We are very grateful to the two anonymous reviewers for their comments, which have helped us to improve this manuscript. We have addressed all comments raised by the reviewers and will revise the manuscript accordingly. The main changes to the revised manuscript will be that we will add a supplement and move certain figures there, following reviewers' feedback. Additionally, we will add extra figures as suggested by the reviewers into the supplement. A point-by-point response is provided below, with reviewer comments shown in black italics and our responses shown in blue.

S. Radionovskaya, on behalf of all co-authors

Reviewer 1: The authors present a rich dataset to document deepwater circulation variability during MIS4 obtained with robust methodologies. They suggest that despite different ice volumes, MIS4/3 and MIS1/2 transitions show similarities and that similar DO-type variations might have different impacts on deep-water circulation. However, I do have some comments that I would like the authors to address before considering their manuscript for publication. My comments are as follows:

We are very grateful for the comments provided by the reviewer, which we address below.

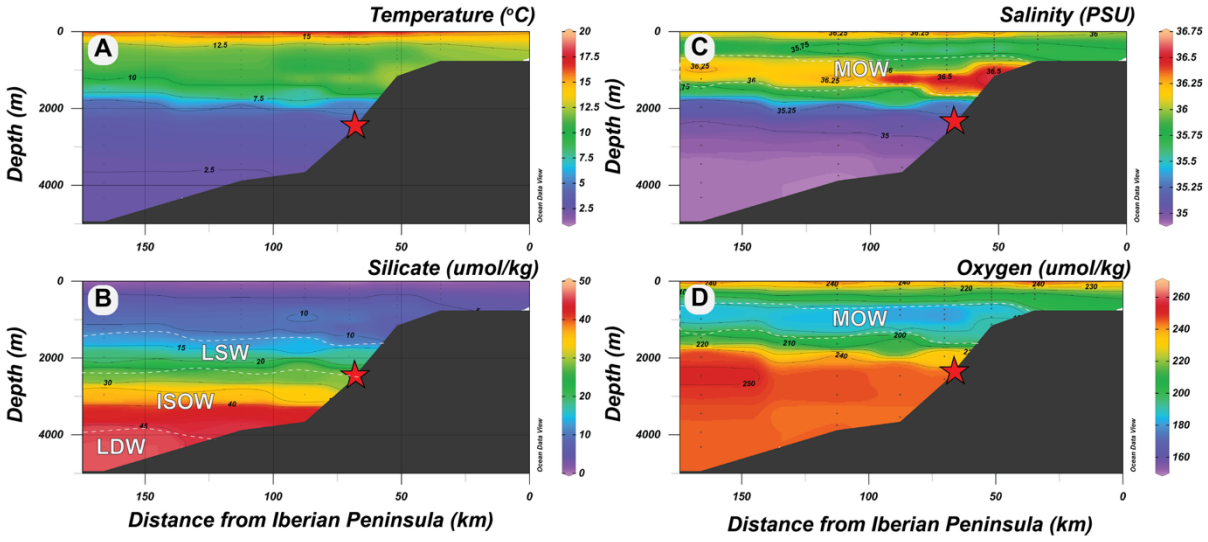
1) I suggest you take a look at the new paper by Hughes et al. (2026) about stadials-interstadials:
https://www.sciencedirect.com/science/article/pii/S0277379126004038#sec6

We thank the reviewer for this suggestion. We are aware of this recent paper, which was published after submission of our MS. All references to "stadials" and "interstadials" in our MS refer to Greenland Stadials and Greenland Interstadials, following the stratigraphic framework of Rasmussen et al. (2014). We will clarify this in the revised MS.

We refrain from referring to MIS 5a an "interstadial" to avoid confusion with Greenland Interstadials. In the context of our study, MIS 5a represents an intermediate climatic state between the full interglacial conditions of MIS 5e (and the Holocene) and the glacial conditions of MIS 4 (and the LGM). We will therefore revise the terminology throughout the MS, removing references to "interglacial" MIS 5a and instead referring to it as "substage MIS 5a". Further details of our terminology and rationale are provided in the Introduction of Radionovskaya and Skinner (2026).

2) You speak about many water masses. I think you should display them on a map along with a temperature-salinity transect.

We thank the reviewer for this helpful suggestion and agree that a map and/or hydrographic cross-section would provide useful context for the water masses discussed in the manuscript. We will add the following figure to our revised MS.



3) MOW expansion can reach down to ~1800-1900 m below its lower core range at ~1500 m as diluted parts in some locations. You can check it with GLODAP data. You suggest that because its core range is 500-1500 m, MOW has not affected your site (lines 122-123, line 500), which is ~2.65 km deep, but in line 611, you suggest your site was affected by subsurface warming. That part, I think, you should reassess thoroughly.

We thank the reviewer for raising this interesting point. We agree that MOW can undergo substantial mixing and dilution as it propagates along the Iberian Margin, and that its influence is therefore not necessarily restricted to its core depth range. We have reassessed this interpretation using available hydrographic observations and reanalysis products.

At the Iberian Margin, the present-day MOW core occupies approx. 500-1500 m water depth in GLODAP v2 (Liu and Tanhua, 2021), OVIDE hydrographic observations (Garcia-Ibanez et al., 2015) and CMEMS reanalysis data (de Pascual-Collar et al., 2019). Although diluted MOW can extend below this core range, its direct influence at our site, at ~2.65 km water depth, appears unlikely based on these datasets.

