Response to reviewers' comments

Summary

We would like to thank the reviewers for their critical but constructive comments, which we have now addressed in substantial revisions to all sections of the manuscript. In this revised version we have abandoned the use of North Atlantic sites in our compilation, and instead analysed new sites in the Southern Ocean. This new analysis has considerably reduced the error bounds in our results. We do however still make a comparison between our compilation and North Atlantic site ODP 980. Specific comments are addressed below, **with the reviewers' comments in bold type** and our replies in normal type. Since most of the manuscript has been rewritten we refer the reviewers to specific sections of the manuscript relevant to each comment rather than copying extensive parts of the revised text into this responses document.

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 (Point 2.5 below), so it would seem readers expected us to use our results to discuss ice sheet response, or at least basal melting, in more detail. This was not the original intention – our aim was to compile existing temperature records that are 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 have made the objectives and scope clearer [Lines 70-77]. Nevertheless, some discussion of implications for ice shelf melt is useful, and we have added this in a new Section 5.4 "Implications for ice shelf basal melting". This new analysis shows some interesting differences between thermal forcing estimates in three sectors of Antarctica (new Fig 10).

(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 only briefly at original Lines 65-67. We have now clarified this choice [Lines 64-69]: (i) it covers the period for which there are sufficient data to establish a meaningful synthesis at a practical resolution; (ii) it covers the 100 kyr glacial cycles most relevant to our present climate state, albeit before the onset of anthropogenic influences; (iii) inclusion of colder interglacials prior to MIS 11 can build a clearer picture of Earth system response to warming; (iv) it matches the duration of the longest ice core record (EPICA Dome C: Jouzel et al. 2007); and (v) proxy records derived from oxygen isotopes (the main data source here - see methods) are considered less reliable prior to the MPT (Bates et al. 2014).

Additional records

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 widespread use of benthic d18O in establishing age models even in the Southern Ocean. The extent to which we should expand the scope to include the analysis of new sites, should consider the following points.

(i) The method to convert benthic foraminiferal d18O (d18Ob) to BWT 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 have been respectively reconstructed or measured. 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 that the method is not suitable during glacial inceptions and terminations – as we noted in original Lines 307-312. Unfortunately, the interglacials are too short, especially those from MIS 11 onwards, for ice sheets to reach approximate equilibrium with climate, as this would require tens of thousands of years of constant temperature (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.

(ii) We have reviewed ~130 benthic d18O records south of 40 degS in the Mulitza database. For our purposes, records need to cover several glacial cycles with sufficient temporal resolution, to identify calibration windows and confidently establish a transfer function; they also need to represent a suitable water mass (ideally lower CDW). We also require records to extend back to at least MIS 11, so that our results from MIS 11 to present are not impacted by a changing spatial distribution of records. Based on those selection criteria we have identified six sites, of which only one (ODP 1090) was included in Bates et al. 2014.

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

(iv) For the two Southern Ocean sites with Mg/Ca, calculation of BWT using both Mg/Ca and d18O would provide an interesting comparison.

Given that analysis of the six selected sites seems worthwhile, we also take this opportunity to develop the method used by Bates et al. 2014. This includes updated and additional sea-level markers, and use of 2nd or 3rd-order polynomials instead of linear piece-wise transfer functions. The site selection and analysis of d18O are now described in detail in the methods Sections 3.1 and 3.3. Details of the sea-level markers and sites are included in the new Tables 1 and 2.

(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 original synthesis agreed closely with the Rohling et al. (2021) global mean DWT reconstruction, when their reconstruction has been resampled to match our 4 kyr resolution. This good agreement is generally maintained in the revised version (new Figs 5, 8; sections 4 and 5.1), except perhaps during glacial periods when we find less cooling (interpreted as their being less possibility to cool water that is already closer to its freezing point). Good agreement with Rohling et al was not an inevitable outcome, given the very different geographic scope (Southern Ocean CDW versus global deep water), 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 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. 2018, Crotti et al. 2021). 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 was actually better agreement between the NADW and CDW subsets of records, than between the d18O and Mg/Ca subsets (original Fig 4). However, now that we have analysed new Southern Ocean sites it is of course better to not include the North Atlantic sites. We have been clearer about this point in the revised Methods (Section 3.3 "Site selection").

We also compare our synthesis with site ODP 980 (Figs 8 & 9).

(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.

Both methods are now described in much more detail, in the Methods (Section 3).

(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.

We have revised the oceanography background (Section 2) and added a new schematic Fig 1 summarising water masses in the Atlantic sector, based on slightly different versions of this schematic by Howe et al. (2016), Ferrari et al. (2014) and Matsumoto (2017). Talley (2013) produced a clear 3D figure of circulation in the Southern Ocean, we have referred to that in the Fig 1 caption (rather than copying it directly into this paper).

(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.

The original Fig 4 is no longer used in the revised version. We now compare the two proxies (Mg/Ca and d18O) in the new Fig 6, where it is hopefully clear that each point represents a 4 kyr time slice in one core.

(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 in our original manuscript 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 (particularly for reconstructions of AABW). This aspect has been addressed in the new methods (Section 3), where we also clarify that sample collection is not a trivial task [Lines 293-299].

<u>**Reviewer #2**</u>

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 in the North Atlantic to reconstruct CDW in the Southern Ocean is certainly not intuitive, even though NADW does eventually mix with CDW. That decision was originally motivated by the use of N. Atlantic site 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 also reflected the lack of Southern Ocean sites for which temperatures had already been analysed.

In our revised synthesis we have abandoned the North Atlantic sites and instead have analysed additional d18O records from the Southern Ocean, including three in the Indian-Pacific sector (Table 2 and new Fig. 2). The methods and site selection are now described in far more detail than originally, in the new methods (Section 3).

(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.

First we would like to clarify that "*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 (original Lines 63-64). We had not implied we would go on to use our synthesis to estimate changes in ice shelf melting, although the study is motivated by the need for an improved ocean temperature boundary condition in ice sheet models. Our objectives and scope are hopefully now clearer in the Introduction [Lines 70-77].

It is true that the strong sensitivity of basal melt to temperature changes of 1 degC or even less is a major challenge for using proxy records – or indeed any other existing method – for estimating basal melting prior to the start of modern observations. Although additional records might improve the spatial and temporal resolution of CDW temperature reconstructions, they are unlikely to confidently capture decadal or even centennial changes that are also important for ice shelf melt and grounding line migration (Jenkins et al., 2018). If proxy records are used as the basis for the ocean temperature boundary condition in an ice sheet model, they will only provide a long-term baseline, so that shorter time-scale variability would ideally be added as stochastic noise with characteristics informed by other methods (e.g., modern observations or numerical modelling). We have now noted this limitation at [Lines 531-532]. The problem of smoothing is also now discussed, in Section 5.2.

We have added uncertainty ranges to temperature anomalies quoted in the text.

(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.

The oceanography background (Section 2) has been revised based on Talley (2013) and Carter et al. (2021), as well as other recent studies referred to in Section 2. Our usage of CDW follows its usage in those references.

(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.

The method for calculating BWT from d18O is now described in a lot more detail in Section 3.1.2. Plots for individual sites are now included in Fig. 3.

(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, this was also raised by Reviewer #1. We have now added Section 5.4 and Fig. 10 on potential changes in thermal forcing in three different sectors of Antarctica, noting the important caveat that we reconstruct only CDW temperature and not the rate of its transport into ice shelf cavities.

We agree limitations were not dealt with in sufficient detail in our original manuscript (the reader was simply referred to original publications) and these are now described in more detail in the methods (Section 3).

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