Response to reviewer RC1

Review in italics, our response in standard text

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

*Wolff and colleagues present some advantages and drawbacks of available chronostratigraphic parameters to date "old ice records" (stretching back to 1.5 Ma) that should be retrieved from ongoing ice core drilling efforts in Antarctica. Providing reliable chronologies to such ice cores are of utmost importance for climatic interpretations. Dating is indeed sometimes regard as secondary compare to interpretations of elegant proxies or high-tech developments requested to recover such deep-old ices. It is however of primary importance to understand climate dynamics. In this paper, authors discuss synchronization and relative matching between ice core and sediment records using four independent ice core parameters and proxies: water isotopes vs Mg/Ca, dust vs iron acc., CH4, 10Be. The similarities already observed over the last 800 ka between ice core and marine records serve as key assumptions to propose and test matching and synchronizations of old ice records (using these proxies as analogues). Such a discussion is very welcome and this paper certainly contributes to this important goal by suggesting few templates and patterns of relative changes useful for dating. The manuscript is well written, concise and rather clear for a broad audience (not necessarily specialists of one of the presented proxies). I therefore recommend this paper for publication following minor revisions (see comments below).*

Thank you for the positive comments.

***Comments***

*Page 2, first paragraph: authors could maybe explain in few sentences the MPT conundrum and why it is particularly "hot" for understanding climate changes mechanisms.*

We are happy to do that, and will add a sentence about the nature of the MPT.

*Page 2, third and fourth paragraphs: as this paper is dealing with chronological issues, it could be interesting to read here a bit more about the dating (methods, ages, uncertainties) of the 2 Ma blue ice. For instance: what's the typical uncertainties associated with these methods? Why is it vital to reduce ages uncertainty to try understanding the underlying causes of the MPT? What is the acceptable minimum uncertainty to interpret the dynamics (amplitude, frequency) of climatic changes over the MPT? Why (and how) is it important (despite uncertainty) to adjunct radiometric ages to fix relative chronologies onto absolute timescale? Etc.*

We will add a note about the use of 40Ar to date the blue ice and quote its precision. The Yan et al paper quoted a precision for the 40Ar method of 110 kyr which is clearly insufficient for placing data within a 40 kyr climate cycle.

*Lines 139-140: please explain how you scaled up the two records. Is maximum stretching still within chronologies uncertainty? This could be interesting since it would somehow quantify the elasticity between two largely used chronologies for two important records, i.e. LR04 and AICC2012 for ODP site 1123 and EDC, respectively.*

The scaling in this sentence refers to the scaling in amplitude not in time. The two records are presented on their own age scales (AICC2012 and LR04 respectively). There are no major discrepancies between the two age models within their respective uncertainties. We will adjust the text to emphasise that it is the amplitude we scale.

*Lines 151-153: it is interesting to note that these mismatches occur within the Mid-Brunhes transition (MBT). Could it be a problem as well within the MPT where amplitude change drastically?*

Yes, this is the point we already make in line 152-3, so we don't think we need to make a further change.

*Line 199: same comment as above, explain the "appropriate scaling" a bit more (how many tie-points, maximum stretching amplitude…).*

Again it is the amplitude we have scaled. Martinez-Garcia et al (2011) carried out the graphical alignment that converted their depth scale into the ice core age scale. We simply converted their ages (on the old EDC3 age scale) into our ages on AICC2012.

*Lines 225-227: based on which arguments?*

Our argument is that the common factorial change is from the changing source strength and from changes at the start of the transport, which are common between the ice and marine records. Only if the lifetime changes completely reversed and overwhelmed the source changes would the pattern differ between the two records We would therefore expect the major features to remain in common. We will add a sentence to explain this.

*Line 269: I'm not sure whether the mention "soon to be published" without any other information is acceptable. It does not tell much to readers. It is a bit annoying since this planktonic isotope and SST records from site MD95-2042 should "serve as a regional template for D-O variability".*

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. If the editor prefers us to remove this phrase then we can do so.

*Lines 271-272: I agree, using CH4 requests testing the validity of the underlying assumption first. Is any such East Asian speleothem record already exist or is soon to be published?*

We are not aware of any longer records from East Asia. There are spelothem data over longer periods from other locations (eg Bajo et al 2020) but this does not answer the question regarding methane source regions.