We agree, however, that the occurrence of subsurface warming at our site warrants careful consideration. We observe pronounced “subsurface warming”, i.e. warmer bottom-water temperatures at 2.65 km depth, during MIS 5a stadials C19 and C20 (Figure 6). As discussed in lines 500-504, we consider it unlikely that the warming resulted from glacial deepening of MOW. If MOW was responsible for the warming, we would expect to see an associated increase in Cibs $\delta^{13}C$ (>1 ‰), consistent with the influence of Mediterranean-derived waters (e.g. Voelker et al., 2006), which is not observed during C19 and C20 (Figure 6). We instead discuss other possible explanations in Section 5.1 and conclude that the warming most likely reflects changes in deep water source regions in the subpolar North Atlantic, i.e. more southward convection sites during C19 and C20 (lines 506-511).

4) Trace elements - in continental margins, regardless of cleaning methodology, high Fe values can be observed, possibly due to sediment input

(https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2023GC010885). The samples you might have potentially discarded as contaminated may not necessarily be contaminated.

We agree with the reviewer that elevated Fe/Ca values can occur in benthic foraminifera from continental-margin settings, event following cleaning. Fe/Ca in our samples ranges from 0.09-0.33 mmol/mol, with only four samples exhibiting values >0.21 mmol/mol. These values are consistent with previous measurements of *G. affinis* Fe/Ca from the Iberian Margin: Skinner and Elderfield (2007) report a range of ~0.1- 0.22 mmol/mol, Skinner et al. (2003) report a range of ~0.1-0.3 mmol/mol (Fig. 3B), and Thomas et al. (2026) report values of up to 0.79 mmol/mol.

Importantly, we did not exclude any measured samples from the dataset presented in our MS on the basis of contaminant metals (Fe/Ca, Mn/Ca, Al/Ca). Rather, we assessed the Fe/Ca values together with Mn/Ca and the downcore trends shown in Figure 2 of the original manuscript. The four samples with relatively elevated Fe/Ca also do not coincide with the C19 and C20 warming events (Fig. 2).

5) 3.6.3 Tdw Uncertainty Modelling - I find this section overwhelming. The calibration equation already comes with an error margin. I do not see the point of this extra analysis. If you wish to keep it, I suggest moving the details to supplementary material, including the additional plots.

We are grateful to the reviewer for this comment. We will move uncertainty modelling Figure 3 into the supplement in the revised MS.

Indeed, the calibration equation already includes an error margin. Our Monte Carlo analysis addresses additional sources of uncertainty associated with the reconstructed temperature record, including analytical uncertainty, proxy parameters uncertainties (such as sampling) and random noise. Importantly, we do not use the absolute temperature values to draw our main conclusions. Instead, our interpretation focuses on the temporal variability and magnitude of temperature changes (section 3.6.1).

The uncertainty envelopes shown in Figure 6 therefore represent the uncertainties quantified by the MC analysis described in section 3.6.3 and do not include the calibration uncertainty associated with the Mg/Ca-temperature relationship. We will clarify this distinction and simplify the description of the approach in the revised MS, following the reviewer's suggestion and comments of Reviewer 2.

6) Line 301-307: If you think there are uncertainties caused by RSL, why did you apply this method? You also do not discuss what these uncertainties are or why the 6ka window is any better than the previous method. If you wish to keep RSL calculations, move them to supplementary material.

We agree that the detailed RSL calculations and Figure 4 are better suited to the Supplement, and we will move them accordingly.

As explained in lines 295-299, $\delta^{18}O_{dw,\ local}$ (i.e. $\delta^{18}O_{dw}$ corrected for "ice volume effect") can be estimated by "subtracting" an RSL reconstruction scaled to $\delta^{18}O$ from $\delta^{18}O_{dw}$ record. However, this approach introduces uncertainty associated with the choice of RSL reconstruction. For example, some studies suggest millennial-scale RSL variability during MIS 3 (Siddall et al., 2003), whereas others do not identify such variability (e.g. Rohling et al., 2008). Furthermore, multiple RSL reconstructions are available, meaning that the resulting $\delta^{18}O_{dw,\ local}$ record would depend on the reconstruction selected. We therefore use subtraction of 6-ka moving mean as an alternative approach that does not require an assumed prior knowledge of past RSL. The 6-ka window is intended to remove the long-term component of the record while retaining the shorter-timescale variability of interest, rather than to represent a specific physical timescale of RSL variability.

Importantly, Figure 4 demonstrates that both approaches produce very similar $\delta^{18}O_{dw,\ local}$ records. We also do not use the resulting $\delta^{18}O_{dw,\ local}$ record to derive absolute salinity estimates because of additional uncertainties in the $\delta^{18}O$- salinity relationship and its calibration. Instead, we use this record to examine residual hydrographic variability not explained by temperature or global ice volume changes. Our conclusions therefore depend on the temporal variability in the record rather than its absolute values.

7) Line 390-391: Brine rejection - this comes out of nowhere and needs a reference. Plus, some studies argue against brine rejection (i.e. doi.org/10.3402/polar.v27i2.6172, https://doi.org/10.1002/2015PA002815). Hence, your interpretation in line 486 may align with earlier interpretations in previous studies.

