Increased abyssal ocean density stratification across the Middle Pleistocene Transition

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In the following, the Reviewers' comments are shown in black, and our responses are shown in blue. We also indicate the line, section or figure numbers where changes have been made in the revised manuscript.

Reviewer Report #1

I am in principle happy with the revision apart from the few points below which need further attention:

We are sincerely grateful to this anonymous Reviewer for their careful and constructive review of our revised manuscript. We greatly appreciate the thoughtful scrutiny and insightful comments, which have helped us to improve the manuscript substantially.

1. I made the point that the hypothesis of Raymo et al. (2006) might not be relevant anymore, since it relies on changes in precession and I argued that precession is not in the recent SST data compilation of Clark et al. (2024) as discussed in Clark et al. 2025 (Science). In the response letter the authors argue that "the absence of detection of significant precession in global mean SST does not preclude precession-paced influences on regional surface salinity or ice–ocean interactions". Note, that the is also no power in precession in the regional SST stacks in Clark et al. (2024) (northern / southern extratropical or tropical SST). While I can follow this argument, I disagree that this is taken to change nothing at all to the discussion of the relevance of the hypothesis of Raymo et al. If this is kept in the paper it should be accompanied with some lines of discussion that the Clark et al data do not support it due to the missing power in precession, but that the authors have reasons to believe that this shows not the whole story. This is even more relevant since the Raymo et al. hypothesis has been directly addressed in the Clark et al. (2025) paper in Science.

We acknowledge this disagreement with Clark et al. (2025) and have revised the text to clarify our reasons for retaining the reference to Raymo et al. (2006) We maintain the lack of precession in the variance spectrum of global temperature by Clark et al. (2025) is due to stacking of relatively low-resolution records with significant chronological error and noise. The higher frequency of precession is affected more so than obliquity by these processes. The suppression of precession relative to obliquity in these types of records has been well documented and discussed by Liautaud et al. (2020) and Liautaud (2021).

For the purpose of this response, as further evidence that the Clark et al. (2025b) reconstruction has underestimated the precession in their spectral analysis, we performed cross-spectral analyses of the rates of change (ROC) of our basin-volume-weighted MDOT and mean $\delta^{18}O_{seawater}$ stacks relative to ETP (the astronomical solution combining normalised eccentricity, tilt and precession; Laskar *et al.* 2004). These analyses cover two-time intervals (0–800 ka and 960–1500 ka) for direct comparison with Clark et al. (2025b). Our results (Table R.1 below) show significant coherence in the precession band at the 95% confidence level for ROC of both MDOT and $\delta^{18}O_{seawater}$ during the 0–800 ka interval, whereas coherence with precession is only significant for MDOT in the earlier interval. These results indicate that precession is expressed in the rates of change of our MDOT reconstructions both before and after the MPT, whereas precession is only significant in the ROC of $\delta^{18}O_{seawater}$ during the later interval and demonstrate that the absence of precession in hemispheric ΔSST reconstructions does not necessarily preclude precession-paced variability in deep-ocean climate. We therefore consider it appropriate to retain the mechanism proposed by Raymo et al. (2006) as one of several possible explanations.

Method:
Cross-spectral analyses were performed to determine the coherence, phase relationships and statistical significance between the rates of change of basin-volume-weighted MDOT and mean $\delta^{18}O_{seawater}$ and orbital forcing (ETP) (Laskar *et al.* 2004). ETP was modified by multiplying the precession index (e*sin(ω)) by –1 to give the appropriate phase relationship with Northern Hemisphere summer insolation. Cross-spectral analyses were performed with linear detrending using ARAND software (Howell *et al.* 2006).

The rates of change (ROC) of the volume-weighted MDOT and mean $\delta^{18}O_{seawater}$ stacks were calculated as the numerical first derivative ROC = dy/dx, where dy is the backward difference between adjacent values and dx is the 3-kyr sampling interval.

Table R.1 Results of cross-spectral analyses of the rates of change of the basin-volume-weighted MDOT and mean $\delta^{18}O_{seawater}$ stacks relative to ETP

