Response to reviewer RC3

Review in italics, our response in standard text

We thank the reviewer for carefully reading our paper and for their comments.

Wolff et al. address the challenging problem of dating ancient ice from Antarctica with ages exceeding 800 ka BP – currently the oldest ice in the continuous ice core record. Such dating is challenging in particular when the ice is no longer in stratigraphic order. The authors suggest 4 ice core records (dD, dust, CH4 and 10Be) that may in theory allow for stratigraphic matching to independently-dated marine-sediment records (of benthic Mg/Ca, Iron, Atlantic SST and geomagnetic dipole, respectively).

Thank you.

The paper is well-written and easy to follow. The topic is suitable for Climate of the Past. I have a series of comments for the authors to consider, in particular for an improved discussion of the approach.

(1) I would ask the authors to provide a more in-depth comparison of the methods, including perhaps a ranking of the methods. There is currently no quantitative analysis of the four methods they propose – this could be remedied by adding a table with the correlation coefficients between the ice core record and the marine target record for each of the methods proposed.

Such a table could also include an assessment of how likely the physics that produce the correlation for the last 800kyr is to persist during 0.8 – 1.5 M also. Such an assessment is likely qualitative and somewhat subjective by necessity (e.g. “likely”, “unlikely”, “uncertain” etc).

A third entry into the table could for example be whether the method would allow for value-matching, or only the matching of sequences.

Combined, these elements would allow the reader to assess the relative usefulness of each method. The authors appear to give somewhat of a ranking of the four methods starting on line 366. The authors appear to favor the Dust-Fe matching over the dD-Mg/Ca matching – a choice that I agree with. This ranking is however never stated explicitly, or justified by the analyses.

We will provide, as requested, a correlation coefficient for the isotope and dust matches, where the whole record is involved as a template. This cannot be done for the 10Be where we have only a few snapshot periods of comparison at present. It also does not make sense for methane, where we point out that the strength of millennial scale events is different between methane and either water isotopes in ice or isotopic data in marine sediments. We point out that the utility of the methane method would be to provide millennial scale interpolation between records that have already been synchronised by other methods.

For information, Elderfield et al gave a correlation coefficient of 0.67 (r^2 = 0.45) for 1123 Mg/Ca temperature against ice core delta-D based temperature (after alignment). For ice core dust against site 1090 Fe MAR, we calculate a correlation coefficient of 0.83 ($r^2 = 0.69$). This is the same for dust flux and dust concentration in the ice (see response to reviewer 4).

We have considered providing a table showing the characteristics of each method. However we believe that the textual description we give in the first paragraph of section 7 provides the information requested, and that a table would be an inflexible way to do the same job. We do not favour a ranking as we believe that it will be most advantageous to use as many methods as possible, to ensure that false matches are not made, and to take advantage of the strengths of each method.

(2) I have reservations about the dD to Mg/Ca matching. To me, one of the most exciting prospects of a 1.5 Ma ice core record would be to investigate the Antarctic dD climate record and its spectral properties. This possibility would be lost if it were wiggle-matched to the deep ocean temperature.

A surprising aspect of the 41-ka world is the absence of a precession (21 ka) signal in the benthic d18O. The hypothesis by Raymo et al. (2006), paper cited in the text, is that the NH and SH ice volume each responded to local precession forcing, yet this cancels out in the benthic d18O (and presumably also in benthic Mg/Ca) as the precession forcing is out of phase between the hemispheres. One of the key questions of a 1.5Ma ice core dD record is whether it has power in the precession band in the 41 ka world – the Raymo hypothesis requires that it does. If the dD record is wiggle matched to a benthic record, we would lose the ability to independently assess the differences in spectral content between such records – one of the key scientific objectives.

The authors suggest that the ODP 1123 benthic Mg/Ca resembles Antarctic dD so strongly because both are controlled by SH high-latitude SST. They follow the argument by Elderfield (2012) here (Lines 125-129). Antarctic dD also strongly resembles global benthic d18O (LR04), and it also strongly correlates with mean ocean temperature from ice cores (Shackleton et al., 2021; Fig. 3). I suspect that LR04 may actually give a better correlation to dD than ODP1123 Mg/Ca does (can you check/show?). Since bottom waters formed around Antarctica are at the freezing point, lowering Southern Ocean SST will not cool them further. It seems to me that global mean ocean temperature, the rate of bottom water formation, and circulation may all impact ODP1123 benthic Mg/Ca. ODP1123 certainly looks identical to LR04 (Elderfield 2012) in d18O, suggesting good connectedness to global ocean conditions.