We agree that our original wording was unclear and could imply that brine rejection is an established mechanisms of deep-water formation throughout the North Atlantic. Our intention was instead to explain the difference in $\delta^{18}O$ between NADW and AABW. We will revise the sentence to:

"In the modern Atlantic Ocean, NADW has higher $\delta^{18}O$ than Antarctic Bottom Water (AABW), reflecting its high-latitude source regions and combined effects of cold surface water and high rates of evaporation, whereas AABW is made dense via brine rejection (Adkins et al., 2002; Ferrari et al., 2014; Thomas et al., 2026)."

8) Line 399: "mode of operation of deep-water formation" - what does it mean?

We thank the reviewer for highlighting this unclear wording. By "mode of operation of deep-water formation" we mean the mechanism and geographical source region of deep-water water formation, including, for example, open ocean convection versus shelf-based formation (e.g. Haine, 2021) and changes in the latitude or location of deep-water convection sites (e.g. Blaser et al., 2025; Wharton et al., 2026). To improve clarity, we will add this clarification to the revised MS.

9) Line 487: "However, the origin of this warming remains debated." - needs reference.

We agree that this statement requires clarification. We will revise this sentence to "However, the mechanism responsible for this warming remains debated with previous studies proposing different explanations." The following discussion then outlines the proposed mechanism and their supporting evidence, with the relevant references, before discussing the interpretation that we consider most consistent with our data.

10) Lines 610-611: "mode … of subsurface heat budget variability" - what does it mean?

We thank the reviewer for asking us to clarify this terminology. Here, by "mode of heat budget variability" we intended to refer to the characteristics of subsurface warming, including:

(i) its geographical distribution, e.g. whether heat anomalies are restricted to high northern latitudes or extend to mid-latitudes such as the Iberian Margin;
(ii) the depth to which subsurface warming penetrates; and
(iii) the magnitude of the warming.

We show in lines 431-437 that the subsurface warming observed at 2.65 km depth at our Iberian Margin site during MIS 5a stadials C19 and C20, is not observed at the same site during MIS 3 Greenland Stadials or Heinrich Stadials. Therefore, we interpret the contrasting spatial and temporal characteristics of the warming as evidence that different dynamics operated during the intermediate climatic conditions of MIS 5a compared with the glacial stadials of MIS 3. This interpretation is also consistent with modelling studies discussed in lines 439-444 (Knorr et al., 2021).

11) There are typos such as Mccave, Carrrara, and there is also a typo in Fig. 3c.
We thank the reviewer for bringing these errors to our attention. We will correct these in the revised MS, and carefully check the revised MS and figures for similar errors.

Reviewer 2: This paper presents a new sortable silt record and a new benthic foraminifera Mg/Ca-derived deep water temperature record from MD01-2444 on the Iberian Margin from MIS 5a and MIS 4. This is a well-studied core site and region, and the authors utilize both previously published and newly reported data to make the valid and substantial conclusion that three different deep-water hydrographic regimes occurred throughout this interval. The scientific methods are sound, but aspects of the methods section should be revised according to the comments below. The assumptions and uncertainties associated with the data are outlined for the Mg/Ca measurements and temperature calibrations, but not are not discussed for the sortable silt flow speed records. The discussion of the assumptions and uncertainties associated with the Mg/Ca data and temperature calibrations could also be improved for clarity. See the comments below. The authors should also choose distinct symbols to represent standard deviation and standard error, as they currently use sigma for both. The authors should consider changing the title of the paper to include both MIS 4 and late MIS 5, which would be more representative of the content. The authors did not provide any supplemental material and indicate that the data associated with the paper will be uploaded to public repositories, but it is currently not available for review.

We are very grateful for the comments and feedback provided by the reviewer, which we address below.

We will revise the title to: “Deep-water hydrographic variability in the Northeast Atlantic during MIS 5a and MIS 4”

All the new data associated with this MS has been uploaded to PANGAEA (**https://doi.pangaea.de/10.1594/PANGAEA.997203**). The data includes *G. affinis* trace metal data (Mg/Ca, Fe/Ca, Mn/Ca), new *G. affinis* $\delta^{18}O$, and sortable silt mean and % data. PANGAEA dataset is currently password protected but will be made open access upon publication of this study. In the meantime, we are happy to share the data with the reviewers, if required.

Section & Figure Comments:

Section 2: I recommend adding a map figure and/or section plot of modern oxygen at the Iberian margin that shows the vertical distributions of these different water masses, with the depth and location of MD01-2444 labelled. That will make the paper more accessible to those unfamiliar with this region.

We thank the reviewer for this helpful suggestion and agree that a map/ cross- section figure would be useful. We will add a new figure showing the main water masses and their vertical distributions, together with the location and depth of MD01-2444. Please see our response to Reviewer 1.

Section 3.1. Chronology: How different is the newly developed chronology from the original? This could be good to include in a supplement. Also, I believe the age model tie points and information should be reported in a supplement, repository, or pre-print prior to publication to make sure the figures are reproducible? There may be an exception if the other publication mentioned is in review elsewhere, but since the tie points are shown in Figure 1 maybe those could be reported in this study.

We thank the reviewer for raising this important point. The chronology used in this MS has recently been published in Radionovskaya and Skinner (2026), which provides the full age model and tie point information for MD01-2444. Figure 2 of Radionovskaya and Skinner (2026) presents the age model over the 50-85 ka interval considered here, while supplementary table S1 provides all tie points, including references to previously published tie points. The chronology used in the present study is therefore fully documented and publicly available in the published study, and we do not repeat the information here.

