fig. 3).

To account for the reviewer’s comment and to enhance clarity for the readers, we will modify the relevant sentences in Sections 4.1 (“Sample material”) and 5.1 (“Foraminiferal test preservation at Site 849”) when revising our manuscript.

R.1.6: “Seasonality of *G. ruber*: Sediment trap studies often show a distinct seasonality in *G. ruber* fluxes when areas are affected by seasonal upwelling conditions. So this may mean that also in the cold tongue *G. ruber* is more inclined towards the season when upwelling decreases in intensity (Mohtadi et al., 2009; Jonkers and Kucera, 2015).”

In extratropical regions *G. ruber* most likely represents summer surface-water conditions (e.g., Schiebel and Hemleben, 2000; Schiebel et al., 2002), while in the tropics, where seasonal climate variability is low, plankton tow studies indicate that this species can be found year around and is therefore typically used as recorder for mean annual surface-water conditions (e.g., Deuser et al., 1981; Lin et al., 1997). This is supported by a study on global planktic foraminiferal shell fluxes that show that in waters with temperatures >25 °C (i.e., sea-surface temperatures [SSTs] prevailing at our study site during interglacials; Jakob et al. [2017]) *G. ruber* has a less predictable flux pattern with random peak timing (Jonkers and Kučera, 2015). In colder waters, seasonality appears to be more prominent, with decreasing fluxes when temperature decreases (Jonkers and Kučera, 2015). The reviewer is therefore correct in stating that *G. ruber* proxy data (particularly of glacial periods) might be biased towards the season with lower upwelling intensity. In contrast, however, Mohtadi et al. (2009)

show that *G. ruber* fluxes in the upwelling region off south Java are not solely related to upwelling: Although this species consistently shows enhanced flux rates during the upwelling period, its flux rates are also temporarily as high during non-upwelling seasons.

In light of the above considerations, we find it reasonable to assume that *G. ruber* is a more likely recorder for mean surface-water conditions than for a particular season in the low-latitude EEP upwelling regime. However, to account for the reviewer's comment, relevant references will be added to Section 5.3 ("Stable-isotope and Mg/Ca records of *G. crassaformis* and *G. ruber* at Site 849"), and more detailed information on seasonality at Site 849 will be provided (for details see our response to comment R.2.14 by Reviewer #2).

R.1.7: "Add bars in the figures to better be able to distinguish between glacial and interglacial time periods."

Bars as suggested by the reviewer had already been included in the figures of the original manuscript version. However, we will act on this suggestion by highlighting the bars by a more prominent colour.

Response to line by line remarks:

R.1.8: "Page 8, line 23: although in the case here with *G. ruber* and *G. crassaformis* living in different water masses, your $\delta^{18}\text{O}$ may also indicate a difference in salinity."

We fully agree with the reviewer and will revise this paragraph.

References (other than those already cited in the discussion paper)

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