

Comment on cp-2021-68

Anonymous Referee #1

Referee comment on "Pliocene evolution of the tropical Atlantic thermocline depth" by
Carolien M. H. van der Weijst et al., Clim. Past Discuss.,
<https://doi.org/10.5194/cp-2021-68-RC1>, 2021

The new version of the manuscript by van der Weijst et al. is definitely improved in that additional data have been generated and included, Mg/Ca and stable oxygen and carbon isotopes, into the study. This makes the picture of a deepening of the eastern tropical Atlantic thermocline (TAT) during the warmest part of the Pliocene very convincing. Interestingly, the thermocline shoals again with the intensification of Northern Hemisphere Glaciation. My main point of review on the previous version was that the discussion was not very well developed. And though this has significantly improved I feel this can still be improved. The three possible options that are presented to explain why the TAT shows the same behaviour as in the Caribbean are related to closing of the Panamanian Gateway, temperature changes in the source areas of the thermocline waters and changes in cyclone activity. But the discussion stops with mentioning that neither of these fits very well. I think, however, that the data are convincing enough to make a choice on which explanation the data point to, i.e. related to the closing of Panama and the formation of warm pool-like conditions in the western Atlantic that may have well had their impact as far as the eastern Atlantic. You show in figure 1 the thermocline tilt from the Caribbean to the eastern Atlantic. It would seem very likely to me that a big change like the closing of Panama occurs, that this affects the whole tropical Atlantic.

A second point that I still find not very well developed is the global comparison with other sites. To identify common trends in different basins is a good idea to place the records in a global perspective. But then include some of the compilations that are present, also for the Atlantic like Karas et al. (2017), Bell et al. (2015) or De Schepper et al. (2013, 2014). The location of Site 959 is a great addition to these paper as it indeed shows that it is filling in a blank spot on Pliocene data.

I'm still a bit confused on why you chose to use Site 1000 for the comparison with Site 959 rather than Site 999. The main reason is that the later part of Site 1000, due to its much shallower waterdepth, is heavily affected by diagenesis (Groeneveld et al., 2006). Especially the Mg/Ca-related temperatures but probably also the isotopes are strongly

biased by inorganic precipitates. Site 999 on the other hand comes from a greater waterdepth where diagenesis is not issue, and continuous high-resolution Mg/Ca and d₁₈O records are available.

Regarding the number of specimens used for isotopes and Mg/Ca. Were these coming from the same pool of 60 specimens and separated after crushing for either isotopes or Mg/Ca? Or were these separate batches?

Lines 132-135: According to Dekens an Atlantic correction is not necessary until 2.8 km waterdepth.

Line 170-172: I agree that propagated errors are getting pretty large, but which alternative do we have? It's the main reason absolute salinities are usually not calculated but we rather stick with relative changes.

It would be helpful in the figures to indicate the present-day characteristics, e.g. what is the present salinity difference between surface and thermocline?

Supplement: Put the species names in italics and the isotope numbers in superscript.

In conclusion, I think the manuscript still needs more discussion but the addition of new data has improved the story a lot. Along with a clear structure and easy reading I recommend moderate revisions to make this a good contribution to Climate of the Past.