The principal difference between the chronology used here and the previously published age model for MD01-2444 is the incorporation of additional stratigraphic tie points that allow the millennial-scale variability observed in the record to be more accurately aligned with the Greenland ice core records. The only previously published age model that covers the full 50-85 ka interval of interest is that of Hodell et al. (2023) and (Hodell et al., 2013) (lines 127-129), which use the same time points in both studies. When the MD01-2444 *G. bulloides* $\delta^{18}O$ record is plotted against the Greenland $\delta^{18}O$ record using the Hodell et al. (2023) chronology, several millennial events are not aligned with their corresponding Greenland events. We show this comparison below to

illustrate the rationale for the revised chronology. We therefore consider the chronology published in skaya and Skinner (2026) to provide the most appropriate age model for the present study.

Radionovskaya, S. and Skinner L.C. (2026). A micropalaeontological perspective on changes in bottom-water conditions at the Iberian Margin during the last glacial cycle. *Quaternary Science Reviews*, 392, 110256. https://doi.org/10.1016/j.quascirev.2026.110256

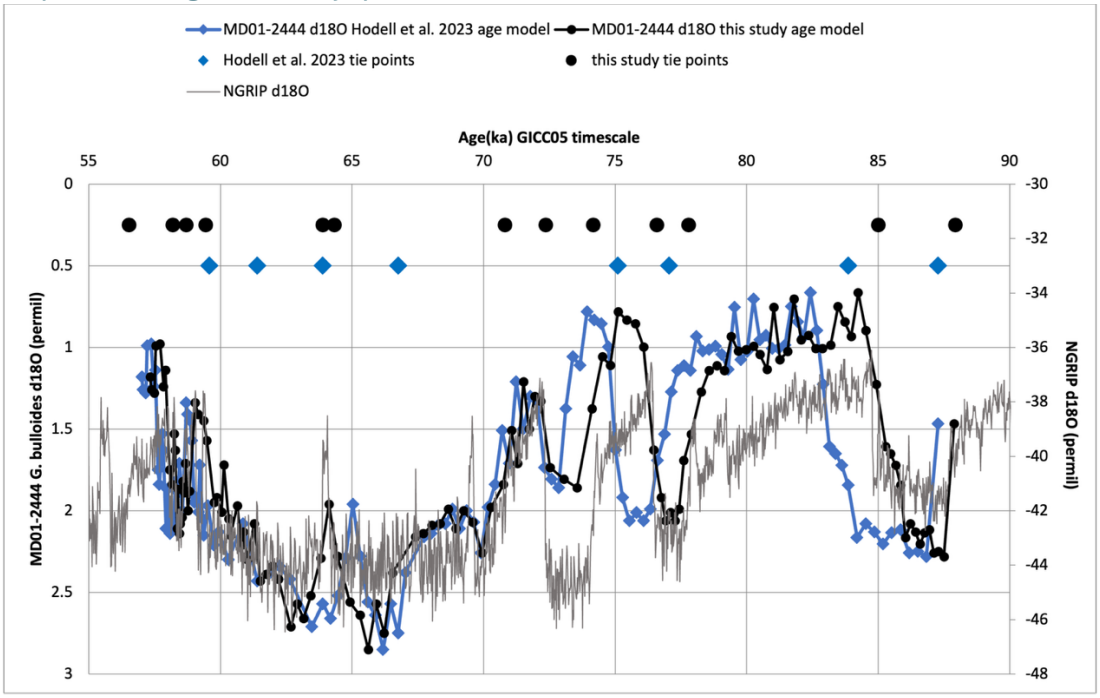


Figure 1. Could you include the other tie points from previously published studies as well? ***Between 65 and 55 ka, MD01-2444 and ODP 1057 and 1055 seem to be poorly aligned. Why would that be the case? Also,*** *to make the plot easier to read I would suggest putting all of the MD01-2444 planktic d18O labels on the same side of the plot (the left side) and coloring the y-axis labels by the same schemes as the time series.*

We thank the reviewer for these helpful comments. We agree that the comparison with previously published tie points is important. However, we feel that including all previously published tie points directly onto Figure 1 would substantially increase the visual complexity of this figure. We therefore propose to retain Figure 1 focused on the chronologies adopted in this study.

The age models for ODP Sites 1055 and 1057 are independently constrained. The lower resolution nature of the data for Site 1057 reduced the visual match but the major events are observable. To account for this, we plotted ODP 1055 planktic $\delta^{18}O$ record as points rather than an interconnected line over the particularly low-resolution interval at ~76-82 ka. In addition, the planktic $\delta^{18}O$ records at these sites do not necessarily capture exactly the signals, and differences in local hydrography, sedimentation rates and resolution of the records can affect the alignment and apparent timing of individual events.

We have revised Figure 1, following reviewer's suggestions and to make age model comparison clearer. The revised figure is provided below.

