

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 #2

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The paper by Jakob et al focusses on a new high resolution paleoceanographic record of the onset of the large Northern Hemisphere Glaciations at the Plio-Pleistocene transition 2.6 Myrs ago. The authors have documented changes in the surface hydrography at site ODP849, in the Eastern Equatorial Pacific, based on coupled $\delta^{18}\text{O}$ and Mg/Ca in *G. ruber*, record mostly published in previous papers by the same group, and compare this record with a new *G. crassaformis* $\delta^{18}\text{O}/\text{Mg}/\text{Ca}$ record interpreted as a deep thermocline species. Those geochemical datasets are augmented with a record of sediment fluxes and with some countings of *G. crassaformis* and *G. menardii/G. tu-*

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mida. Using the difference in the temperature records and $\delta^{18}\text{O}$, the authors describe what they think changes in the Eastern equatorial Pacific thermocline, with a thermocline shoaling until 2.55 Myr followed by a stable thermocline. The article is a welcome addition as it does document in the EEP a Mg/Ca record for a deep dwelling species.

The manuscript is well written and the figures are also generally well crafted. As this is the third manuscript on the same record, the paper also details what are the novelties compared to the previous records. I do feel that the technical issues are well thought-out, e.g. the potential impact of Mn crusts on the Mg composition of the foraminiferal calcite is ruled out with some backed up arguments.(but missing the Pena et al, 2005 study which worked in the EEP to estimate the impact of these crusts on the Mg/Ca of foraminifera). On the choice of the calibration used, the authors are also quite careful, and do pick the Cléroux et al calibration quite sensibly.

My main comment on the manuscript, is that it does miss a real discussion. Symptomatically, the authors did not compare their records to any other records either from the same region or from more remote sites, which would have lent some weight to their hypothesis. Their *G. crassaformis* record is interesting and should be more carefully addressed. I will detail a series of questions that should be addressed, from my point of view through some significant revisions :

(1) The Mg/Ca values measured in *G. crassaformis* are quite low, and give some very low temperature range, mostly between 1 to 6°C (regardless of the calibration used is the one by Cléroux or the one by Regenberg). Those temperatures appear to be even colder than modern temperature at the sites, and it is unlikely that the LGM temperatures were much colder than 1°C. I am thus puzzled by those extremely low temperatures, though one might argue that they are close to the *Tcrassa* inferred at site DSDP214. Moreover the temperatures at the site 849 are much colder than surface subantarctic waters during the same time interval (site 1090). I would like to have some sense of the process by which the water masses where *crassaformis* do live would be much colder in the equatorial Pacific than in subantarctic waters.

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(2) The location of the site ODP849 is at the edge of the cold tongue. Deglacial studies have shown that this cold tongue did migrate both longitudinally, but also latitudinally (e.g. Koutavas et al. 2003). I wonder if one might not interpret the subtle changes in the record as a long term shift the EEP rather than a subsurface process.

(3) I am puzzled by the number of *G. crassaformis* found in the record, reaching at sometimes close to 30% of the $>250\mu\text{m}$. Though the comparison with modern and LGM census of planktonic foraminifera is not straightforward, as late Pleistocene counts are based on the $>150\mu\text{m}$ fraction, I am surprised that coe tops data show extremely low percentages of *G. crassaformis* (typically below 1%, exceptionally reaching 5%), far less with results from this study. I understand that the authors do have some arguments that the dissolution is limited at this site (fragmentation index for example), yet I cannot find an alternate process that would selectively get rid of most of the surface to subsurface species.

(4) The ΔT record does not show any glacial/interglacial dynamics. This is quite surprising as there are a large number of studies (modelling and observational) that have shown some changes in the thermocline depth during the most recent glacial/interglacial transitions. I wonder then if the choice of picking a quite deep species (see below) and a shallow species such as *G. ruber* does really reflect changes in the thermocline. Species such as *G. tumida*, *G. menardii*, or *N. dutertrei* living closer to the thermocline would have been more sensitive to changes. I would therefore be grateful if the authors could add some lines on how they can groundcheck their proxy of the thermocline ?

(5) The living depth of *G. crassaformis* in this study is supposed to be within the 500 to 1000 m range. To set the record straight, the authors have to be clear that they think that the "calcification range" of *G. crassaformis* is within this range. All the studies quoted by the paper to posit this range come from surface sediment samples, in which the authors have made the assumption that the isotopic temperature reflects the calcification depth. This is different from the actual mean living depth. As a couple

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of examples, the paper by Jones (1967) in the equatorial Atlantic did find most *G. crassaformis* at depths ranging 200 to 300 meters, not below 500 meters. The authors also quote Wejnert et al 2013 indicating a calcification depth below 500 meters. This is not what the paper states, as they indicate that the range is above 300 meters. Please correct accordingly.

Details :

note [page 2]: One might also consider the last major tipping climate history : the Holocene to Anthropocene transition or the last deglaciation. Please reword more carefully.

note [page 2]: I would tend to think that it is not the shallow depth of the thermocline that exerts a role in the ENSO, but rather the reverse. So please reword in thinking the Eastern Pacific Ocean as a part of the ocean where atmospheres and surface oceanic layers are subtly interconnected.

note [page 2]: I understand the framing in two alternates hypotheses, but there is also a mid-ground solution where the state of the equatorial Pacific did play a substantial role, without being the main climatic ruler. Moreover, if one would really test the role of the EEP, he would have to reconstruct the dynamics of the equator to pole gradient.

note [page 2]: "We use planktic (both sea-surface- and thermocline-dwelling) foraminiferal geochemical ($\delta^{18}\text{O}$, $\delta^{13}\text{C}$ and Mg/Ca) proxy records in combination with sedimentological (sand-accumulation rates) and faunal (abundance data of thermocline-dwelling foraminiferal species) information to reconstruct thermocline depth for the final phase of the late Pliocene/early Pleistocene iNHG from ~ 2.75 to 2.4 Ma (MIS G7-95)" : This final sentence of the introduction, which sums up the methods should be either moved in the methods, or argumented.

note [page 6]: The use of this very large size fraction is not regularly used. Could you elaborate on this choice?

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note [page 7]: A low fragmentation index might also correspond to the selective preservation of only resistant species. Please rephrase this sentence.

note [page 7]: Please be more specific: what is the seasonality at the location of the site? - even though it might be significantly different, it cannot be ruled out without testing it

note [page 8]: What is the mean Temperature at the site?

note [table1 page 17]: Add the number of samples processed for each site and study to give a sense of the effort included in this study.

note [Figure 1 page 18, panel B]: A latitudinal transect would be more useful to test whether the front did change as in Koutavas et al.

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