Summary

We would like to thank the reviewers for their critical but constructive comments, which will be addressed in a revised manuscript.

Reviewer #1

(1.1) The premise on the ice sheet melting is not really dealt with - I was expecting the authors to get back to it at the end and show how their new records might link to it.

A similar comment was made by Reviewer 2 (see Point 2.5 below), so it would seem readers expected us to use our results to discuss implications for ice sheet response or at least ice shelf basal melting in more detail. This was not the original intention – our aim was to compile existing temperature records that are potentially suitable for estimating CDW temperature changes (Lines 62-68). The results would then be used by others, as a transient boundary condition for ice shelf basal melt parameterisations (for example with PICO, Reese et al., 2018), or for validating alternative estimates (e.g., a glacial index, Sutter et al., 2019; or linear response function, Albrecht et al., 2020). In the Introduction section of the revised paper we will make the objectives and scope clearer, while also adding further discussion in Section 5 as follows.

In Section 5.5 ('Recommendations for use as a boundary condition or for model validation") we already discuss some implications/limitations of our results from a modelling perspective, but this section can be expanded. We could add more discussion about the link to ice sheet modelling and particularly (i) the recent use of the N. Atlantic ODP 980 NADW temperatures as a boundary condition in some previous Antarctic Ice Sheet modelling (Quiquet et al., 2018; Crotti et al., 2022) and (ii) a persistent lack of independent validation of ocean temp estimates in past AIS modelling studies over glacial cycle time scales (de Boer et al., 2014; Tigchelaar et al., 2018; Albrecht et al., 2020; Sutter et al., 2022). This study could provide such validation – as we have done for our recent ice sheet modelling study with PISM (Chandler et al., in review) – at least at multi-millennial time scales.

As there are no ice sheet simulations in this study, we cannot use our results to estimate changes in shelf basal melting directly. However, calculation of changes in thermal forcing for some example ice shelves in Antarctica provides some interesting insights that can be discussed, as follows.

Here we consider the Wilkes and Amundsen Sea regions, which have both been considered susceptible to marine ice sheet instability in the future or in past interglacials. The thermal forcing plotted in Fig. 1.1 below, is:

$T_F = T_0 + \Delta T_{CDW} - T_{PMP}$,

where T_0 is the regional-average present-day ocean bottom water temperature at 500-800 m depth; ΔT_{CDW} is the CDW temperature anomaly we have calculated in this study, and T_{PMP} is the pressure melting point, taken here as -1.8 degC. When T_F is positive, ice shelf basal melt rates increase as temperature increases. When T_F is negative there is no basal melting. Present-day temperatures for each region are extracted from the World Ocean Atlas (Locarnini et al., 2018) and closely match previous estimates by Schmidtko et al. (2014) for both regions. Although transferring CDW temperature anomalies directly to thermal forcing at grounding lines is very simplistic representation of how ocean warming might translate to sub-shelf melting (as alluded to in Lines 354-359), this approach has some precedent (Sutter et al., 2019; Albrecht et al., 2020) and illustrates important regional differences in likely ice shelf susceptibility to interglacial warming.

Even with our wide uncertainty envelope, we find T_F in the Wilkes region is significantly negative for extended periods during glacials, and ambiguous (T_F = 0 lies within the error envelope) during most interglacials. In contrast, thermal forcing in the Amundsen Sea region is ambiguous through all the glacials and significantly positive in interglacials 19, 11, 9, 7, 5 and 1. Overall this highlights how we really need to reduce uncertainties in reconstructed CDW temperature – not only during interglacials, but also during glacials. Importance of the latter is illustrated by the Amundsen Region: here the LGM is often used as a spin-up or initial state for ice sheet models, yet the ocean temperature uncertainty envelope encompasses both $T_F < 0$ degC (negligible melt) and T_F close to 1 degC (substantial melt). During model tuning and optimisation these two scenarios would likely lead to quite different modelling choices, in particular relating to parameterisations of calving and sub-shelf melting, which in turn would impact simulated responses to future warming. The same problem affects the penultimate glacial maximum (PGM; MIS 6), with uncertainties in sub-shelf melt and ice volume during the PGM in turn feeding into LIG sea-level estimates (Dendy et al., 2017).

We also note the coarse time scale (4 kyr) likely misses short-term warming events and will dampen interglacial peaks. As we discuss below in Point 1.2, this cannot easily be remedied by adding additional d18O sites – it would require either additional Mg/Ca records or application of a novel proxy (perhaps clumped isotopes).