For these reasons I would ask the authors to reconsider recommending wigglematching dD to ODP1123 as a dating strategy.

We fully understand the reluctance to use matching between marine and ice records because of the fact that this removes any possibility to investigate phasing between records. We would certainly favour methods where there is an a priori reason to expect the common signal to be recorded simultaneously in ice and marine records. This is the case for cosmogenic isotopes with their common production signal. It is partly the case for dust (common production signal) but we can expect some additional influence from overlying transport/lifetime signals that appear to reinforce the production-based signal, but cannot be assumed to be perfectly in phase. It's correct that it is a less strong constraint for a temperature signal, although the mechanism proposed by Elderfield et al (2018) would lead to a similar timing of the temperature signal in ice and deepwater. We will certainly adjust the text to make sure this point is understood.

However, it is important not to forget the underlying reason why we thought this paper was necessary: that it may be quite challenging to date the ice by other means. We would certainly prefer not to use correlations with records (orbital or marine) that require phasing assumptions that cannot then be tested. But without such methods we may have no age scale at all, which certainly precludes the phasing tests discussed by the reviewer.

We are less convinced by the suggestion to compare ice core water isotope records directly with the benthic isotope record from LR04. That record is indeed well-correlated with the EDC isotope record, but it is a mixed signal of deepwater isotopic content (hence ice volume) and deepwater temperature. While it's true that everything looks similar on glacial-interglacial timescales, there are good reasons to expect a more robust mechanistic link between the directly measured Mg/Ca deepwater temperature proxy and the ice core temperature proxy than between the mixed benthic signal and the ice core proxy. Probably the ice core mean ocean temperature signal would be an even better comparator, but it will be a while before a continuous high resolution record of that is available.

(3) could you elaborate on the temporal resolution needed to do the CH4 / planktic d18O matching, and whether this resolution is available in the planktic record (which is promised but unfortunately not shown). For example, planktic d18O misses a DO event around 778ka BP that is clear in the EDC CH4. Could there be DO events that are missed altogether by both records?

The resolution of the planktic record shown in Fig. 7 is about 200 years. The resolution of the methane record in the deepest part of EDC is typically about 500 years. This is sufficient to resolve millennial scale features, though not some of the sharper D-O events that are seen in the last glacial cycle. We discuss problems of resolution that might arise in older ice (lines 291-295 of original paper). We will add a sentence about resolution of the planktic record.

(4) if Raymo et al. (2006) is right, the MPT reflects a transition from terrestrial to marine terminating Antarctic ice sheets. If so, could we get local Antarctic dust sources to contribute to the ice core record? Could this impact the dust matching?

This is an interesting point. If the ice retreated significantly, uncovering large areas of new dust sources it could indeed influence the dust record. We thank the reviewer for reminding us of this, and have added a further caveat to the text. In particular we mention the importance of checking for new dust sources through isotopic fingerprinting such as that used by Delmonte et al (2008).

(5) line-by-line comments:

Line 40-44: acknowledge the Japanese and US ice core communities are also working on this in funded projects.

We already write (line 40 of original text) "Several projects to obtain such a core are partially underway". We then specify the two funded projects in the Dome C region as it is the Dome C record we use as the ice core template. We are a little reluctant to start naming every nation/group with a plan, as this would also include Chinese, Korean, and Russian groups as well as Japanese and US. These plans are at various stages of readiness, and we do not have the information to confirm which of them actually have the funding to drill a deep ice core (for example the US Coldex project is funded for oldest ice work but it would not be true to say they are funded to drill a deep ice core). Rather than getting ourselves into a diplomatic quagmire, we prefer to refer to "several projects" and then specify the two we know best and which take place in the region from which we have taken ice core data.

Line 65: other tuning targets are air content and d18O-O2.

Indeed. We used the word "including" to indicate a non-exhaustive list. Since we do mention d18O-O2 in the next paragraph we will now include that (and a reference) in this paragraph.

