

Clim. Past Discuss., referee comment RC1
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Comment on cp-2021-151

Clara Bolton (Referee)

Referee comment on "Plio-Pleistocene Perth Basin water temperatures and Leeuwin Current dynamics (Indian Ocean) derived from oxygen and clumped-isotope paleothermometry" by David De Vleeschouwer et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-151-RC1>, 2021

This paper by David De Vleeschouwer et al. presents new stable oxygen isotope data and clumped isotope data generated on mixed-layer dwelling foraminifera, and discusses the Pliocene to early Pleistocene evolution of the Leeuwin Current offshore northwestern Australia. This period is an important one in terms of northern hemisphere glacial intensification and the evolution of Indonesian Throughflow, and the study site is well placed to track potential changes in the latter (related to sea level and tectonics). The paper is well written and contains interesting, high-quality new data. New foraminiferal clumped isotope data will certainly contribute to the growing body of data that will allow us to better constrain absolute ocean temperatures in the pre-Pleistocene, and also provide important information on depth habitats of particular species. I find it interesting that there is no clear unidirectional trend in isotopic gradient over the intensification of northern hemisphere glaciation interval, and LC current intensity on secular timescales thus seems to be almost independent of this (although LC intensity is apparently strongly linked to sea level changes at the G-IG scale) – this aspect is not discussed much. I think the paper is suitable for consideration in *Climate of the Past* after minor revisions, see comments below.

Comments on temperature reconstructions

Figure 4a: perhaps add modern SST and T at proposed calcification depth of *T. sacculifer* (base of mixed layer) onto the Figure for comparison.

Fig. 5: Calcification temperature reconstruction based on $\delta^{18}\text{O}_{\text{planktic}}$ and Rohling Sea Level curve: I had a hard time assessing the robustness of this. Is the calculation of T in

this way therefore based on the assumption that local $d_{18}O_{sw}$ (i.e. the part of $d_{18}O_{sw}$ related to local hydrological cycle effects and not global sea level) was constant? Is this assumption reasonable both on the long-term and on G-IG timescales (in the context of salinity/water mass changes associated with L current and upwelling changes in your study area) at your site? I think some discussion of expected local $d_{18}O_{sw}$ changes on G-IG timescales, and how this would impact the temperature reconstruction, would be nice.

In Figures 5 and 6, different sea-level reconstructions appear to be shown or used in calculations – Rohling 2014 in Fig. 5 and Rohling 2021 in Fig. 6. Is the more up-to-date sea-level record not also suitable for calculating the glacial contribution to $d_{18}O_{sw}$ in Fig. 5?

It would be interesting to consider the G-IG amplitude of the *G. sacculifer* $d_{18}O$ signal in the context of contemporaneous and/or regional records from surface-dwelling species such as *G. ruber*, to see if this can give you any support for the proposed deep mixed layer depth habitat or relatively constant $d_{18}O_{sw}$ conditions. Also, in the methods, it is not mentioned whether individuals with gametogenic calcite final chambers were avoided or included during picking.

It may also be relevant to consider and compare the new paper by Meinicke et al. (2021, already cited) that also contains D47 measurements on *Trilobatus trilobus* and a deeper-dwelling species for the Plio-Pleistocene from the Western Pacific Warm Pool - I think their interpretations re: depth habitat for *T. trilobus* are not the same as in this paper, are the two interpretations compatible?

Line 349: This is interesting, I wonder is there any evidence in support of the hypothesis that Thaumarchaeota thrive/are more successful when phytoplankton biomass is low under oligotrophic conditions?

Line 361: Perhaps add that diagenesis (seafloor recrystallisation?) of foraminifera would specifically lead to a cool bias on D47 temperatures.

Line 362: please briefly define “isotopic scrambling” for non-clumped specialists

Line 498: Not clear how exactly low-resolution clumped isotope and TEX86 temperatures “demonstrate that the LC continued to operate [...]” – do you deduce this from absolute temperatures? Or latitudinal gradients?

Minor comments

The last sentence of the abstract is not clear to me. “The common ITF forcing explains the observed coherence of Southern Hemisphere ocean and climate records.” – do you mean

ocean vs atmospheric records in the Southern Ocean? or ITF/low-latitude vs higher southern latitude records?

line 65 – north

line 67 – do you mean between Indo-Pacific paleoceanography and paleogeographic/gateway changes?

line 76 – “Pliocene aridity is punctuated” sounds strange to me. Do you mean that aridity is intensified/magnified during glacials?

line 85 – might be useful to add Site U1461 to the map? Also names mentioned in the text such as Carnarvon/Perth Basins

Figure 1 caption: (a) and (b) January vs June don't seem to match up with the text in the figure panels.

Fig. 1c: the depth/time plot y-axis extends to >500m water depth, yet the site U1459 is according to the methods at 192m water depth. Is the profile from further offshore? Is this because of the paleodepth? Maybe add details to the caption.

line 195 – misplaced comma?

“Composite forcing” record: did you assume equal weighting of the two forcings? (maybe I missed this)

Regional increases in PP – how can we tell if they are Leeuwin Current eddy-mixing driven, or “weak Leeuwin Current” SAMW upwelling driven?