Pre-960	Coherence 95% confidence >0.79	Phase angle	Phase Error angle	Phase (kyr)	Phase Error (kyr)
Precession (min)					
Mean $\delta^{18}O_{seawater}$ ROC	–	–	–	–	–
MDOT ROC	0.81	-0.35	0.07	-7.74	1.64
Obliquity (max)					
Mean $\delta^{18}O_{seawater}$ ROC	0.91	-0.03	0.05	-1.07	2.05
MDOT ROC	0.85	-0.15	0.06	-6.34	2.67
Eccentricity (max)					
Mean $\delta^{18}O_{seawater}$ ROC	–	–	–	–	–
MDOT ROC	–	–	–	–	–
Post-800					
Precession (min)					
Mean $\delta^{18}O_{seawater}$ ROC	0.95	-0.46	0.03	-10.94	0.81
MDOT ROC	0.83	0.47	0.07	10.27	1.51
Obliquity (max)					
Mean $\delta^{18}O_{seawater}$ ROC	0.96	-0.04	0.03	-1.80	1.37
MDOT ROC	0.84	-0.12	0.07	-4.86	2.81
Eccentricity (max)					
Mean $\delta^{18}O_{seawater}$ ROC	0.85	-0.17	0.06	-17.16	6.37
MDOT ROC	0.84	-0.31	0.07	-31.19	6.81

In text revision reads:
Raymo et al. (2006) suggested the East Antarctic ice sheet was terrestrial-based with melting margins before the MPT and transformed to a marine-based ice sheet afterward, which would have decreased meltwater to marginal seas. Hemispheric ΔSST reconstructions exhibit little precession-band variability (Clark *et al.* 2025b) and have been interpreted as evidence against this mechanism. However, precession is often underestimated by spectral analysis in relatively low-resolution stacked records with chronologic error and noise (Liautaud et al., 2020, Liautaud, 2021). We therefore include the mechanism proposed by Raymo et al. (2006) as one of several possible explanations for the increase in salinity of Antarctic marginal seas.
Lines 517-529

2. MOT from Allan Hills and MOT from Clark et al. (2025): In the response letter and in the draft (line 571) the authors cite Wolff (2026), which is a News & Views on the Shackleton et al. (2026) Allan Hills MOT data. There, E. Wolff writes: "In my view, the results of Shackleton and colleagues undermines the basis of that study [-> MOT from Clark et al. 2025], because they show that the scaling of sea surface to deep ocean temperature is invalid." Since this is a personal opinion in a comment (without further evidences) I do not think this should be taken as argument in a peer-reviewed paper, and I disagree with that opinion. Furthermore, in Shackleton et al. (2025) the two MOT data sets are never plotted with the uncertainties in both data sets attached. If you do that and if you furthermore consider the suggested cold bias of up to 0.5°C in the Allan Hills data and that

they are probably weighted towards interglacials you would find that both MOT data sets agree within their uncertainties with each other. I therefore ask that the corresponding statements around line 571 are revised accordingly and that the citation to Wolff 2026 is dropped. Related to that one might also say that within uncertainties MDOT of Clark et al. (2025) and this paper (Fig. 10d) are not so different and agree within the uncertainties with each (even if the mean values diverge before the MPT). One might argue that there is missing G/IG variability in the Clark DMOT record, but this is due to the way it was reconstructed (see Clark et al. (2025) in ClimPast for details), because of which the uncertainties are really important and tell that both approaches are not completely disagreeing with each other.

We agree that opinion should not be used to support this comparison and have therefore removed the citation to Wolff (2026). We have also revised the discussion to acknowledge the overlapping uncertainties between the independent reconstructions, while clarifying the differences in the magnitude and expression of glacial-interglacial variability, which reflect differences in the methods used to reconstruct mean ocean temperature.

The revised text reads:
The central estimates of both our MDOT and noble-gas MOT reconstructions (Shackleton *et al.,* 2026) differ from the ΔGMSST-derived deep ocean temperature reconstruction of Clark *et al.* (2025) prior to and across the MPT, particularly through MIS 22. Although the uncertainty envelopes of these independent reconstructions overlap, they differ in the pattern of reconstructed deep-ocean temperature variability, including its amplitude and evolution through time. In particular, the Clark et al. (2025a) ΔGMSST-derived reconstruction exhibits relatively little glacial-interglacial variability in the earlier part of the record prior to MIS 26 (~960 ka), reflecting differences in the approaches used to estimate MOT.
Lines 606–611

3. d18O_sw: The caption to Fig 11 contains that Clark et al. (2025) corrected d18O by 0.083 permil/Myr due to assumed diagenesis. While this might be unproven (as said there), it was brought up by Raymo et al. (2018) in order to understand Pliocene sea level reconstructions. It is up to the authors if they want to add these details here. But I strongly suggest that the discussion on these details is not only found in the figure captions, but also in the main text, where it is missing so far.

We now explicitly note in the main text that unlike our reconstruction the $\delta^{18}O_{seawater}$ reconstruction of Clark et al. (2025) incorporates the long-term benthic $\delta^{18}O$ correction proposed by Raymo et al. (2018). This difference is small relative to the propagated uncertainties of the $\delta^{18}O_{seawater}$ reconstructions and does not alter the principal differences between the two records.