Line 115: I think both cited studies suggest dD is a proxy for site temperature – they just disagree as to what the correct calibration is.

But the reason why calibrations are uncertain is because water isotopes are influenced by many factors and processes not all of which are directly related to temperature. We are happy with our wording here.

Line 128: The temperature of the deep waters formed around Antarctica is probably always close to the freezing point. Could it not reflect the volume of deepwater formation, for example, and the mixture with other deep water masses?

While no doubt there are many factors involved in deepwater formation, our statement correctly characterises what the Elderfield paper hypothesised. In fact if you accept their Mg/Ca dataset as representing the deepwater temperature, then it is not always at the freezing point, but actually varies over a range of about 4 degrees. We have not made a change here.

Line 131: You could add Shackleton 2021 here too (new MOT data from MIS 4). In that paper she explicitly plots the very strong correlation between MOT and ice core d18O, which strengthens your argument as you correctly note.

OK, reference added.

Figure 3: is it possible to also plot the comparison as a Mg/Ca vs. dD scatter plot? That way the reader can assess the correlation better.

Line 146: can you give the correlation coefficient for the comparison?

We will provide a correlation coefficient (value given in response above). A scatter plot does not seem necessary to us in addition.

Line 177-180: I fully agree with this sentiment, but feel the same logic can (should?) be applied to ODP 1123. The Benthic temperature has an imprint of southern high-latitude SST, but also of global ocean temperature, volume of deep water formation, circulation, etc.

I think we have to agree to differ here. We agree that the close connection of Antarctic temperature to deepwater temperature at a southern site cannot be guaranteed to be simple or static and we discuss this in some detail (lines 154-166 of original text). However, it is certainly much more closely connected to Antarctic temperature than a record of water isotopes in the North Atlantic, which contains two controlling components: one (ice volume) dominated by northern hemisphere ice, and the other (deepwater temperature) now at a site where both northern and southern waters set the temperature. We agree that we could derive a good observational relationship between deuterium in ice over the last 800 kyr and the benthic record (whether from the Portuguese Margin or from the LR04 stack). However given the multiple controls on benthic isotopes we feel that the argument for constancy of the relationship across the MPT is much weaker than for the ODP1123-Antarctic temperature relationship. Our discussion around this point will allow the reader to make their own judgement whether they agree with us.

Line 203: How good is the match? Can you quantify, e.g. via a correlation coefficient?

We will include a correlation coefficient for the dust match (value given in earlier response above).

Line 255: Perahaps note that DO 2 has no CH4 peak at all.

We think this is too detailed as it would require us to number DO events (so readers know what we mean) and then discuss an event that is actually quite hard to pick even in the NGRIP water isotope record at the scale used here.

Line 268: Throughout what? Throughout the record? throughout the Pleistocene?

We have added “throughout the past 1.45 Myr”.

Line 269: can you give more details than “soon to be published”. Is there a title and author list?

To us, it seemed helpful to alert the reader that this is a record that will appear, not just a long term wish. However we do not use the unpublished data, or even hint at its content, and we do not believe it’s appropriate to list it as a “paper in preparation” with full bibliographic details. If the editor prefers us to remove this phrase then we can do so.

Line 317-318: This is unfortunate (though no fault of the authors). why is the full dataset from a 2013 publication not publicly available at this point? Can you provide more details?

As the reviewer will note the data are shown in a thesis but have never been fully published, hence the lack of a dataset. We cannot answer the question posed.

Line 366: Here the authors appear to suggest a hierarchy with the dust matching being considered more accurate than the other methods. Is this really what you mean to imply?

As discussed above, we see the different records as complementary, each with their own strengths. Dust could indeed be superior to water isotopes but not if (as the reviewer pointed out) we find new dust sources for example. Hopefully the text in section 7 clarifies the strengths and weaknesses of the methods.

Line 378: However, wouldn’t the absolute values of the 10Be be challenging to use due to the accumulation uncertainty? Also, the VADM record is really variable, and certain 10Be values are not necessarily very unique.

Yes. We wanted to inject some optimism as a lot of effort will go into producing the 10Be record. However we don’t want to minimise the issues with using it in practice and hope we reflect this balance in our text.