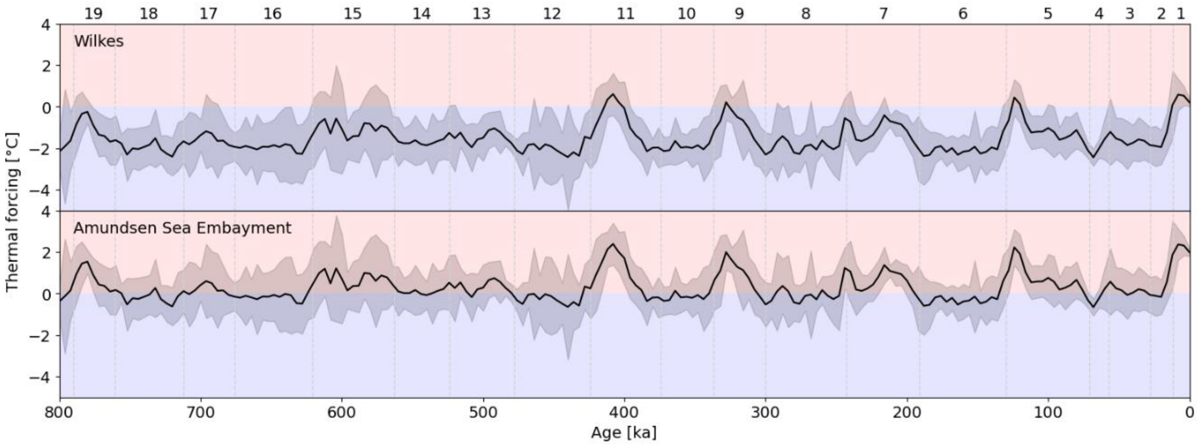

Figure 1.1. Thermal forcing in two regions considered susceptible to marine ice sheet instability: Wilkes (Cook, Ninnis, Mertz ice shelves) and Amundsen (Pine Island, Thwaites, Dotson ice shelves). In both regions, ice shelf melt is currently driven by incursion of CDW onto the continental shelf.

(1.2) Furthermore it is unclear to me why they focussed on the last 800 ka - they didn't really try to interpret anything older than 400 ka for the interglacials. Why only 7 records - there are others in the literature for LCDW already published that go back to ~ 800 ka e.g. ODP 1168, ODP 1170, ODP 1171 from the South Tasman Rise, south of Tasmania (Nurnberg et al., 2004). I would suggest the authors look at the new dataset of d18O recently published by Mulitza et al., 2022 ESSD, which would have many more records covering the last 400 and even 800 ka. Given the lack of records that go back 800 ka why not focus on the shorter time periods – especially when there are no periods warmer than present between MIS11 and MIS19. They compare to several other datasets that do not cover the last 800 ka in their discussion.

<u>Study period</u>

This was chosen for several reasons, noted briefly at Lines 65-67 (we can make this clearer), as follows. (i) We focus on the period dominated by 100 kyr glacial cycles after the MPT, which is the climate state best characterising our present-day interglacial - albeit before the very significant anthropogenic influence. (ii) To match the longest Antarctic ice core record (EPICA Dome C). (iii) Importantly, we find added interest of including colder interglacials (rather than starting from MIS 11), since we can build a clearer picture of Earth system response to warming by including cooler as well as warmer interglacials. We will emphasize this more throughout the text.

<u>Additional records</u>

As a synthesis paper, our original scope was to compile records for which bottom water temperatures had already been published. There are of course far more benthic d18O records, than the few analysed by Bates et al. (2014) – as evidenced by the recent efforts of Mulitza et al. (2022) and previously by the common use of benthic d18O in establishing age models. We were originally hesitant to include these d18O records without published bottom water temperatures for several reasons, but agree with the reviewer that it might be useful to perform a more full analysis.

Our main concern relates to the method to convert benthic foraminiferal d18O (d18Ob) to BWT, which relies on establishing site-specific transfer functions between sea-level and d18Ob (Siddall et al., 2010; Bates et al., 2014). The transfer functions are then used to separate the two main influences on d18Ob in the paleotemperature equation (sea water d18Osw, which is closely related to ice sheet ice volume and thus sea-level; and the ambient seawater temperature during the growth phase). The transfer functions are linear piece-wise functions established using "calibration windows" – typically full glacial or interglacial conditions – for which both sea-level and d18Ob data are available. Crucially, Bates et al. note that "*The calibration windows for sea level are chosen as prolonged interstadial or stadial events, when sea level and temperature are at approximate equilibrium*" and caution that the method is not suitable during glacial inceptions and terminations – as we have noted in Lines 307-312. Unfortunately, the interglacials are likely too short for ice sheets to reach approximate equilibrium with climate, as this would require tens of thousands of years of constant temperature and sea-level (for example the Antarctic Ice Sheet: see Garbe et al., 2020). In contrast, deep ocean temperature responds to surface climate change at centennial time scales (Yang et al., 2011; Li et al., 2013). Hence, the ambient temperature signal in d18Ob might respond to global climatic changes over time scales of order 0.1 kyr, while the ice volume (d18Osw) signal likely responds over time scales of 10 kyr, potentially biasing reconstructions with this method during the rapid climate changes encountered through interglacials. Adding extra sites based on d18O can help improve the signal to noise ratio at our current 4 kyr temporal resolution, and can help by targeting a more geographically relevant region, but we would essentially be reinforcing an underlying bias during interglacials (which are often the periods of most interest). This same limitation also prevents us from justifying an increased temporal resolution even if there are sufficient data to do so from a statistical aspect. Consequently, these extra Southern Ocean sites could certainly benefit a 800 kyr coarse resolution synthesis, but we would not gain much useful information by attempting a higher resolution synthesis over a shorter period e.g. since MIS 11.