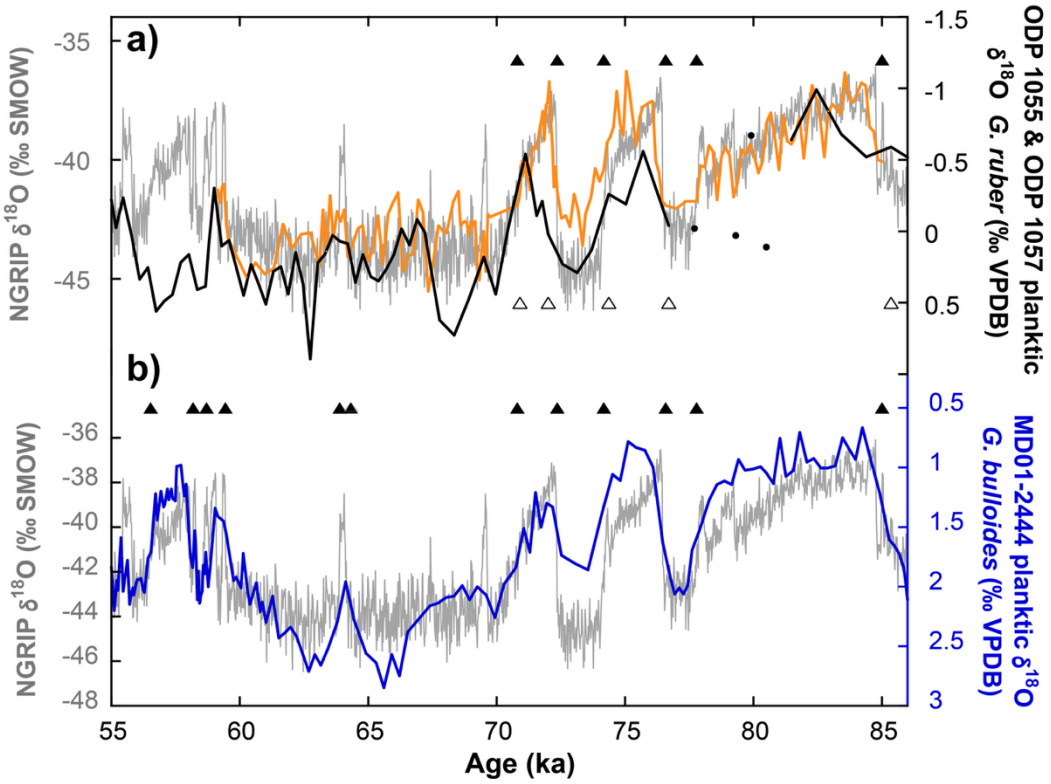


Table 1. Please provide the complete ODP sample codes (Site-Hole-Core-Section-Top Offset) for the tie points, in addition to meters composite depth.

These have now been provided.

Section 3.4. Trace Metal Analysis: You later mention measurements of Al, Mn, and Fe concentration in addition to Ca and Mg. I'm assuming you measured those all by the same method, but you should mention this explicitly. Also please report the associated uncertainty or relative standard deviations of replicates measured for those, if you have them.

All samples were analysed for Ca, Mg, Al, Mn, Fe, amongst other trace metals, simultaneously using ICP-OES. We will clarify this explicitly in section 3.4 of the revised MS.

Analytical reproducibility was assessed using the in-house R4 consistency standard, which was measured after every 10 samples and/or standards. The Mg/Ca analytical uncertainty (±0.041 mmol/mol) and relative standard deviation (RSD 0.79%) reported in lines 190-191 are based on replicate measurements of R4 across all analytical runs used to generate the dataset presented here. For Mn/Ca and Fe/Ca, the corresponding concentrations in the R4 standard are ~0.002 and is ~0.01 mmol/mol, respectively,

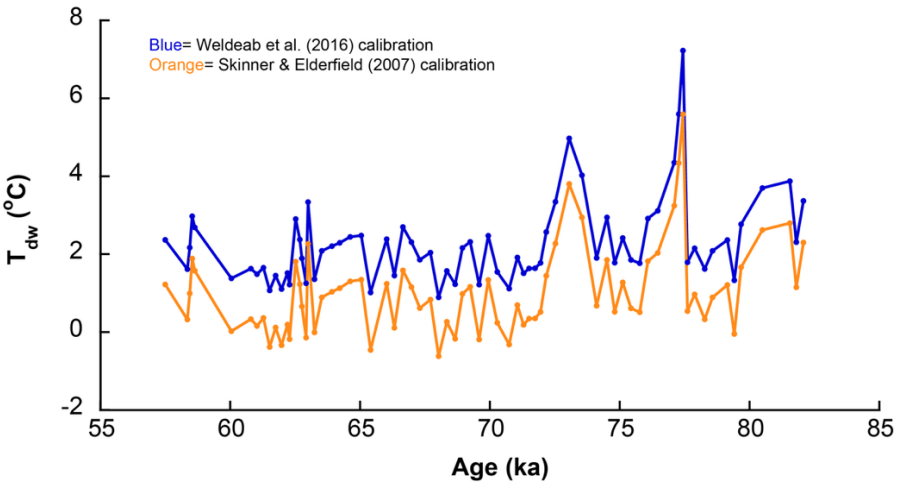
which are close to the detection limit. Consequently, the R4 measurements do not provide a reliable basis for calculating RSDs for these ratios.

Figure 2: Round the values for the equations and the r squared values, and report the p-value.

We will round the values in the equations and r squared values and report the corresponding p-values in the revised MS.

Section 3.6.1. Calibration: You mention two calibrations, but only use one. Could you add some material to the supplement that shows how some of the reported Tdw values change between the calibrations? It Is unclear where the 2% difference comes from. If that is demonstrated in a previous study, you could also cite that.

