Hauge Braaten et al. present a combination of thermometers (Mg/Ca and D47) applied to benthic foraminifera from 2 sites (in the Pacific Ocean and in the Atlantic Ocean) to reconstruct the bottom water interaction between the 2 oceans during the mid Piacenzian. This article presents a significant amount of measurements and high quality conclusions. It deserves to be published. I have a few suggestions, mostly on salinity reconstructions and minor comments.

Regarding the salinity reconstruction, I would suggest being more critical of your methodology. The relationship between d18Osw and salinity changes over time. Assuming that the relationship during the mid Piacenzian is similar to today's is probably wrong. Although the authors mentioned it, they assume that their reconstructions of salinity is correct and I suggest being more careful in interpreting your data. Additionally, the authors should add a discussion of the uncertainties in thier estimated salinity. Is a change of 2 PSU, using this methodology, really significant? (Line 377).

We recognize the inherent uncertainties in calculating salinity from d18O for the Pliocene given the likely changes in d18O-salinity relationships over time as well as uncertain mean ocean d18O. Our calculations were therefore meant more as a sanity check rather than reliable estimates of salinity. Given the reviewers comments, however, we suggest reducing the perceived emphasis on absolute values for salinity by omitting these calculations altogether, and simply stating that our d18Osw data suggests a large salinity gradient between the two basins.

Same question for a salinity difference of 0.2 between the Atlantic Ocean and the Pacific Ocean (Line 391).

This value refers to the observed salinity gradient between Pacific and Atlantic oceans today.

Finally, did the authors take into account the global changes during this period (based on sea level change for example) in order to obtain the local salinity signal?

See our reply above. This part of the discussion will be shortened and clarified.

The preservation and contamination tests presented in supplementary material are not mentioned in the text. I found it confusing to only read that the foraminifera are well preserved without any justification, until I read the supplementary material. I would suggest, at least, mentioning the supplementary material.

We had included references to the supplementary information about the contamination tests (line 193-194) and SEM images (line 144 and 215), but we will adjust the main text to more explicitly state where information about the preservation state can be found.

Lines 209, 254 and 255: do the authors have uncertainties at 1 or 2 sigma?

We will update the text to specify that the values refer to uncertainties at the 2SD level and will add propagated uncertainties on the Mg/Ca temperatures to Figs. 2, 3 and 5.

Line 281: I would mention that the author are still talking about Mg/Ca derived temperature in this paragraph.

We will clarify that this line refers to the Mg/Ca-derived temperature record.

Line 295: MIS not MI2

Thank you for noticing this error. We will fix this.

Figure 2 and 3: It would be great if the authors could also add the uncertainties on the Mg/Ca temperatures

Please see our response above.

Lines 395-396: Can the authors expand on this sentence, please?

The previously published Mg/Ca record from Site 1208 (~3350 m) in the North Pacific produces temperatures that are, on average, somewhat colder than present whereas our Central Pacific record from the deeper Site 849 (~3850 m) suggests temperatures that are somewhat warmer than present. We therefore speculated that Site 1208 may be more directly influenced by deep waters forming in the North Pacific (as is hypothesized for the Pliocene), driving the observed temperature difference between these two Pacific records. However, this would imply that the North Pacific sourced water was colder than the (likely southern) source bathing Site 849. Furthermore, the absolute temperatures derived from Mg/Ca at Site 1208 depend strongly on the selected calibration. With only the Mg/Ca record available for that site, there is currently no independent way to cross-check the results like we were able to do at Site 849. We can also not easily transfer the calibration we used at Site 849 to Site 1208 since that record is based on a different species of benthic foraminifera (*Uvigerina spp.*). As a result, we will clarify this part but would also like remain more vague and rephrase this section to: "The existence of a large temperature gradient up to 4°C between these two basins in the Pliocene was first suggested by Woodard et al. (2014). Their foraminiferal Mg/Ca based BWTs from ODP Site 1208 suggests temperatures in the North Pacific were, on average, ~1°C colder (when adjusted for Mg/Ca_{sw} — see suppl. information and Fig. S6) than today during our study interval. In contrast, at Site 849 we observe warmer BWTs (by 1°C) during the mid-Piacenzian compared to today. At face value this discrepancy would suggest that different water masses influenced the North Pacific and the central Pacific. As deep water formation in the North Pacific has been suggested for the Pliocene (Burls et al., 2017; Shankle et al., 2020; Ford et al., 2022), it is possible that temperatures at Site 1208 were more directly influenced by this water mass and are thus less representative of the Pacific Ocean as a whole than those at Site 849. It is however important to keep in mind that the absolute Mg/Ca-based temperatures are highly calibration dependent and are currently only cross-validated with Δ_{47} at our study sites."

Line 410: What are the authors referring to? Model, data? References are missing

This statement does not refer to a specific study but is a speculative explanation, based on end-member mixing, for how we could in principle end up with a large temperature gradient between the Pacific and North Atlantic while maintaining a modern circulation

state. We suggest to make this more clear by rephrasing to “Hypothetically, a possible explanation for the observed temperature gradient, if water mass mixing was identical to today, could be that the Southern Ocean end member cooled enough to compensate for the warm Atlantic waters to produce a cold Pacific end result. Given the globally warm surface conditions of the mid-Piacenzian, this scenario is, however, rather unlikely.”

Lines 458-462: How did the authors conclude on the ice volume proportion of thier d18Osw parameters? How did they remove the salinity part of the d18Osw, as explaining previously?

Please see our reply above; we suggest omitting the salinity calculations to avoid putting unintended emphasis of these very uncertain estimates.