Bearing in mind the potential pros and cons of analysing additional d18O records, we have carefully reviewed ca. 130 benthic d18O records south of 40 degS in the Mulitza database. For our purposes, suitable records need to cover at least one complete glacial cycle to enable a transfer function to be established. They also need to represent a suitable water mass (ideally lower CDW). If we also include a few additional ODP sites seemingly missed from that database we find ~28 suitable d18O records, of which only one (ODP 1090) was included in our original synthesis. To estimate temperatures at these sites we would have to establish transfer functions at each site, for which we could use more recent sea-level estimates than those used by Bates et al. (2014; their Appendix A). Evidently the selected sea-level estimates should be independent of global benthic d18O.

We would need to note that a synthesis with many d18O sites and only two Mg/Ca sites (assuming we exclude the North Atlantic sites), will be heavily biased towards the d18O method.

For the two Southern Ocean sites with Mg/Ca (and potentially the Atlantic sites M16672 and Chain 82-24-4PC), calculation of BWT using both Mg/Ca and d18O would provide an interesting comparison.

In a revised manuscript, we plan to first analyse the five suitable d18O sites that extend through the full study period from 800 ka to present (MD02-2588; ODP1090,1094,1123; PC493) as this subset represents all three main Southern Ocean basins and includes the two sites with long Mg/Ca records (ODP 1094,1123). The subset also includes the valuable Antarctic continental margin site PC493. Further details are provided in Table 1 below. We could also analyse an additional five sites with data from MIS 11 onwards (FR1/94-GC3, MD07-3076&3077; ODP1168, 1170; PS75-059-2). Discussion of results could then focus on (1) the full 800 ka period based on the seven long records; (2) extra interesting details, if any, that emerge in the shorter period (after 424 ka) with twelve records; (3)

comparison between the two proxies, which is limited as before by the small number of sites with Mg/Ca; and (4) comparison with selected North Atlantic records including ODP 980 used for AIS modelling by Quiquet et al. / Crotti et al., and the equatorial Mg/Ca record M16772 for NADW used in our original synthesis.

The limitations of the d18O method as described above will also be explained in more detail in the Methods section.

Table 1: Suggested list of sites to include. Sites in **bold** cover the full 800 ka study period, and sites in normal type at least the period since the start of MIS 11 (424 ka).

Site	Lat/Lon/Depth [deg, m]	Max age [ka]	Basin	Proxy	Hydrography	Proxy ref
MD02-2588	-41.2, 25.5, 2905	1656	IND	d18O (Cbw)	NADW (Hall et al., 2018).	Starr et al. (2021)
ODP 1090 & TTN057-6-PC4	-42.9, 8.9, 3702	2903	ATL	d18O (Cbw)	LCDW/NADW (Hodell et al., 2003). (Glacial GNADW? Howe et al., 2016).	Hodell et al. (2003)
ODP 1094	-53.2, 5.1, 2807	1557	ATL	d18O (Cbs) Mg/Ca (Mp)	LCDW (Hasenfratz et al. 2019). (Glacial GNADW? Howe et al., 2016).	Hasenfratz et al. (2019)
ODP 1123	-41.8, -171.5, 3290	1546	PAC	d18O (Us) Mg/Ca (Us)	LCDW (McCave et al., 2008).	Elderfield et al. (2012)
PC493	-71.1, -119.9, 2077	800	PAC	d18O (Cbw)	LCDW (Williams et al., 2019)	Williams et al. (2019)
FR1/94-GC3	-44.3, 150.0, 2667	454	PAC	d18O (Cbw)	LCDW (Moy et al., 2008; Struve et al., 2022).	DeDeckker et al. (2018)
MD07-3076 & 3077	-44.2, -14.2, 3777	440	ATL	d18O (Cbs & Us)	LCDW (Gottschalk et al., 2016) (Glacial GAABW? Howe et al., 2016).	Gottschalk et al. (2016, 2019)
ODP 1168	-42.6, 144.4, 2463	500	IND	d18O (Cbw)	LCDW (Moy et al., 2008; Struve et al., 2022).	Nürnberg et al. (2004)
ODP 1170	-47.2, 146.1, -2704	460	IND	d18O (Cbw)	LCDW (Moy et al., 2008; Struve et al., 2022).	Nürnberg et al. (2004)
PS75-059-2	-54.2, -125.4, 3613	480	PAC	d18O (Cbs)	LCDW (Ullerman et al., 2008).	Ullerman et al. (2016).

Ocean basins: ATL, IND, PAC Atlantic, Indian, Pacific. Foraminifera taxa: Cbs Cibicdoides spp.; Cbw Cibicidoides wuellerstorfi; Cs Cibicides spp.; Mp Melonis. pompiloides; Us Uvigerina spp.