We have removed the statement from the caption in Figure 11, and now include the following text:

The $\delta^{18}O_{seawater}$ reconstruction of Clark et al. (2025) incorporates the long-term benthic $\delta^{18}O$ correction as originally proposed by Raymo et al. (2018) (0.083 ‰ Myr^{-1}), but our

reconstruction does not. This correction amounts to a maximum adjustment of ~0.12 ‰ at 1.5 Ma, which is small relative to the propagated uncertainties of the two $\delta^{18}O_{seawater}$ reconstructions and would increase divergence between them.
Lines 652–655

4. References to Figures. It is not clear to me, why in line 425 Fig S11c is cited for MDOT, while this is also plotted in Fig 8c (but with different weighting). Maybe use both. I think this might also be the case for other figures, where the version on the SI and the version in the main text show similar things, but differ only by small details (weighted by ocean volume or unweighted).

As described in Section 2.6, we presented both unweighted and basin-volume-weighted versions of the MDOT and mean $\delta^{18}O_{seawater}$ stacks because they provide complementary perspectives. Whereas, the unweighted stacks show the mean behaviour of the compilation records, the volume-weighted stacks are intended to represent the average properties of the global deep ocean. We have revised the text to make it explicit which version is being referred to and, where appropriate, have cited both the corresponding main text and supplementary figures to improve clarity.

In line text has been edited to:
The non-weighted MDOT stack (Fig. S11c)…
Line 449

5. All figures are really low resolution and details are hardly detectable. This need improvements.

We thank the Reviewer for pointing this out. The figure resolution has now been improved so that all figures are clear and crisp in the revised manuscript. In addition, faint border artefacts around some figures, particularly the maps, have been removed.

6. line 543: At the end of the line "MPT" is missing.

Done
Line 576

Reviewer Report #2

Thomas and colleagues provide a new Mg/Ca-based deep water temperature record and d18Osw reconstruction from IODP Site U1385 in addition to global compilations of deep ocean temperature and d18Osw. Although I did not review the previous version, it is apparent from the response to reviewers and authors' tracked changes that the authors have made significant modifications in response to the previous reviewers' concerns. To that end, I intend to provide a lighter touch review that mostly focuses on readability. Overall I found this manuscript well suited for publication in Climate of the Past following a series of minor changes, below.

We greatly appreciate the reviewer's careful evaluation of our revised manuscript and their thoughtful consideration of the substantial changes made during the first round of review. We are grateful for their constructive and insightful comments, which have further improved the clarity and readability of the manuscript.

Abstract L8-22. Two minor issues here:
1. The abstract is missing an opening 1-2 sentences of essential framing for this study. Why is it important to have basin-scale and globally distributed compilations of deep-water temperature and d18Osw? The authors provide this in L24-61; they just need include the executive summary of the problem here.
2. Surprisingly, in the abstract the authors do not mention the major data product of their work: A new Mg/Ca-derived bottom water temperature reconstruction from Site U1385. That should be added as a "Here we" sentence coming prior to the current 1st abstract sentence.

We thank Reviewer 2 for highlighting this important omission.

Additional lines to have been added to the Abstract which now starts:
The cause of the fundamental reorganisation of the climate system during the Middle Pleistocene Transition (MPT; ~1.25–0.65 million years ago (Ma)), when glacial cycles intensified and lengthened from 41-kyr to quasi-100-kyr periodicity, remains one of the enduring unsolved questions in palaeoclimate science. Increasing attention has focused on the role of the marine carbon cycle, with enhanced carbon storage in the deep sea linked to changes in deep-water temperature and salinity that increase abyssal ocean density stratification. Here we report new high-resolution reconstructions of deep-water temperature and the isotopic composition of seawater ($\delta^{18}O_{seawater}$), a proxy for ice volume and salinity, from North Atlantic Site U1385, using paired benthic foraminiferal Mg/Ca and oxygen isotope ($\delta^{18}O$) analyses. Together with published records, we present basin-scale and globally distributed compilations of deep-water temperature and $\delta^{18}O_{seawater}$ to assess changes in abyssal ocean stratification for the past 1.5 million years (Myr). Across the MPT,...

Lines 8–16

L24. Is 0.64 million a typo, perhaps from being one key off to the left? Usually the MPT is noted as 1.25 to 0.75 Ma (e.g., Herbert, 2023).

There is no universally accepted definition of the duration of the MPT. As noted by Clark et al. (2006), estimates vary considerably, with recent literature placing its termination between approximately 0.75 and 0.6 Ma. To remain consistent with recent studies for Site U1385 (e.g. Hodell et al. 2023; Martin-Garcia et al. 2018), we have revised the text to define the MPT as extending to ~0.65 Ma.
Line 31

L32. Also cite Chalk et al. 2017 and Farmer et al. 2019, here, as both studies delved into the NH vs SH controls on the MPT.

