17 July 2024

Dear Erin McClymont

Many thanks for your comments on our manuscript. We have addressed these below, with corresponding line numbers referring to the latest track-changes version.

Best wishes

Dave Chandler

- line 77 “considered less reliable…” Can you clarify that you mean “less reliable as temperature proxies” rather than that the oxygen isotopes are less reliable in general?

We have now written this as “deep water temperature proxy records derived from oxygen … are considered less reliable prior to the MPT” (Line 68).

- Line 180 refers to “local hydrographic controls” perhaps influencing d18Osw but not at glacial interglacial timescales. But given the formation and transport processes of LCDW as described in section 2 (E.g. lines 105-110) is there a chance that mixing with Antarctic waters, which might been more strongly influenced by local impacts on temperature and salinity, could impact LCDW properties?

Yes, if Antarctic water masses (particularly AABW) have a distinct oxygen isotopic signature gained e.g. from ice sheet freshwater fluxes, then this could influence LCDW $\delta^{18}O_{sw}$ as northbound AABW gradually mixes with southbound NADW/LCDW in the lower overturning cell. The glacial-interglacial *change* in LCDW $\delta^{18}O_{sw}$ is very similar to that of the global average (see estimates of $\Delta\delta^{18}O_{swLGM}$ at Line 211), suggesting this local influence is relatively small at our sites, on glacial time scales. However we have highlighted this as a potential avenue for future work, whether focusing on glacial cycle or shorter time scales, at Line 166.

- Related to the previous comment, lines 111-120 there is discussion of the modification of LCDW before it reaches the ice front. In the discussion (line 694) the authors flag the caveat that they reconstruct temperature and not transport: but would they also expect a potential cooling during cross-shelf transport which would also mean their reconstructed temperatures lie towards the maximum expected?

Cross-shelf cooling by mixing with other water masses does mean our reconstructed LCDW temperatures are likely an upper bound on water temperature reaching Antarctic grounding lines. However our synthesis reports a temperature *anomaly*, rather than in-situ temperature. The temperature anomaly at the grounding line should follow the open-ocean LCDW temperature anomaly but with potential further modifications by (1) changes in degree of mixing and (2) changes in temperature of water masses with which LCDW is mixed. It's quite speculative to discuss in detail how that would modify temperature anomalies at grounding lines (they could be adjusted up or down, depending on changes in transport and mixing under past climates). In the text we now note our reconstruction does not account for additional influences of CDW transport rate or modification across the shelf (Line 509).

- Line 392 describes the number of sites used. Two of the sites have two proxies for Tcdw being used, so are they being treated as four separate records (4 sites?). What would happen if only Mg/Ca was used for the two sites where it was available, along with d18O where it was not? Although the potential bias between proxies or regions was explored, the potential bias caused by two records coming from the same site was not.

At sites with both proxies, the two corresponding records are treated as independent estimates in the statistical analysis used to construct means and confidence intervals - so yes, four records from two sites. This is reasonable if most variance in each time slice derives from methodological errors, but not if most variance derives from geographical variability. From Figs 6 & 7 we would suggest it is methodological errors contributing the bulk of the variance, e.g. given there is a stronger match between the Pacific and Atlantic sector sites, than between temperatures reconstructed at the same site with different proxies. See discussion at Lines 405-415. However, there is potential for a slight underestimate of confidence interval widths if the two records from one site are not fully independent. We have added a note of this in the methods at Lines 358.

Use of Mg/Ca where possible, otherwise $\delta^{18}O$:
At ODP 1094 we would end up with a mix of the two proxies, while at ODP 1123 we would almost exclusively use Mg/Ca (very few 1123 time slices have $\delta^{18}O$ and no Mg/Ca). The small differences this makes to the mean at each time slice (Fig R1 below) are not statistically significant given the uncertainty – particularly considering reducing *N* by 2 sites considerably widens the error bars when *N* is already small. These are interesting questions but really need more sites to answer with any confidence. We have noted in the priorities for future work, that application of multiple proxies to the same core samples will be beneficial to quantify biases independently of other sources of variance (Line 578).

