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Climate of the Past Discussions

Interactive comment on "Synchronizing ^{10}Be in two varved lake sediment records to IntCal13 ^{14}C" *by* Markus Czymzik et al.

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In this paper, the authors address an essential issue in any paleo-studies, the chronology. They propose a method to help synchronizing varved lakes with other natural archives which is essential to improve our understanding of the mechanisms driving climatic variability. The group of authors already introduced this method for other sites or other time periods in several publications and pursued successfully their investigations in this paper that I recommend for publication following some revisions. The aim of the paper is essentially methodological and I imagine that climatic discussion based on precise inter-correlation of TSK (less evident for JC) with other records, using the 10Be method, will be presented elsewhere (which is fair). The text would greatly benefit from several complementary notes on method, interpretation and discussion (see

comment below). Given the rather short length of the present manuscript, it should be possible to provide these information without weighing too much on the final version of the paper. I listed below a series of comments and questions – voluntarily naïve or not – for which answers (integrated into the paper) should improved robustness an easy readability of the paper.

I would also suggest a slight change of the title to enclose all aspects of the paper: "Synchronizing 10Be in two varved lake sediment records to IntCal13 14C during grand solar minima" (see below).

Comments (page.line):

2.5. The authors could explicitly mention all type of archives which (will) benefit from 10Be for global synchronization. What is the range of time-scale uncertainties associated with these different archives? This is particularly important since it implies different resolutions associated with inherent archive limitations. Despite the most robust archive-to-archive correlation possible (maybe provided by 10Be), these restrictions constitute a limiting factor for studying specific climatic mechanisms in some archives and/or from older ages, particularly about precise lead and lags in the climate system.

2.10. The authors can add paleomagnetism to the series of useful synchronization tools independent from climatic cycles. Use the term "radionuclides" rather than "isotopes".

2.15. Recent works of groups from, e.g., France (Ménabréaz, Valet, Simon. . .) or Japan (Suganuma, Horiuchi. . .) also documented geomagnetic field forcing on the 10Be production variation, is there an impact of these modulation on your records? More largely, what is the impact of solar activity and geomagnetic intensity variations on the magnitude of atmospheric 10Be production rates? Since authors are discussing a synchronization tool that can (will) be used for other time periods, presenting these elements is important because they explain why and how 10Be works, particularly at certain period of time.

2.25. What is the results of this spatial heterogeneity? Does it complicate easy inter-regional correlations? If yes, to what extent? This is important for using 10Be as an accurate global synchronization tool of course.

2.30. Add the recent Raisbeck et al. paper (Clim. Past, 13, 217–229, 2017) which discusses synchronization between Greenland and Antarctic ice cores using 10Be. Does "synchronization of terrestrial paleoclimate records around the globe" need to assume a global homogenization of 10Be production/deposition (see above)? 2.35. Why only studying these three periods? Do you expect higher level of 10Be changes during these intervals? It might be interesting to give some precision here. Moreover, it could be a good idea to mention the three grand solar minima it in the title since your study is focused on these periods.

3.15. What is the extent of sedimentary changes in both cores through the studied intervals? Are they related to any known (studied) climatic cycle? This is important since sedimentary changes can drastically disturb Be records in geological archives. For instance, the last two sentences dealing with current air masses and precipitations behavior are interesting for modern settings but do these parameters also prevailed during the periods scrutinized here?

3.20. To what range of depth intervals correspond a 20-year resolution? What is the sediment amount needed for method? How many years are integrated by the sampling (thickness of the sediment samples)? Also, I do understand that authors want to keep short, which is definitely not a bad idea, but since the chronology is central in the paper (e.g. Title) I find important to present how age models have been obtained (not simply referring to the original publications). What is their resolutions and uncertainties? There is no need to develop too far, but to provide with enough elements for the readers to judge the resolution and potential bias induced by inevitable age errors. This is particularly important since the paper discusses about age offsets with resolutions of only few years back to > 5 ka BP.