Done
Lines 39-40

L75-78. Not necessary, but this would be a useful distinction to visualize in a supplementary figure.

We appreciate the Reviewer's suggestion that this distinction could be useful to visualise and now include an additional supplementary figure (Fig. S1) shown below (Fig. R2.X) to illustrate the point. The Rohling et al. (2021) and Elderfield et al. (2012) reconstructions do not show the pronounced cooling in deep-water temperature during MIS 24 and 22, that is evident in the Clark et al. (2025) reconstruction.
Figure S1 is now referred to in:
Lines 86–88

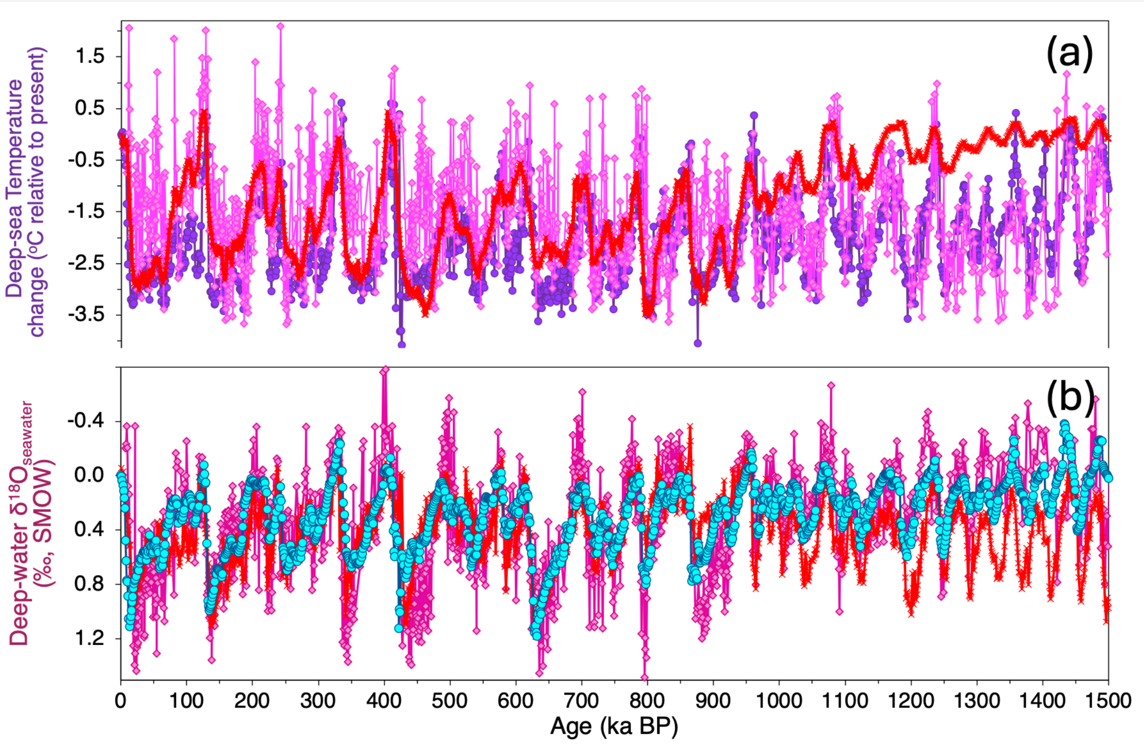

Fig. R.2X Comparison of deep-water temperature and $\delta^{18}O_{seawater}$ reconstructions derived from proxy- and process-based approaches.
(a) Deep-water temperature reconstructions relative to the present using Mg/Ca-palaeothermometry from Site 1123 (pink line with diamonds; Elderfield et al., 2012) and process-based approaches from Rohling et al. (2021) (purple line and circles) and Clark et al. (2025a) (red line with crosses). (b) $\delta^{18}O_{seawater}$ reconstructions derived using the same approaches in (a) from Site 1123 (dark pink line with diamonds; Elderfield et al., 2012) and process-based reconstructions from Rohling et al. (2021) (blue line and circles) and Clark et al. (2025a) (red line with crosses).

L79-90, and again on L119. A question on the veracity of the stacking approach: Is it to be expected that U1385, at a 2578m and in the northeastern basin, would record the same

watermass characteristics as DSDP 607, at 3427m and in the northwestern basin? On L119 the authors state that "All study sites are deeper than 2500m and are therefore assumed to be representative of the deep ocean". That is a major oversimplification. And it is noteworthy to this reviewer that the temperature estimates in Table 1 are about 0.7°C offset for the two sites, which is significant for the deep ocean and suggests that the two sites may reside in different "limbs" of modern NADW. I should note that the results section supports the stacking approach, but it would help to have a bit more modern support for the method.
To address this, the authors should compare the modern density of seawater at these two locations to see if they fall within the same watermass (see, e.g., Liu & Tanhua, Ocean Sciences, 2021).
Liu, M. and Tanhua, T., 2021. Water masses in the Atlantic Ocean: characteristics and distributions. Ocean Science, 17(2), pp.463-486.