(1.3) Additionally, the new compilation shows strong agreement to past DWT compilations - so I'm not entirely sure that is really adds much to the literature. I was really hoping it would get into how it might influence the ice sheets or modelling so that it did add something new.

It is true that our synthesis agrees closely with the Rohling et al. (2021) global mean DWT reconstruction, when their reconstruction has been resampled to match our 4 kyr resolution (see our original Fig 3). Note there was poorer agreement with the Shakun et al. (2015) global DWT (which showed consistently warmer interglacials). Good agreement with Rohling et al was not an inevitable outcome, given the different geographic focus, and we view this as an interesting result rather than a weakness. We would also note the agreement is strong at the coarse 4 kyr resolution that we used, but as more Mg/Ca data become available and a higher resolution synthesis is feasible, it is of course possible that periods with weaker agreement will emerge.

Regarding the influence on ice sheets or modelling, please see our response to Point 1.1 above.

(1.4) I also find the inclusion of the NADW records (4 out of the total of 7) a little odd given the focus on the LCDW and evidence in the literature of the shut down of AMOC and therefore I would not expect the NADW to be a strong contributor to LCDW during the glacials. The authors came to this eventually in the paper, but only after putting it all together and didn't really discuss the implications on their records which have 4 records from NADW that may well swamp the LCDW datasets. Although I realise the authors are interested in the interglacials for the ice sheet melting. But then didn't really come back to this and how the MIS5, 9 and 11 may have impacted the ice sheets.

It is indeed unintuitive to include NADW (particularly from N Atlantic sites) - this decision was originally motivated by the use of the North Atlantic ODP 980 BWT anomaly directly as an ocean temperature boundary condition in Antarctic Ice Sheet modelling (Quiquet et al. / Crotti et al.). We need to be clearer about this point in the Introduction and/or Section 2. In our original version we already discussed this issue of whether NADW records should be used, and compared the records for CDW and NADW, which it seems are well correlated – there is actually better agreement between the NADW and CDW subsets of records, than between the d18O and Mg/Ca subsets (original Fig 4). The scope of that comparison is rather limited by the low number of respective sites.

We are aware that sites currently bathed in NADW could have become bathed in a southern-sourced water mass (SSW) during glacial stages. However, northbound AABW (interglacials) or SSW (glacials) both eventually recirculate to mix with southbound NADW (interglacials) or GNAIW/GNADW (glacials). See Fig 1.4 below, with schematics copied from Howe et al. (2016) and Matsumoto (2017), as well as Ferrari et al. (2014). Assuming the Atlantic remains a relatively important heat source for CDW, temperature changes at sites intermittently bathed in deep northbound water masses remain relevant even during glacial periods, and should still reflect changes in CDW temperature albeit with more delay than for southbound water masses. The extra delay is not a critical issue at 4 kyr resolution. Nevertheless, it would be good to avoid reliance on such arguments.

Following Point 1.2 above, in the revised paper we aim to restrict our synthesis to records south of 40degS, by analysing (or re-analysing) the d18O data in some selected cores (Table 1). However, because (i) ODP 980 has recently been used by the ice sheet modelling community, (ii) data from the North Atlantic could help boost the sparse data in the Southern Ocean, and (iii) there is potential interest in the changing contribution of NADW to CDW, we prefer to keep some discussion and comparison of the North and South Atlantic NADW records even if they are excluded from the main synthesis.

