RC1 Referee comments

Review van der Weijst et al ' Pliocene evolution of the tropical Atlantic thermocline depth. This is a revised manuscript submitted to Clim. Past. Discuss in 2020 which I also reviewed. For this revised manuscript the authors carried out further planktonic foraminifera Mg/Ca measurements and also show the carbon isotopes (new Figures 3 and 4). Thermocline trends inferred from DMg/Ca, Dδ18O and Dδ13C more or less go in a similar direction between 2.8 and 3.5 Ma at Site 959, with a deepening in the later part (slight offset between oxygen isotopes and carbon and Mg/Ca).

Overall I am happy with the changes implemented by the authors and would like to see the manuscript published.

However, there are a couple of issues that I would like to authors to reflect on first:

Having studied Figure 6, I am not convinced with the following statement (Lines 22-23): 'The tropical thermocline depth evolution of the tropical Atlantic differs from the Pacific, which is characterized by gradual basin-wide shoaling across the Pliocene'. If you compare trends between the Pacific and Atlantic in Figure 6, they seem to more or less the same between ~ 4.2 and 2.8 Ma, with a shoaling between 4.2 and ~3.4 Ma, followed by a deepening until 2.8 Ma. The big contrasting thermocline changes occur earlier between ~4.2 and 5.2 Ma, where that of the Atlantic is deepening and that of the Pacific shoaling. I encourage the authors to make this clear in a further revised manuscript.

Author reply:

We rephrased the header of section 4.2 (line 205 in revised MS) and carefully scanned passages of the MS that discuss the difference between the Atlantic and Pacific thermocline evolution. In combination with the text in the abstract and the figures, it should now be clear that the Pliocene thermocline evolution in the Atlantic was considerably different than in the Pacific, but that the trends were not consistently opposite (in anti-phase).

Lines 181 to 191. I think that in this part of the discussion the authors should reference the work the work by LeGrande and Schmidt (2006, GRL), where slopes and intercepts for the various regions have been quantified. If using the basin-specific equations create any differences, please discuss this in your further revised manuscript.

Author reply:

We cite the slope of LeGrande and Schmidt (2006) in line 186. This slope falls within the range that we considered in Figure 5 (0.11-0.22‰/salinity unit).

Please provide details about the LOESS smoothing.

Author reply:

We specified that the data were LOESS smoothed in PAST (Hammer et al., 2001) in the captions of figures 4-6.

Figures in general:

Consider making your figures (especially 4 to 7) more compatible for colour-blind individuals.

Author reply:

We selected more neutral colours for the vertical bands.

Figures detailed:

Figure 4: why are the axes and labels coloured in a and b? Your colour scheme only fits with c!

Author reply:

The axes in 4a and b were changed to black.

Figure 5: why are the axes and labels coloured in a and b? The colour scheme only fits with c. Is this figure actually needed? A lot of data is duplicated from Figure 4.

Author reply:

The axes in 5a and b were changed to black, consistent with Figure 4. We prefer to retain both figures to separate results (Figure 4) from discussion (Figure 5) and avoid an overwhelming figure where all records are combined.

Figure 6: Data from ODP Site 959 and 1000 have considerable gaps. Can you stipple this in the smoothed records to reflect this? Specifically at site 959 between 3.5 and 4.4 Ma there isn't that much data.

Author reply:

We stippled the lines as suggested.

RC2 Referee comments

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.

Author reply:

Because it is clear that much more work is needed to confidently assign a mechanism to the documented tropical thermocline changes in the Atlantic, we would like to refrain from expressing stronger conclusions. We instead expand on potential approaches for future CAS-related research in lines 250-254.

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.

Author reply:

We carefully reviewed the data presented in these papers and found two southern hemisphere SST records in Karas et al. (2017) that are helpful in the comparison with the thermocline records, these were added to Figure 7.

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

Author reply:

The communities' understanding of Mg/Ca calibration and correction is continuously evolving, and the correction factor used in this study could someday be deemed inappropriate. Therefore, we provide raw Mg/Ca values in the supplement so that the records can be recalibrated according to new insights in the future.

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?

Author reply:

We explain in lines 171-176 why we use a semi-quantitative method to calculate relative changes in the vertical salinity gradient. For detailed information on the modern situation, the reader is referred to Figure 1 (line 187).

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

Author reply:

Changes were made.

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.