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Interactive comment on “Atmospherically-produced beryllium-10 in annually laminated late-glacial sediments of the North American Varve Chronology” *by* Greg Balco et al.

Anonymous Referee #1

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Review of Balco et al.

Balco et al. present new 10Be/9Be data from several varved sediment records which form part of the North American Varve Chronology (NAVC). Their aim is to test the applicability of these cosmogenic radionuclide records to synchronize the NAVC to Greenland ice core 10Be records. This would allow for detailed studies comparing the timing of climate changes recorded in both archives. To do this, they investigate a number of records addressing different questions: i) Do the sediments record the 11-year solar cycle? ii) Do the sediments record longer term solar variability? iii) How does the measured variability compare to ice core records? Balco et al introduce a linear model which separates the measured 10Be into two components: one representing the 10Be from recent fallout (which is the crucial figure when wanting to reconstruct production rates in order to achieve synchronization to ice cores), and a second component derived from various sources ("old" 10Be present in the ice or adsorbed to the sediment) which they refer to as "inherited". By measuring 9Be and assuming that inherited Be has a constant 10Be/9Be, Balco et al. correct for the effect of variations in the delivery of inherited 10Be, isolating 10Be derived from fallout. Applying this model to the different records they find that i) The sediments do not record the 11-year solar cycle, ii) they may record longer term production rate variability, iii) the longer term 10Be variability shares features with the ice core records and may hence be synchronized. Subsequently, they derive possible timescale offsets between ice cores and sediment by lagged correlation analysis and discuss the results in context with previous timescales based on 14C and climate-matching.

Generally, I think this study is a very careful piece of work which provides great insight into 10Be records in complicated settings. Nevertheless, I have major reservations about the interpretation of the 10Be records the authors provide. Mainly, I do not think that the 10Be records reliably reflect production rate changes, but rather that they are dominated by variations in the 10Be/9Be of what the authors call "inherited" 10Be. I want to stress, that this does not affect the discussion and quality of this work, which is excellent. In fact, the authors point to the uncertainties of their records throughout the manuscript. Nevertheless, I think the sections dealing with the synchronization should possibly be left out of the manuscript as I consider them unreliable, but they might be used by future studies. Below, I will outline the reasons for my skepticism in more detail and propose additional tests.

Whenever attempting to use cosmogenic radionuclide records for synchronization, the key-assumption underlying all this, is that all records reflect production rates. This needs to be demonstrated before any further synchronization is attempted. Otherwise, one may synchronize spurious signals. This may not be necessary when all records are very similar, but looking at figure 18 and 22 I'd argue that they actually do not share a lot of similarities, but that it is mainly single peaks that are being aligned.

The authors attempt to verify their 10Be records by looking at frequency spectra and cycles – none of which are giving a clear indication that we are indeed looking at production rate changes. One major argument that is not explored is the amplitude of changes. From production models we know which amplitudes we expect from production rate changes. For solar activity changes this is typically in a range of around 50% (R Muscheler & Heikkilä, 2011), while geomagnetic field changes may introduce larger changes (however, there are no large geomagnetic field changes within the study period). Through their regression model, the authors demonstrate that in their setting, meteoric fallout ($\sim$production rate changes) only contributes to about 10% of the total 10Be, while 90% are inherited. Given this big reservoir, production rate changes will be strongly dampened. A production rate change of 50% would appear as a 5% change in the record, which is essentially equivalent to the estimated uncertainty of the data. More importantly, a variation of only 5% of the 10Be/9Be ratio of the inherited 10Be would have the same effect in the record: Can this really be excluded?

The authors argue that the reproducibility of 10Be variation at different sites demonstrates the influence of fallout 10Be on the sediment records. In my opinion, the records shown in figures 16/17 do not share a lot of variability and their disagreement should be used as an uncertainty estimate. The slight covariability between both sites is also seen in the mass accumulation rates, and hence, both records may simply share the same bias, introducing similarity.