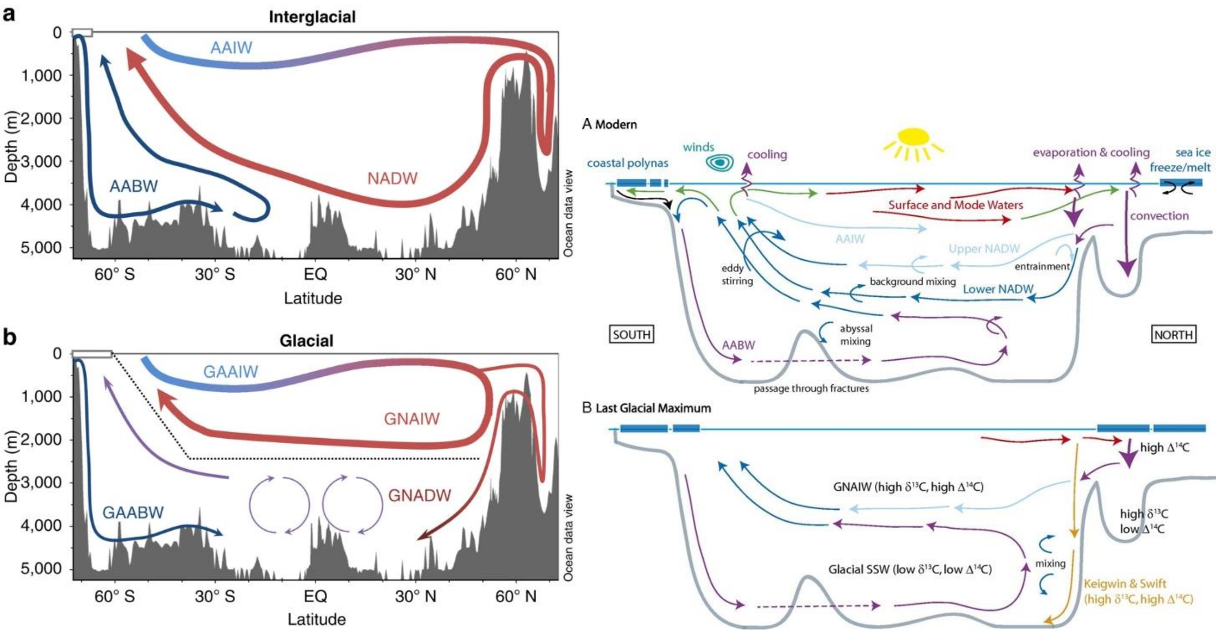

Figure 1.4. Two schematics comparing modern and glacial ocean circulation patterns in the Atlantic. The left hand pair is copied from Howe et al. (2016) and is based mainly on ε_{Nd}. The right hand pair is copied from Matsumoto (2017), and is largely based on carbon isotopic analysis by Keigwin and Swift (2017). Throughout a glacial cycle, bottom water in the North Atlantic (south of approx. 60N, and below ~2000 m) eventually mixes with water masses upwelling in the Southern Ocean (CDW, or a glacial equivalent) contain contributions from southbound NADW/GNAIW in the upper overturning cell as well as AABW returning south in the lower cell. Therefore, temperature changes in NADW/GNAIW as well as AABW should all eventually influence the temperature of upwelling CDW.

(1.5) Many sections there was insufficient detail on the methods and the issues – they came back to many of these in the discussion at the end – but they should have been upfront. There really was insufficient detail around the methods used for the Mg/Ca and the d18O. I realise these are in the original papers – but you need to provide a summary here so the reader doesn't have to go back to the original papers.

We are happy to add more details on both proxies used (d18O and Mg/Ca) and will move information on their limitations from the Discussion to the Methods (Section 3). In particular we will include some issues related to the d18O proxy (Point 1.2 above) and carbonate dissolution (Point 1.8 below).

(1.6) I felt the oceanography background needed more information and detail on the CDW – and differentiate between the NADW, LCDW, UCDW and mCDW – it would have helped to have a 3D/Depth figure to show the links between these water masses.

Rather than a 3D figure could we suggest complementing Fig 1 with vertical sections of the Atlantic sector and SW Pacific, showing key circulation features, as used in many similar studies (similar to the examples copied above for Point 1.4). We can also refer to original definitions of these water masses.

(1.7) It was unclear to me how the authors plotted up Figure 4 when you can't look at BWT on both the NADW and the LCDW at the same time? How do you make plots like this? Also which cores had both benthic d18O and the Mg/Ca done on them to make the other X-Y plot.

Fig 4a in the manuscript compares the averages of BWTs reconstructed from all cores with Mg/Ca (Chain 82-24-4PC, M16772, ODP 1094, ODP 1123), to the averages of BWTs calculated from all cores with d18O (DSDP 607, ODP 980, ODP 1090). Each point represents one 4 kyr time slice. There were no cores with both proxies.
Similarly for Fig 4b there is one point for each time slice. NADW BWT anomaly is averaged over cores currently bathed in NADW (Chain 82-24-4PC, DSDP 607, M16772, ODP 980, ODP 1090). CDW BWT anomaly is averaged over cores currently bathed in CDW (ODP 1094, ODP 1123).
This will be clarified in the caption.

(1.8) The reason why there are not many DWT records using Mg/Ca for benthic foraminifera is that it is not a trivial thing to do. Firstly there are not always sufficient tests of the right species of benthic foraminifera. Secondly the method is time consuming and has some issues – some of which were outlined in the limitations. But one thing that has not been mentioned in the paper is the potential impact of dissolution and the Carbonate Compensation Depth - the LCDW of the Southern sits at or very close to the Carbonate Saturation Horizon which is around 3000 m and the CCD which sits around 4000 m- so not many cores actually preserve sufficient Carbonate organisms or the records can be compromised by period of dissolution due to the shoaling of the CCD - during the glacial cycle.
Yes the preservation issue was mentioned only very briefly (calcite dissolution) and not specifically linked to CCD, which is indeed an important limitation on where samples can be collected. This aspect will be discussed in more detail and we will refer to original studies to check for reports of carbonate dissolution at each site.
We will also move details of the limitations from the Discussion to the Methods.

Reviewer #2

Chandler and Langebroek compiled 7 deep water records (3 benthic Mg/Ca, 1 ostracod Mg/Ca, and 3 benthic d18O) during the last 800 ka, aiming to understand the glacial-interglacial temperature variability of Circumpolar Deep Water. However, there are several problems with data selection, data interpretation, and presentation, I have to say that this study, at least in the current form, is not to the standard of the CP.
Here are my main concerns.