We agree that our previous wording was an oversimplification. Sites U1385 and 607 are not expected to record identical water-mass properties because they occupy different parts of the North Atlantic and are influenced by different proportions of Labrador Sea Water (LSW), Iceland–Scotland Overflow Water (ISOW), Denmark Strait Overflow Water (DSOW), and Antarctic Bottom Water (AABW). Modern hydrographic observations (Figure R.2XX) show that both sites nevertheless lie within the Lower North Atlantic Deep Water density range ($\gamma n \approx 27.95–28.10$ kg m^{-3}; Liu and Tanhua 2021), although Site 607 is colder because it is deeper. The purpose of the stack is therefore not to imply that the two sites record identical water masses, but rather to estimate the mean properties of deep North Atlantic waters below ~2500 m by combining records. Furthermore, despite subtle differences between the two records due to their different hydrographic settings, both capture the major features of climate variability across the MPT. This consistency provides additional support for combining the records to represent basin-scale deep-water evolution. We have revised the manuscript to clarify this point.

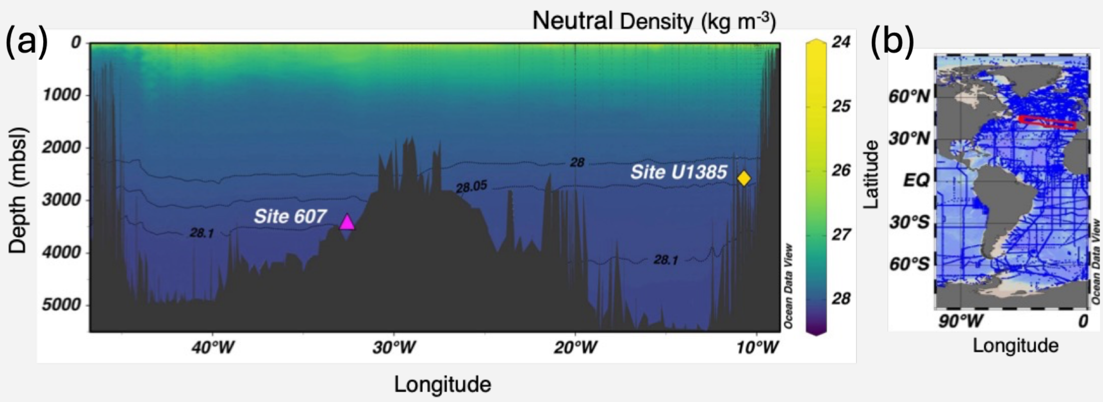

Fig. R.2XX Modern hydrographic setting of Sites U1385 and 607. (a) Neutral density (γ^n; kg m^{-3}) along a North Atlantic hydrographic transect passing through Sites U1385 and 607. (b) Map showing the location of the transect route. The figure was created in Ocean Data View (Schlitzer, 2019; http://odv.awi.de) using GLODAPv2.2023 data.

Text added in Lines 130–137
Although both sites lie within the deep North Atlantic, they are influenced by different mixtures of deep-water masses. In the In the modern ocean, Sites U1385 and 607 occupy

different parts of Lower North Atlantic Deep Water and receive varying proportions of Labrador Sea Water, Iceland–Scotland Overflow Water, Denmark Strait Overflow Water and Antarctic Bottom Water (Liu and Tanhua 2021). The relative contributions of these water masses to each site also likely varied through glacial–interglacial cycles. Modern hydrographic observations nevertheless place both sites within the Lower North Atlantic Deep Water neutral density range ($\gamma^n \approx 27.95$–28.10 kg m^{-3}). The North Atlantic stack is therefore intended to represent the average properties of deep North Atlantic waters rather than a single homogeneous water mass.

L93. Simplify: "Although there are few deconvolved benthic…"

We have revised the sentence for clarity.

In line text now reads:
Although relatively few long reconstructions based on deconvolved benthic Mg/Ca–δ^{18}O records are available for stacking over the past 1.5 million years (Myr), some distinct differences emerge in the patterns of inferred deep-ocean temperature and salinity change between the North Atlantic and Pacific basins.
Lines 104–106

L148. meters

Changed to metres
Line 163

L153-163. If this wasn't a revision and I wasn't providing a review for the first time, I'd recommend that the authors use a consistent age model approach (ProbStack) for all records. But, alas, I 'm not sure that's an entirely fair new comment at the revision stage, so I'll defer to the authors if such consistency is necessary for their stacking approach.