3.30/4.5. How do you homogenize sediment samples? What is the sediment weight used? Authors should write that they are interested only by the fraction adsorb or precipitated on sediments (sometimes called "authigenic"), and why are they interested by this fraction? They could precise that metal hydroxides and silicates are precipitated while Be remains in solution. Why precipitate at pH 10 and not 8.5? Are you not precipitating (or risk to precipitate) Boron at this pH level? These last two questions are probably not interesting for the paper, personal interest about the method. It could be useful to add a citation that provide with full description of the method followed here. Add at the end of the last sentence: "and corrected for radioactive decay (Chmeleff et a., 2010; Korschinek et al., 2010)". I totally understand that this correction does not change your results, but better be precise with radioactive elements. Retrieving the exact 10Be concentrations imply a correction for radioactive decay, even if changes occur at the margin given the sediment ages and the T1/2 of 10Be. Note also that authors could add somewhere in the text the half-life of both 14C and 10Be to give the time extent, and therefore theoretical limits, of these tracers (probably more useful for 10Be than for 14C which is already well known by the community).

4.10/15. What is the time uncertainty associated with your data?

4.20. I would remove any mention to Figs. 2 and 3 in the results section as these figures are plotted versus age. Results versus ages are already part of a discussion because they imply a serious transformation through the application of age modeling. Presentation of the raw 10Be concentration data versus depth in new figures is maybe not mandatory since I guess these data will be available as supplementary material or easily available from the web. I know this comment is annoying but discussion will likely evolve while the data will remain, and are therefore important for the community. The authors should highlight directly on the figures the location and duration interval of the grand solar minima discussed (which do not represent the whole box intervals).

4.30. What kind of non-production forcing parameters can explain part of the 10Be concentration variations in varved lake sediments? 2-3 sentences could help readers

to rapidly understand such processes without having to refer to a third party (interested readers will of course go to these citations). I'm wondering why you selected these parameters specifically (i.e. TOC, SAR, Ca, Si, Ti)? Are their fluctuations representing correctly all lithological changes observed in the lakes (e.g. productivity, grain-size, mineralogy)?

5.5. I agree that significant contribution of TOC and Ca on the whole intervals justify their used to the multi-regressions treatment. Yet, it is possible that other elements presented in the paper also impact the 10Be signal within specific depth intervals (e.g. Ti since about 200 a BP in TSK). If they are associated with specific events linked to rapid climatic changes, how can you estimate their residual influence on the 10Beenvironment record calculated? Actually authors correctly discuss that matter later in the paragraph but it results into a blurry questioning about the reliability of the environmental correction procedure, essentially because the method does not rely on any mechanistic linkages between 10Beconc and TOC/Ca, as mentioned by the authors themselves. One could mention here that the method is mainly working because the outcome (10Becomp) is highly comparable with 14C production (Fig. 6) but, although valid, this argument is slightly circular. The main question remains: how to correctly remove, or say diminish, environmental variability imprints on 10Be records in lakes?

5.15. As the authors are interested by multi-decadal variations (see Figures 5 and 6), why not working on 10Beconc series directly as this variability is similar between both 10Becomp and 10Beconc series. This would avoid unnecessary and questionable data treatments while preserving the conclusion.

5.25/25. These two paragraph are rather interesting but could be move above (5.5) to support the use of these two elements for the multi-regression method used to obtain the 10Bebias. Also, it would be interesting to discuss a little bit more (or cite references?) about the exact – or supposed – mechanisms explaining "preferential binding of 10Be to organic material", while the affinity of 10Be to Ca has been indeed demonstrated in several studies already cited in the paper.

6.5. Does result differs when using 10Beconc instead of 10Becomp (see comment above)? In TSK, the unfiltered two 10Be peaks visually correlated with the two sunspot number lows, why not mentioning it? Do you have sedimentological elements to sustain a transport of "old" 10Be? By which processes such a transport can take place (physical remobilization or desorption form sediments previously deposited onto "shelves")? It might be interesting to mention it here, or to refer to explanations provide later in the text.

6.20. Are you using this best fit result to propose a new chronology for TSK?

6.30. See above (point 6.5).

7.5/10. Conclusion is fine and clearly wrap up the main objective of the paper, i.e. 10Be is a robust tool for synchronization (TSK) unless environmental imprint is too strong (JC).

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-117/cp-2017-117-RC1-supplement.pdf

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