(2.1) The premise of using a compilation of BWT at selected sites to reflect CDW BWT change. Including NADW sites in the compilation is not appropriate because 1) NADW sites could be bathed in different water masses during glacials and interglacials due to circulation change, as already mentioned by the authors in Section 5.3, 2) shallow NADW sites (e.g. ODP 980) would have minimal influence on CDW that can potentially upwell underneath the Antarctic Ice shelves, due to the lower seawater density. On the contrary, Pacific sites at relatively deep depths, though downstream of CDW, may more reliably record the CDW temperature as those could be on the same isopycnal as CDW upwelling underneath the Antarctic Ice shelves. More benthic d18O records from the Pacific included in Bates et al (2014) thus might be included in the compilation as well. But ultimately, it is not most convincing to include sites not bathed in CDW to infer CDW changes. There may be more SO sites available if the time span of the compilation can be shortened.

As we have noted in response to Reviewer #1 (Point 1.4 above), the use of NADW is certainly not intuitive and was originally motivated by the use of ODP 980 directly as an ocean temperature boundary condition in some recent Antarctic Ice Sheet simulations (Quiquet et al., 2018; Crotti et al., 2022). It does seem that deep water masses in both the N and S Atlantic have contributed to upwelling CDW in the Southern Ocean through glacial as well as interglacial climates, just less directly in the latter case (see Point 1.4 and copied schematics above). However, it would clearly be better to not include the N. Atlantic records.

In our revised synthesis we intend to analyse additional d18O records from the Southern Ocean, including some ‘downstream’ locations in the SW Pacific (Table 1 above). It was not our original intention to analyse additional records – but it will greatly help – please see our more detailed response to Point 1.3, as there are several restrictions limiting the number of suitable sites.

(2.2) Using a compiled CDW T to inform ice shelf melting triggered by CDW.

The authors set out to use a compiled CDW T record to infer potential ice shelf melting triggered by CDW during previous warm interglacials. It is noted that such an event can be triggered by ~1 degC warming in a short period (a year) in the modern ocean (e.g., Jenkins et al., 2018), which is a really small difference challenging for any given record reconstructed by any proxy to resolve with confidence, let alone a compilation smoothing multiple records. So the 0.1-0.5degC warming during previous interglacials mentioned in the abstract must be interpreted in the context of the uncertainty range, which is not reported but can be expected to be large enough to make the reported warming statistically insignificant.

Here the comment “*The authors set out to use a compiled CDW T record to infer potential ice shelf melting triggered by CDW during previous warm interglacials*” was beyond our original stated intention of compiling a temperature synthesis (Lines 63-64). We had not intended to use our synthesis to estimate changes in ice shelf melting. However, it seems that this was not clear, and we will add some related discussion (see Point 1.1 above).

It is true that the strong sensitivity of basal melt to temperature changes of 1 degC is a challenge for using proxy records – or indeed any other existing method – for estimating basal melting. We already admit the 4 kyr resolution record is too coarse and too uncertain, at present, to be used directly, and that is why we recommend its use for validation of alternatives rather than directly as a boundary condition (Lines 347-353). The high uncertainty is not in itself a reason to not attempt this compilation, and one of our aims of this study is to highlight how the uncertainty is still high, in the hope this stimulates community interest in acquiring more data. Of course, we acknowledge that will not be an easy task! Owing to the problems highlighted above with converting benthic d18O to BWT we can suggest efforts should be directed towards additional Mg/Ca records, or to clumped isotopes which may be feasible in the Southern Ocean and at relevant temperatures (Peral et al., 2018; Leutert et al. 2021), but have not yet been applied at time scales relevant to this study. Regional averages over several sites remain the best way at present to overcome substantial uncertainties at individual sites.

Regarding *Warming in the interglacials is not statistically significant and should have been reported with uncertainty intervals in the abstract:* Since we intend to add more records, we hope that the error bounds will be reduced. Either way we will report error bounds more consistently.

Finally, the short time scales: it seems very unlikely that we will be able to reconstruct annual or decadal temperature changes, over such a long time period. This is a potential issue when for example transition from one state to another is influenced by stochastic noise as well as longer term changes in climate forcing (e.g., Niu et al. 2019). We suspect the best approach will be to use observations, such as those from Jenkins et al. (2018) and several others, to characterise short term variability as noise that can be superimposed on the millennial-scale ocean temperature changes. A similar approach is used for surface climate forcing in ice sheet models. We can add this point to the discussion.

(2.3) Hydrographic settings of CDW.

As a manuscript on CDW, there are lots of inaccurate statements about CDW. While NADW is the heat source of the modified CDW, it is not the principal source water mass of CDW (Line 7). Deep waters from the Indian and Pacific sectors, as well as deep water formed around Antarctica, are also key sources of CDW (refer to Talley 2011 for detailed hydrography). Also, UCDW and LCDW are not defined properly. And modified CDW is often used in the context of melting under ice shelves, but it is not a counterpart of UCDW and LCDW.

Thanks, we will correct these inaccuracies in the revised manuscript and provide some definitions.