We thank the reviewer for raising this point. We agree that demonstrating the effect of the two calibrations on the reconstructed T_{dw} values would make the difference clearer. We will add the figure below to the supplement of the revise MS, comparing Tdw reconstructions using the two available calibrations. The stated 2% difference was calculated from the range of Mg/Ca values observed in our dataset. Specifically, for each calibration, we calculated the T_{dw} range corresponding to the minimum and maximum Mg/Ca values measured in this study. We then calculated the difference between the T_{dw} ranges obtained using the two calibrations and expressed this difference as a percentage of the total T_{dw} range. This gives a difference of <2% for both calibrations. We will clarify this calculation in section 3.6.1.



Section 3.6.3. Uncertainty Modelling: This section was difficult to follow. You alternate between referring to sigma as the standard error and the standard deviation. I would prefer if you only used the symbol sigma to refer to standard deviation, and used SE as a short-hand for standard error. Switching between reporting 1 sigma and 2 sigma values is also confusing (such as in Line 264). Since you mostly report 2 sigma, I think you should stick with that in the text. Also please modify Lines 268-270 for clarification. I think you mean that the error bars in subsequent plots represent only the error from the data (with the MC modeling) and not the error from the calibration, but that isn't clear right now.

We are grateful to the reviewer for this helpful comment. We agree that the terminology and distinction between standard deviation and standard error should be made clearer. We will use sigma exclusively to denote standard deviation and SE for standard error throughout the revised MS.

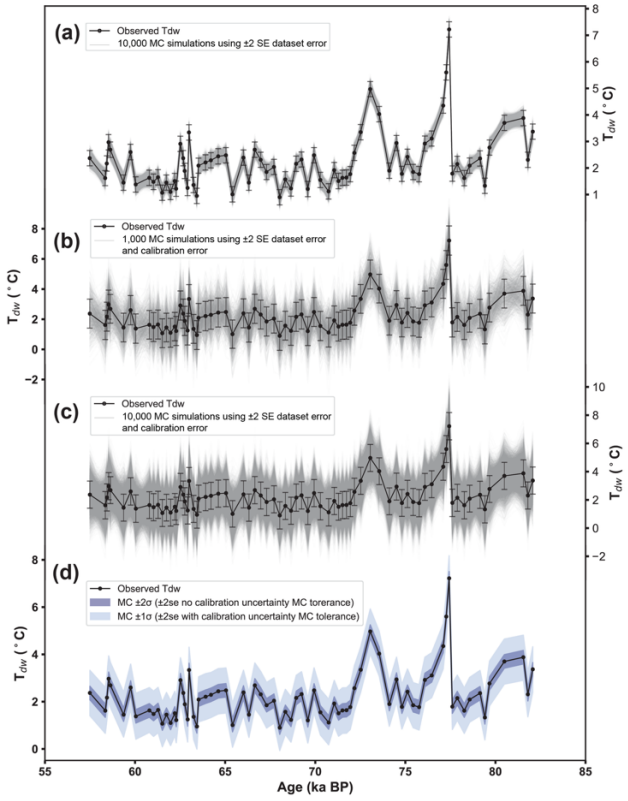
We will revise lines 268-270 to clarify that calibration uncertainty is not propagated into the T_{dw} uncertainty estimates presented in subsequent sections. The revised text will be as follows: "In subsequent sections, calibration uncertainty (error) is not propagated for the Tdw record. Instead, the uncertainty envelopes represent ±2 sigma standard deviation obtained from MC modelling, by modelling only the error of the dataset (using ±2 SE of dataset tolerance for the MC model)."

Figure 3. Add a legend to describe what the error bars and shading are for each plot. I think it is confusing that you use the shading both to show the distribution of MC simulations and the uncertainty. Can you plot the MC simulations as individual points? The figure caption is difficult to understand at the moment, and a legend on the plot would help.

We will move figure 3 into the supplement, following the recommendation of from Reviewer 1.

We agree that the distinction between the distribution of MC simulations and the resulting uncertainty should be clearer. The grey "shading" in the current figure represents the density distribution of 10,000 MC simulations (with each individual one plotted as a very thin light grey line), with the large number of simulations causing the individual simulations to appear visually as a continuous distribution. We will therefore add a separate panel showing 1,000 simulations to make the distribution more readily distinguishable, together with a clear legend explaining the simulations and uncertainty estimates. Please see the revised figure below.

We do not plot all 10,000 simulations as individual points because this would result in substantial overplotting and obscure the underlying distribution. Instead, the revised figure will show individual simulation trajectories for a reduced subset of the simulations (as an extra panel), while retaining the full 10,000 simulations for the uncertainty calculation. We will also revise the figure caption to clearly distinguish the MC simulation distributions from the resulting uncertainty envelope.



Section 3.6.4. d18Odw and d18Odw,anom reconstructions: What is benthic d18Oanom? Can you explain what this is and how It was calculated in the text?

Benthic $\delta^{18}O_{anom}$ is the detrended benthic $\delta^{18}O$, calculated by subtracting 6ka moving average from the measured benthic $\delta^{18}O$. We currently describe this calculation in the caption of Figure 4, but agree that it should also be explained in the main text. We will add a description of the calculation to section 3.6.4 in the revised MS.

Figure 4. Somewhere on the plot or in the figure caption you should indicate that all of these records are from MD01-2444.