We appreciate the reviewer's suggestion to transfer all records onto a new age model but we note that LR04 and Prob-stack show close agreement in the timing and overall structure of glacial–interglacial variability over the past 1.5 Ma, and only minor differences exist between the stacks (Ahn et al., 2017). Given that our analyses focus on orbital-scale phase relationships, we consider the use of LR04 appropriate for the stacking approach adopted here.
Now relative to Lines 168–177

L265-290. Solving for d18Osw. A question here concerns the alignment of data generated from different foraminifera species with different depth habitats. G. affinis can be found quite deep (up to 15 cm), whereas C. wuellerstorfi is thought to be epifaunal (e.g., Corliss, 1985). In the case where the authors are calculating d18Osw from d18O in C. wuellerstorfi and BWT derived from Mg/Ca in G. affinis, how sure are they that the two proxy measurements are in fact contemporaneous in time? One might expect that the C. wuellerstorfi d18O data lead the G. affinis data.

To address this, the authors could compare d18O from C. wuellerstorfi and G. affinis in

the same depths; if they are comparable with known species offsets, then this depth habitat offset is probably negligible. They could also note the high sedimentation rate of the site (10 cm/kyr, Hodell et al. 2015) which would make any depth habitat difference minor in time. And the 3kyr interpolation (L305) probably makes this a moot point. But worth adding a sentence or two in the methods to note/refute this habitat depth issue.
Corliss, B.H., 1985. Microhabitats of benthic foraminifera within deep-sea sediments. Nature, 314(6010), pp.435-438.

We thank the reviewer for this helpful comment and compared paired benthic $\delta^{18}O$ measurements of *C. wuellerstorfi* and *G. affinis* from the same sediment samples after applying the established species corrections. Across 3,580 paired measurements spanning the full Site U1385 $\delta^{18}O$ record, the mean residual difference is only -0.02 ± 0.23 ‰ (1σ), indicating close agreement between the two species. Restricting the comparison to 98 available paired samples used in the Mg/Ca analyses gives a similarly small residual difference of -0.04 ± 0.26‰ (1σ). In light of these results, together with the high sedimentation rate at Site U1385 and interpolation of the reconstructions onto a 3-kyr interval, we have added a short paragraph to the Methods describing why any temporal offset associated with differing microhabitats is unlikely to affect our reconstructions.

The inserted paragraph at the end of Section 2.2 reads:

Species-specific benthic $\delta^{18}O$ offsets are commonly assumed to be constant when correcting between co-occurring taxa, although temporal variations have been reported Hoogakker et al., 2010). Where same-species paired $\delta^{18}O$ measurements were unavailable *C. wuellerstorfi* $\delta^{18}O$ values were supplemented. Comparison of paired $\delta^{18}O$ measurements from *C. wuellerstorfi* and *G. affinis* (n =3,580*)* at Site U1385 indicates that the corrected values agree closely with a mean residual difference of only -0.02 ± 0.23 ‰ (1σ). Restricting the comparison to 98 available paired samples used for the Mg/Ca analyses yields a similarly small residual difference of -0.04 ± 0.26 ‰ (1σ). Together with the high sedimentation rate at Site U1385 (~11 cm kyr^{-1}; Hodell *et al.* 2015) and interpolation of the reconstructions to 3-kyr resolution (Section 2.6), this suggests that any temporal offset associated with differing microhabitats is likely to be negligible relative to the temporal resolution of the study.
Lines 188–196

L378. For ease of comparison, please express the equivalent amounts of cooling at U1385 over the two time intervals.

To facilitate comparison, we have revised the manuscript to include the equivalent description for Site U1385 over the same intervals discussed for Site 607. Linear regression indicates significant long-term cooling at Site 607 before 900 ka, whereas Site U1385 shows little evidence for a comparable long-term cooling trend over the same time interval.

The manuscript text has been revised accordingly and now reads as:

Greater cooling is expressed at Site 607, where temperatures show decreasing trends of ~1.2 °C between 1500–900 ka and ~1.6 °C over the past 1.5 Myr. In contrast, Site U1385 shows no statistically significant long-term cooling prior to ~900 ka and a decreasing trend of ~0.8 °C across the full record.
Lines 404–406

L383-384. Agreed.

No response required
Lines 408–411

L385-386. Please delete this. The comparison to SST is not a point for results, but rather a point for the discussion further down when these data are presented in Fig. 9 and L468-470.