(2.4) Lack of details about their method.

how the BWT is calculated? How is the sampling done? Which types of uncertainties are included in the uncertainty envelope shown in Figure 4? These questions are not trivial. Details, such as choice of d18O-T equation, treatments of ice volume change, and local effects on seawater d18O, are lacking for benthic d18O calculation. For the sampling method, no original data is shown, so one would have no way to assess if the compilation is potentially biased by one particular site.

Some additional information on the d18O method is provided in response Point 1.1 above, and follows Bates et al. (2014). The methods of calculating BWT from Mg/Ca and d18O will be provided in more detail in the Methods, and the respective limitations (currently in the Discussion) will also be moved to the Methods section. We will also add a figure with subplots showing temperature reconstructions at each individual site.

Not sure what is the error envelope alluded to in Fig 4 (scatter plots) – is the reviewer referring to Fig 2? In that case the error envelope for each time slice is calculated using the t-distribution, i.e., as mean +/- t*sigma/sqrtN, using all N samples for that time slice.

Individual sites are shown in Figure 2.4 below, using blue crosses for the original data and red circles for the 4 kyr resampled data. The modern water temperature is shown by the dashed black line. A tidier version of this figure can be included in the manuscript.

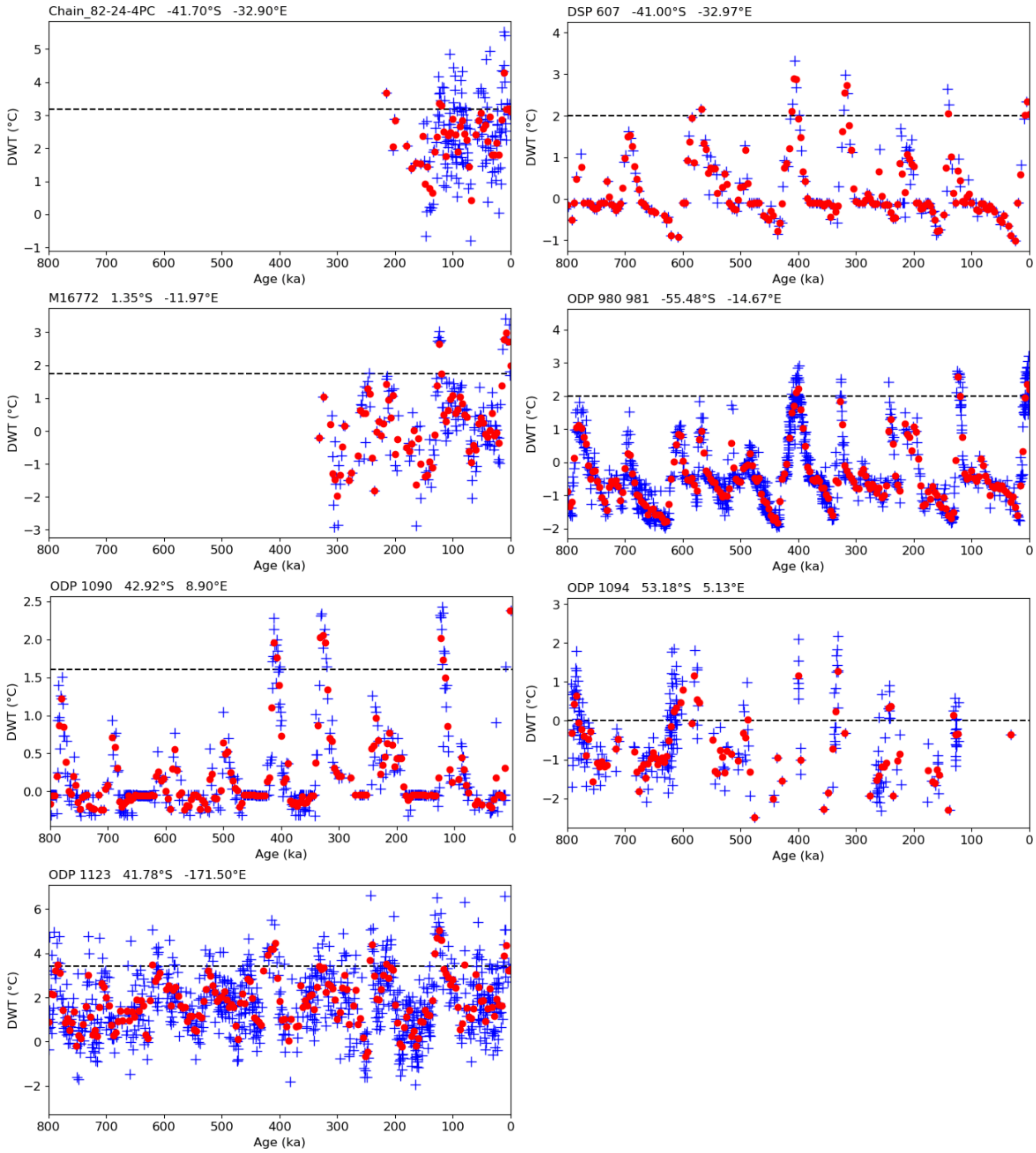

Figure 2.4: Sites used in the original manuscript, showing original data (blue crosses), 4 kyr resampling (red circles), and present-day water temperature (dashed line).