We will add as a title to the figure to indicate explicitly that all records shown are from MD01-2444. We will also move this figure to the supplement as suggested by Reviewer 1.

Section 3.7. Sortable Silt: The uncertainty associated with Tdw is carefully considered in the previous section, but the uncertainty associated with the sortable silt measurements is not discussed. Can you provide any estimates of the uncertainty associated with the reported values?

We thank the reviewer for this question. We assessed the uncertainty associated with sortable silt measurements using replicate analyses of each sample. At least 2 replicate measurements were performed for each sample, with a subset of samples analysed 3-5 times. The analytical uncertainty from these replicate measurements has a mean r.s.d. of 1.05%, with a range of 0.03- 3.65 % r.s.d. The full procedural uncertainty, taken here as the pooled standard deviation of the replicate dataset, is

±0.32 um (1 sigma). We will add these uncertainty estimates to section 3.7 in the revised MS.

Figure 5. Is the decrease in the running correlation around 60-65 ka due to the presence of IRD during HS6? Why do you think the two methods disagree between 70-75 ka? It looks like a sampling resolution bias to me.

We thank the reviewer for these observations. The observed decrease in running correlation is at approx. ~67-63.5 ka, whereas HS 6 occurs at ~59.5-64 ka. The decrease therefore precedes the main time interval spanning HS 6, and we do not believe the presence of IRD during HS 6 to be the primary explanation. Regardless of the precise cause for this decrease, we do not observe major changes in mean SS but nevertheless, interpret this interval with caution (lines 339-341).

We agree that the difference between %SS and mean SS at approx. ~70-75 ka is likely related to sampling resolution. We analysed %SS for only a subset of the samples for mean SS, resulting in a lower-resolution record. Nevertheless, the pronounced % SS peak at ~74 ka is also expressed in mean SS record, indicating broad agreement between the two records.

Section 4. Results: You mention comparisons to the Holocene here and elsewhere, but where is the Holocene data? Could you include that comparison in a plot, or mention specific temperatures?

We thank the reviewer for this comment. By Holocene in the original MS, we refer to the modern/ Holocene T_{dw} values. We use a value of ~3°C from CTD measurements obtained during JC089 cruise (Hodell et al., 2014) as representative of modern deep-water temperature. This is broadly consistent with the two early Holocene *G. affinis* Mg/Ca measurements analysed by Skinner et al. (2003) from nearby Iberian Margin site MD99-2334K (3166 m), which yielded T_{dw} values of ~3.5 and 2.8 °C, using the calibration of Weldeab et al. (2016). We therefore assume that Holocene T_{dw} was relatively similar to modern Td_w. An important caveat is the uncertainty associated with calibrations, which could have particularly large effects on reconstructed absolute values (section 3.6.1). Therefore, we use the modern measured value of 3°C for our "Holocene" estimate. The 3°C reference value is currently given in the caption of Figure 6 and shown as a black dashed line in Figure 6e. We will add this explaining to the main text in the revised MS.

Figure 6. Do you comment on why the Tdw signal is so much smaller in HS6? How does it compare to the magnitude of the changes across HS during deglaciations?

We thank the reviewer for this interesting question. We agree that the relatively small T_{dw} response during HS 6 warrants further discussion. The coldest T_{dw} values during HS 6 are comparable to those observed during the colder intervals of preceding MIS 4, and there is no obvious T_{dw} minimum observed during HS 6 in our data. The range of T_{dw} values during HS 6 is ~1.1- 2.7°C, a range also observed during colder MIS 4 intervals. This behaviour is consistent with previous Mg/Ca measurements of *G. affinis* from the

same core. Skinner et al. (2007) reconstructed T_{dw} values of ~1-2.6°C during HS 4 and ~1.1- 2.8°C for HS 5 using the calibration of Weldeab et al. (2016), a range consistent with HS 6. Additionally, they showed that HS 4 and HS 5 were not substantially colder than other stadials during MIS 3. Similarly, Skinner et al. (2003) found that HS 1 at a deeper Iberian Margin site was not markedly colder than the LGM, with the two intervals showing broadly similar T_{dw} values. These observations suggest that HSs do not necessarily produce an exceptionally large cooling signal at ~2.5-3.2 km depth at the Iberian Margin.

Direct comparison of the magnitude of T_{dw} changes across HSs during the deglaciations is nevertheless limited by the availability of comparable records. The record of Skinner et al. (2003) is from a deeper site than MD01-2444 and therefore has a different background hydrographic setting and likely different T_{dw}, as discussed in section 5.2. Moreover, their HS 1 and LGM values are similar to each other thus a meaningful deglacial HS change is difficult to define.

We will add discussion of these comparisons to section 5.3 of the revised MS, including the similarity between HS 6, HS 4, HS 5, and LGM vs HS 1 and MIS 4 vs HS 6.

Section 6: Conclusions: A schematic figure of the three circulation regimes would be a great addition to this paper.

We agree this figure summarising the three circulation regimes would be useful We will add such a figure to the revised MS.

Additional line-by-line comments:

We are very grateful to the reviewer for all the additional line-by-line comments. We address these below.

5-10: Using the phrase 'is key' twice, consider rephrasing one of the sentences

We will replace the first "key" with "essential".

16: Write out 'sortable silt' instead of using the abbreviation here.

We will add "Sortable silt" here.

20-25: What is millennial-type variability? This may be too vague for the abstract. Could you be more specific about the type of variability?