Deleted
From Line 411

Figures 3-7 & throughout. It would really help the reader if all records from one region maintained a consistent color family throughout these figures. For instance, Site 1123 shifts from light blue (Fig. 3, 5) to dark blue (Fig. 4, 6) to pink/red/purple in the stacks in Fig. 7. Why not keep Atlantic red-scale and Pacific blue-scale, or vice versa?

We thank the Reviewer for this helpful suggestion that consistent colour families may assist readers in tracking records between figures. We have revised the colour scheme throughout the manuscript so that individual site records are more consistently grouped by ocean basin. North Atlantic records are now shown using blue–green colours, whereas Pacific records are shown using red–purple colours where practical. We have retained the existing colours for the basin-scale stacks in Fig. 7 because these represent compiled basin reconstructions rather than individual site records. This revised colour scheme improves consistency between figures while maintaining clear discrimination between individual records and accessibility for readers with colour-vision deficiencies.

L416-417. How are you evaluating that Atlantic d18Osw increased across the MPT? I cannot see any notable change.

We agree that the Atlantic increase is considerably more subtle than that observed in the Pacific and have clarified the text accordingly. To quantify the change, we compared the distributions of Atlantic $\delta^{18}O_{seawater}$ values before and after ~930 ka. The post-930 ka interval exhibits modestly higher values (mean 0.57‰ versus 0.48‰; median 0.58‰ versus 0.46‰), together with higher upper percentiles. A Mann–Whitney U test also indicates a difference between the pre- and post-930 ka distributions ($p = 0.0024$), although this result should be interpreted cautiously because neighbouring observations in a palaeoclimate time series are not statistically independent.

We have revised the manuscript to make this more explicit.
From: Across the MPT, deep ocean glacial $\delta^{18}O_{seawater}$ increased in both ocean basins but the deep Pacific values increased more than those of the deep Atlantic (Fig. 7c).

To: Across the MPT, glacial deep ocean $\delta^{18}O_{seawater}$ increased markedly in the Pacific and more modestly in the deep Atlantic (Fig. 7c).
Lines 440–441

L424-425. It would be worth reminding the reader if this is the stack in question here is volumetrically weighted.

Agreed. Consistent with Reviewer 1's comment, we have clarified in the text that the MDOT stack referred to here is the non-weighted stack.

The text now reads:
'The non-weighted MDOT stack...'
And '… the non-weighted mean $\delta^{18}O_{seawater}$ stack…'

Lines 449 and 450

L474. Missing period at end of sentence

Added
Line 500

L474-478. Suggest reversing the order of these sentences, with the eNd interpretive caveat following the eNd data sentence, but before the d13C gradient sentence.

The order of sentences has now been reversed
Lines 500–504

L479-480. Worth stating the assumption that the salinity of NCW was maintained.

We have edited the manuscript text accordingly.
Original text:
Cooling of Northern Component Water (NCW) would increase its density and create a stratification inversion with deeper water masses in the absence of a density increase of SCW (Knorr et al., 2021).

Edited text reads as:
Cooling of Northern Component Water (NCW), assuming its salinity remained approximately constant, would increase its density and create a stratification inversion with deeper water masses in the absence of a density increase of SCW (Knorr et al., 2021).
Line 505–507

L522-540. The authors' might be able to make their lives much simpler here if they reframe this discussion in terms of how the processes they discuss impact the global efficiency of the biological pump (e.g., Sigman et al., 2010). The relevant processes needed to sequester atmospheric CO2 in the deep ocean require that a greater fraction of the nutrients in the ocean interior are derived from the remineralization of sinking organic matter ("regenerated") instead of advected via deep water formation ("preformed"). In ocean models, any increase in the regenerated/preformed nutrient content of the deep ocean has been shown to sequester atmospheric CO2 (Marinov et al. 2006; 2008). This could happen via (1) a decrease in the rate of upwelling ("stratification") and/or (2) an increase in the consumption of major nutrients.

Marinov, I., Gnanadesikan, A., Toggweiler, J.R. and Sarmiento, J.L., 2006. The southern ocean biogeochemical divide. Nature, 441(7096), pp.964-967.

Marinov, I., Gnanadesikan, A., Sarmiento, J.L., Toggweiler, J.R., Follows, M. and Mignone, B.K., 2008. Impact of oceanic circulation on biological carbon storage in the ocean and atmospheric pCO2. Global Biogeochemical Cycles, 22(3).

We thank the reviewer for this helpful suggestion. We agree that framing the discussion in terms of biological pump efficiency provides a useful conceptual context. We have therefore revised the discussion to note that enhanced deep-ocean carbon storage requires an increased proportion of regenerated relative to preformed nutrients, consistent with the concept of Sigman et al. (2010) and the modelling studies of Marinov et al. (2006, 2008). The subsequent discussion of increased export production and reduced Southern Ocean ventilation has been retained because these are the physical and biogeochemical mechanisms by which biological pump efficiency may have increased across the MPT.