(2.5) The structure of the paper.

A reader is left with the impression that the work done here did not at all contribute to solving the research question raised in the introduction, i.e., the potential influence of CDW T on ice shelf melting during previous interglacials. The introduction sets the expectation of a reader very high and by finishing reading I felt a little disappointed. Perhaps, the authors could put something that can be achieved by this work in the introduction. Also, I think it would be better to provide some background on the limitations of the employed proxies in the introduction.

Regarding the aims and scope of the paper in terms of influence on ice shelf melting, we will add some discussion on thermal forcing - please see our response to Point 1.1 above.

Limitations were not dealt with in sufficient detail in our original manuscript (Section 5.4) and will be described in more detail – in particular with regard to the assumptions underlying the d18O proxy. We will provide these in the Methods section, as we will also be providing more information on the two proxies in that section.

References

Albrecht et al. (2020), Cryosphere, https://doi.org/10.5194/tc-14-599-2020.
Bates et al. (2014), QSR, https://doi.org/10.1016/j.quascirev.2014.01.020.
Chandler et al. (in review), preprint at https://www.researchsquare.com/article/rs-3042739.
Crotti et al. (2022), Nat. Comms, https://doi.org/10.1038/s41467-022-32847-3.
de Boer et al. (2013), Clim. Dyn., https://doi.org/10.1007/s00382-012-1562-2.
Dendy et al. (2017), QSR, https://doi.org/10.1016/j.quascirev.2017.06.013.
DeDeckker et al. (2018), Pangaea datasets https://doi.org/10.1594/PANGAEA.893207, https://doi.org/10.1594/PANGAEA.893210.
Ferrari et al. (2014), PNAS, https://doi.org/10.1073/pnas.1323922111.
Garbe et al. (2020), Nature, https://doi.org/10.1038/s41586-020-2727-5.
Gottschalk et al. (2016), P&P, https://doi.org/10.1002/2016PA003029 and Pangaea datasets https://doi.org/10.1594/PANGAEA.858257, https://doi.org/10.1594/PANGAEA.858257.
Gottschalk et al. (2019), Pangaea dataset https://doi.org/10.1594/PANGAEA.898191.
Hall et al. (2018), AGU Fall Meeting abstract.
Hasenfratz et al. (2019), Science, https://doi.org/10.1126/science.aat7067 and Pangaea dataset https://doi.org/10.1594/PANGAEA.901384.
Hodell et al. (2003), G3, https://doi.org/10.1029/2002GC000367 and Pangaea datasets https://doi.org/10.1594/PANGAEA.218129, https://doi.org/10.1594/PANGAEA.218131.
Howe et al. (2016), Nat. Comms., https://doi.org/10.1038/ncomms11765.
Jenkins et al. (2018). Nat. Geos. https://doi.org/10.1038/s41561-018-0207-4.
Keigwin and Swift (2017), PNAS, https://doi.org/10.1073/pnas.1614693114.
Leutert et al. (2021), Clim. Past, https://doi.org/10.5194/cp-17-2255-2021.
Li et al. (2013), Clim. Dyn., https://doi.org/10.1007/s00382-012-1350-z.
Locarnini et al. (2018), WOA18, https://www.ncei.noaa.gov/access/world-ocean-atlas-2018/.
Malone et al. (2004), EPSL, https://doi.org/10.1016/j.epsl.2004.02.027.
Matsumoto (2017), PNAS, https://doi.org/10.1073/pnas.1701563114.
McCave et al. (2008), QSR, https://doi.org/10.1016/j.quascirev.2008.07.010.
Moy et al. (2008), JQS, https://doi.org/10.1002/jqs.1067.
Niu et al. (2019), GRL, https://doi.org/10.1029/2019GL083717.
Nürnberg et al. (2004), Geophys. Mono. Ser., https://doi.org/10.1029/151GM17.
Peral et al. (2018), GCA, https://doi.org/10.1016/j.gca.2018.07.016.
Quiquet et al. (2018), GMD, https://doi.org/10.5194/gmd-11-5003-2018.
Schmidtko et al. (2014), Science, https://doi.org/10.1126/science.1256117.
Siddall et al. (2010), QSR, https://doi.org/10.1016/j.quascirev.2009.05.011.
Starr et al. (2021), Nature https://doi.org/10.1038/s41586-020-03094-7 and Pangaea dataset https://doi.org/10.1594/PANGAEA.921111.
Struve et al. (2022), Nat. Comms., https://doi.org/10.1038/s41467-022-31116-7.
Sutter et al. (2019), Cryosphere, https://doi.org/10.5194/tc-13-2023-2019.
Tigchelaar et al (2018), EPSL, https://doi.org/10.1016/j.epsl.2018.05.004.
Ullerman et al. (2016), P&P https://doi.org/10.1002/2016PA002932 and Pangaea dataset https://doi.org/10.1594/PANGAEA.833421.
Yang et al. (2011), GRL, https://doi.org/10.1029/2011GL048076.