We will revise this to read: "We also find evidence of previously unreported "millennial-type" (i.e. short-timescale) variability during glacial MIS 4 and, similar to recent studies for MIS 2, a persistent contribution of northern sourced waters during MIS 4 (pre- HS 6)."

35-100: This section provides a good overview of the prior work on North Atlantic Ocean circulation from the LGM, but doesn't go into any detail about prior work that has been

done using the sortable silt proxy. I think it is worth including a bit about this in the introduction, since it is a large part of your study.

We already discuss previous SS studies spanning MIS 4 (Hall and McCave, 2000; Thornalley et al., 2013). In the revised MS, we will expand this discussion to include LGM sortable silt studies, including McCave et al. (1995) and Evans and Hall (2008), as well as the HS sortable silt studies of Hoogakker et al. (2007) and Hoffmann et al. (2019).

109-110: Eastern North Atlantic Deep Water (ENADW) or Northeast Atlantic Deep Water (NEADW)? You mix the initialisms and the names here.

We thank the reviewer for pointing this out. We will relabel this water mass as Northeast Atlantic Deep Water, NEADW, in the revised MS.

138-140: Please write out the full name of GICC05, before using the short-hand.

We will add this to the revised MS.

142: ODP should be written out as Ocean Drilling Program upon first occurrence in the paper. I think there typically is no hyphen between ODP and the Site name, so ODP-1055 should be ODP 1055 or ODP Site 1055.

We will revise this throughout the text in the revised MS.

158-159: What Is the sedimentation rate over your interval in MD01-2444 according to your age model? This is the more relevant sedimentation rate for this study.

The mean sedimentation rate for ~50-85 ka interval of core MD01-2444 is ~15 cm/ka. We will add this to the revised MS.

161: Please write out Globobulimina affinis upon first mention in your paper.

We will add this.

172: wuellerstorfi is spelled incorrectly here.

We will correct this.

189-191: The structure of this sentence is confusing.

We will revise this sentence to the following: "Short term instrument precision for Mg/Ca measurements was assessed using the in-house R4 consistency standard, which has a Mg/Ca ratio within the range typical of foraminiferal samples. Across all analytical runs, the R4 standard yielded an analytical uncertainty of ±0.041 mmol/mol and a precision of 0.79% r.s.d."

195: I suggest removing '(i.e. inefficient cleaning)'.

We will remove “inefficient cleaning” from this sentence in the revised MS.

196: Should this just be ‘coating’ rather than ‘carbonate coating’?

Yes. We will remove “carbonate”.

365: The absence of any signal in d13C outside of HS6 is interesting. Do you have any ideas about why that might be the case?

We observe a decrease in $\delta^{13}C$ beginning at the onset of MIS 4, with lowest values occurring during early HS 6, followed by a recovery during late HS 6 (Fig. 6a). In this sentence, we meant that we only observe low $\delta^{13}C$ for the first half of HS 6 (and not the latter HS 6). We interpret the “recovery” in the latter half of HS 6 as evidence for two-phase structure of HS 6, as discussed in section 5.2. We will clarify this sentence in the revised MS.

383: Should this be ‘southern-sourced water (SSW)’? You refer to SSW in the next line.

We agree this is confusing. We did mean LDW here, we will revise this to “the boundary of southern-source water (i.e. LDW) and northern sourced NEADW.”

410-415: You switch between 2 sigma and 1 sigma here. Stick with 1 or 2 for both, otherwise it is confusing for the reader. If 2 sigma is 2 degrees C and the difference between Tdw is 3-5 degrees C, I wouldn’t say that is a ‘much larger’ difference, maybe just ‘larger’.

We agree this is confusing. We will revise this in the revised MS.

445-446: Including the quotes around ‘stagnation warming’ and ‘collapsed’ makes it seem like you are referencing some other text. If you are, include the reference at the end of this sentence. If you aren’t, take the quotes out.

We agree that the quotation marks around “stagnation warming” are unnecessary and will remove them. However, we used quotation marks around “collapsed” because it is likely that AMOC was substantially weaker rather than completely collapsed, as suggested by recent studies. We will therefore retain the quotation marks around “collapsed” to indicate that we not mean a complete cessation of AMOC.

477: Is ‘glacial NADW’ a typo? Should it just be ‘NADW’?

We use “glacial NADW” here to distinguish it from NADW formed during interglacial periods, following previous studies (e.g. Blaser et al., 2025; Wharton et al., 2026). We will therefore retain the term “glacial NADW”.

563-564: Can you clarify what d18Oanom is in the methods? I get what d18Odw,anom is, but what is d18Oanom?

Yes, we will add this to the methods section 3.6.4.

570: How does benthic d13C compare between the LGM and MIS 4?

C. wuellerstorfi $\delta^{18}O$ and $\delta^{13}C$ from site MD01-2444 spanning MIS 4 have previously been published by Hodell et al. (2023) and Martrat et al. (2007). Figure 6 of Hodell et al. (2023) provides a direct comparison of the LGM and MIS 4 benthic $\delta^{13}C$, showing comparatively low values during both periods, with HS 6 exhibiting slightly lower values than HS 1. We will add text discussing this comparison to section 5.2 of the revised MS.

616: You say 'intermittent to SSW', should there be some other word after 'to'?

We meant to say "intermittent SSW" here. We will remove "to".

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