*Lines 273-277: I would also cite here papers by Giaccio et al., (2015) and Nomade et al. (2019) where millennial events are also clearly identified in records that are also radiometrically date providing potentially important tie-points to fix floating chronologies.*

We thank the reviewer for bringing these interesting papers to our attention. We agree that correlations with records that have radiometric analysis could add absolute ages and we will mention this in the text.

*Line 299: add 'intensity' to "Earth's magnetic field".*

OK, we will add that.

*Lines 305-306: this is a problem since it invokes some circularity, or at least imply a strong assumption. Why not try using regression method (see Zheng et al. 2020 and 2021 for instance) to normalize old ice 10Be records and minimize climate-related variations? One could for instance use water isotopes or ion concentrations data within multi-linear correction method to obtain climate corrected 10Be record. The idea is essentially to remove the shared variance between 10Be and climatic parameters measured at the same depths. This method has been used in wet deposition environments (Greenland), but could maybe provide interesting results, even imperfects, in such dry deposition settings.*

We devote quite a lot of discussion to this issue and in lines 349-355 (original version) we suggest how the problem might be solved. It involves assumptions about the constant relationship between water isotopes and accumulation rate. Despite the assumptions, it does offer a mathematical way to remove the influence of accumulation rate which is more straightforward than looking for shared variance. We have not therefore changed the text here.

*Lines 318-319: this is very unfortunate indeed.*

Yes!!!

*Lines 323-324: authors could emphasis a bit more why 10Be radioactive decay (thus uncorrected 10Be records) could serve as an interesting indicator for long-term age control in case of disturbed or non-continuous ice cores. This was for instance use in Bourles et al. (1989) or to try dating very disturbed (discontinuous) lacustrine records (Lebatard et al. 2010; Simon et al. 2020).*

We agree that the long term decay could give a first order idea of the age, as could the other radiometric methods we mention in the introduction. The use of decay of cosmogenic isotopes for ice is mentioned in line 55, in the context of the use of the composite decay of 10Be/36Cl. Although we agree on the use of these absolute age markers in addition to relative alignments to templates, we think it could be distracting to the line of argument to discuss it again at this point in the paper.

*Lines 333-340 and 349-355: one way to try resolving this issue is to use the regression method (see above) and compare such results with estimated accumulation rate (derived from water isotopes) by an iterative approach.*

We already discuss this point under the comment for lines 305-306.

*Lines 360-363: I agree the spikiness was bypassed in EDC using statistical method. However, the 10Be measurements density in EDC allowed such treatment. Will it possible to do so within older ice samples considering the amount of material needed to measure 10Be. Further works are indeed needed and maybe 10Be measurements on drill-chips (Auer et al., 2009; Nguyen et al., 2021) will be key to obtain high-resolution 10Be records in old ice records.*

We agree that it may be difficult to correct for the spikiness in older ice. Studies are indeed underway, I believe at both Lund and at CEREGE, to try and better understand this issue. Drill chips will likely be used as well. We feel we have already highlighted the difficulty the spikiness imposes and that this is not the place where we can solve it, so we have not altered the text.

***References***


*Auer, M., et al. (2009). Earth and Planetary Science Letters, 287 (3), 453-462.*

*Bourles et al. (1989). Geochimica Cosmochimica Acta, 53 (2), 443-452.*

*Giaccio et al. (2015). Geology, 43 (7), 603-606.*

*Lebatard et al. (2010). Earth and Planetary Science Letters, 297, 57-70.*

*Nguyen et al. (2021). Results in Geochemistry, 5, 100012.*

*Nomade et al. (2019). Quaternary Science Reviews, 205, 106-125.*

*Simon et al. (2020). Quaternary Geochronology, 58, 101081.*

*Zheng et al. (2020). Earth and Planetary Science Letters, 514, 116273.*

*Zheng et al. (2021). Quaternary Science Reviews, 258, 106881.*


References:


Bajo, P., et al. (2020), Persistent influence of obliquity on ice age terminations since the Middle Pleistocene transition, Science, 367(6483), 1235, doi:10.1126/science.aaw1114.