Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
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Comment on cp-2022-2

Anonymous Referee #1

Referee comment on "Stratigraphic templates for ice core records of the past 1.5 Myr" by Eric W. Wolff et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-2-RC1, 2022

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).

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.

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.

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.

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?

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

Lines 225-227: based on which arguments?

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".

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?

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.

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

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.

Lines 318-319: this is very unfortunate indeed.

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).

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.

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.

References

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