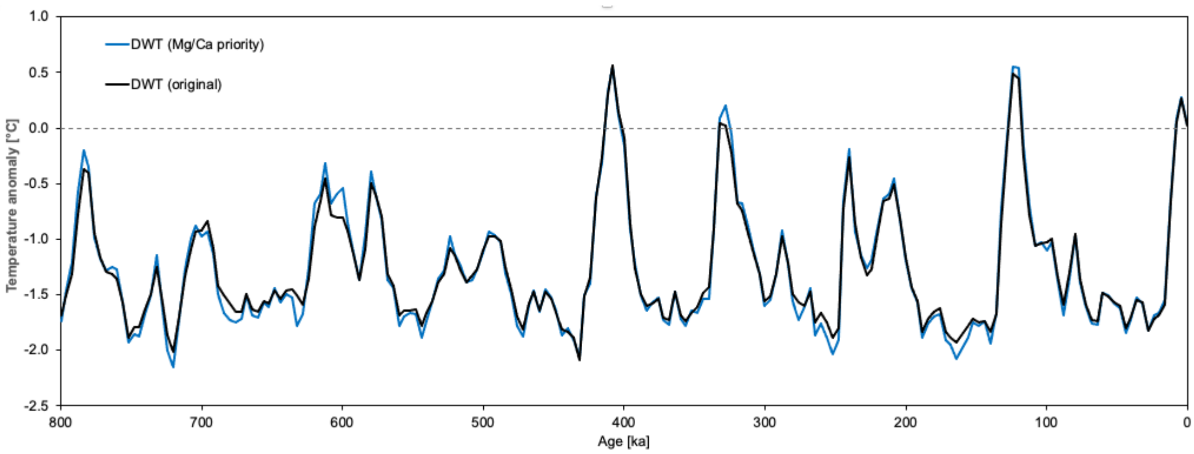

Fig R1: Comparison between DWT estimated using (i) our original synthesis (black line) and (ii) using Mg/Ca where possible but otherwise $\delta^{18}O$ (blue line).

- Line 415 the authors note the Mg/Ca records tend to give cooler glacials and warmer interglacials, so a wider range than suggested by their d18O reconstruction. But the stack is dominated by d18O, so should we treat the stack as a conservative prediction of Tcdw? (Or might this observation explain the quadratic nature of the relationship with other archives due to systematic bias or underestimation of the lowest temperatures?)

If we assume that Mg/Ca is the more reliable of the two proxies, then yes the relatively weaker glacial-interglacial response of $\delta^{18}O$ temperatures would imply our stacked temperature changes are conservative, given the dominance of $\delta^{18}O$ in the stack. However, if it turned out $\delta^{18}O$ was more reliable, then including Mg/Ca would conversely imply that our stack tends to overestimate temperature changes. We haven't commented on which might be the most reliable of the two proxies as we cannot objectively evaluate that with the information we have. A third proxy would be helpful in this respect as now clarified in the priorities for future reconstructions (Line 575).

Regarding the quadratic relationship:
If $\delta^{18}O$ yields less glacial cooling but also less interglacial warming, relative to Mg/Ca, we would still expect linear relationships between reconstructed T_{CDW} and the other temperature indicators, assuming the 'real' relationship with T_{CDW} is linear. Instead we find a quadratic fit, as the T_{CDW} gradient reduces at cooler temperatures. This most likely reflects the already cold temperature of LCDW, which limits the potential for cooling under peak glacial conditions. For example, glacial cooling of AIS surface air temperature is unlimited (in a practical sense) while T_{CDW} cannot cool below its freezing point. See discussion starting at Line 423.

- Figure 5: I can't see the vertical shading described in the text, whether printed or on screen.

We have now darkened the shading so it's easier to see.