Instead, enhanced storage requires a more efficient biological pump, whereby a greater proportion of nutrients in the deep ocean are regenerated through remineralisation of sinking organic matter rather than supplied as preformed nutrients during deep-water formation (Marinov et al., 2006, 2008; Sigman et al., 2010).
Lines 561–564

L542. it is

Corrected
Line 575

L545-546. I agree, but nonetheless recommend deleting. We should not assume that an unpublished record is going to solve a current problem. What if the gas record is compromised by diffusion or respiration, or if the samples are lost?

We accept the Reviewer's recommendation and have deleted the sentence to referring to unpublished but greatly anticipated BE-OI core data. We now refer to Yun and Timmermann (2026).
Line 578

L561. Change "older than" to "before", and "after which" to "but after 1200 ka the amplitude…"

We thank the reviewer for suggesting improvements to the clarity and flow of our text and have revised it accordingly.

Text revised to:
Our MDOT reconstruction agrees well with R21 before ~1200 ka, although after ~1200 ka R21 exhibits larger-amplitude changes (Fig. 10a).
Line 594–696

L579. Typo: Site 607

Corrected
Line 617

L584-585. I'm starting to wonder if the authors were paid to advertise the Beyond EPICA project! See point above. It would be sufficient to say "higher resolution MOT data are needed to capture the full amplitude of glacial-interglacial variations".

Removed 'from BE-OI' and the Barbante et al., 2025 reference.
Line 623

L608-609. What does it mean for Site 1208 to have "anomalously high temperatures during MIS 22"? What is the point for comparison?

We thank the Reviewer for this highlighting this detail and have revised the manuscript text.

Previously read:
This is the result of a warming trend of MDOT during MIS 22 that yields greater $\delta^{18}O_{seawater}$ values when the signal is deconvolved from the benthic $\delta^{18}O$ of calcite. Deep North Pacific Site 1208, in particular, shows anomalously high temperatures during MIS 22 that contribute to this discrepancy.

Revised to:
This is the result of a warming trend of MDOT from MIS 24 through MIS 22 that yields greater $\delta^{18}O_{seawater}$ values when the signal is deconvolved from the benthic $\delta^{18}O$ of calcite. While all records exhibit warming to varying degrees over this interval, the more prolonged warming in the Pacific records, particularly at Site 1208, has a greater influence on the basin-volume-weighted MDOT stack and therefore contributes to the discrepancy with R21.
In lines 643–647.

L611. Specify who "They" refers to, as the previous sentence switched orientation from the authors' results, to Clark et al.'s results, back to the authors' results.

Inserted Clark et al. (2025).
Line 649

L639-641. Wouldn't it be more direct to look for evidence of dissolution in the tests used, e.g. Poirier et al. (2021)? B/Ca would tell you the carbonate ion saturation of the overlying water column, which if anything could be buffered by dissolution.
Poirier, R.K., Gaetano, M.Q., Acevedo, K., Schaller, M.F., Raymo, M.E. and Kozdon, R., 2021. Quantifying diagenesis, contributing factors, and resulting isotopic bias in benthic foraminifera using the foraminiferal preservation index: Implications for geochemical proxy records. Paleoceanography and Paleoclimatology, 36(5), p.e2020PA004110.

We thank the reviewer for this helpful suggestion and have revised the text to acknowledge that a direct assessment of test preservation (e.g. using the Foraminiferal

Preservation Index of Poirier et al., 2021) would provide a more direct assessment of dissolution, alongside complementary B/Ca measurements to constrain carbonate ion conditions.

The revised text reads:
Direct assessment of test preservation (e.g. using the Foraminiferal Preservation Index; (Poirier et al., 2021), together with complementary B/Ca measurements on the same samples, would would help evaluate the extent to which dissolution may have influenced Mg/Ca palaeotemperature reconstructions from different benthic foraminiferal species.
Lines 677-680

L642. Delete space between sigma and end-parenthesis.

Done
Line 681

L655. Reference typo: Shackleton and Opdyke, 1973.

Corrected and changed to Shackleton and Opdyke, 1976
Line 694

L655. The MPT is thought to involve a fundamental change…

We have edited the text to now read: Our results are consistent with the view that the MPT involved a fundamental change…
Lines 694-795

L658. Suggest, or show?

We have retained "suggest" because the inferred changes in abyssal properties are based on proxy reconstructions from a limited number of sites and therefore represent an interpretation rather than a direct observation of the global abyssal ocean.
Line 697

L669-671. Now here is fine to include Beyond EPICA
Thank you!
Lines 708-710