In my opinion, the authors should discuss the amplitude of the changes in their data explicitly. In this light, also the comparison to ice core data done in zscores should be avoided, and instead relative amplitudes (relative to the mean) should be compared because they give additional information. From the regression model, the size of the reservoir (inherited 10Be) can be estimated, which can be used to rectify the amplitude dampening of the "fallout" component – is this amplitude physically reasonable, and significant beyond measurement uncertainty? Are the aligned wiggles of ice core and sediment 10Be of similar amplitude? Looking at figure 20 for example, and assuming a large contribution of inherited 10Be implies, that the glacial 10Be variations are in the order of a few hundred percent, happening within decades. From production rate models we can exclude that this type of variability can be caused by solar modulation. Large cosmic ray events on the other hand would be more short-lived, while geomagnetic field changes occur on longer timescales. And either way, the variations in the ice cores which this wiggle is linked to in figure 22 is significantly smaller, i.e., not the same feature.

Based on these revisions, the authors may or may not want to keep their section about the GICC05/NAVC comparison. I wonder, how much there is to be learned about the NAVC when 10Be records from ice and sediments actually don't really look alike. One will always find single peaks to align. The disagreement between ice core and sediment 10Be records even after alignment could at least be used to derive a posterior uncertainty estimate for the 10Be records (by e.g., RMSE), which could be factored into the uncertainty of the timescale-match. I imagine it will be so big, that all estimates (14C, climate, 10Be) are essentially indistinguishable within error.

As said in the beginning: I do not think that these points really affect the quality of the paper, which provides a great discussion of the factors influencing 10Be in lacustrine sediments. But I think, these points should rather be expanded by an amplitude-discussion, and instead the synchronization aspect should be shortened as it is (at least based on the present analysis) likely unreliable.

Minor Comments: Introduction: Please add a section to the introduction that highlights the main assumption behind the synchronization of radionuclide records (i.e., that the production rate changes dominate the signal) and why this is not a given for any archive (transport, deposition, reservoir, residence time, variable dilution,. . .) and thus a major complication for the aim of this (or any) study.

NAVC varve count error: In the entire discussion there is no mention of the varve counting (relative) error of the NAVC. Is this really zero? It should at least be noted in the appropriate sections that differences in the GICC05/NAVC offset may well in part be due to errors in the NAVC, not just GICC05.

9Be/10Be extraction: Typically, 9Be and 10Be are measured on the same leaching fraction of sediment. Here, the authors employ different extraction techniques for both isotopes. For this to work, the extraction efficiency of each method must be constant. Is this a problem? Maybe the authors could comment on whether this has the potential to introduce variations in 9/10Be.

P 1, L2: replace “calendar year timescale” with “ice core timescale”

P 1, L11-14: long, convoluted sentence. Consider dividing.

P 1, L20-24: difficult sentence. Similar but different?

P1, L20: “not consistent with independent evidence” – the previous sentence states the opposite?

P2 L15-16: Please indicate what this uncertainty refers to, i.e., how many sigma.

P2, L28-29: “generally globally synchronous”. See my comment on an additional section for the introduction. This is only true under ideal conditions and when averaging sufficiently over meteorological variability. In many settings, meteorological influences will be dominant over the production rate changes.

P2, L30: Heikkilae et al. 2013 is not a good reference here, as they do not reconstruct solar variability. Please find more appropriate references such as: (Bard et al., 1997; Raimund Muscheler et al., 2007; Steinhilber et al., 2012).

P2, L32: Again, Heikkilae at al and Adolphi et al. do not contain new measurements. Please give credit to the authors that produced these long records: (Adolphi et al., 2014; Baumgartner et al., 1997; Finkel & Nishiizumi, 1997; Raimund Muscheler et al., 2004; Vonmoos et al., 2006; Yiou et al., 1997)

P3, L22-24: If there is a transient transition over 100-200 years in the lake system: How can a synchronization of local climate records to Greenland be attempted? The result would depend on which varve record was chosen? Could you please comment? What does this mean for the achievable uncertainty of climate synchronizations?

P6, L1: See earlier: Could you please add a sentence on the counting uncertainty of NAVC? How valid is the approach of using a single value for the offset?

P6, L16-26: Recently a new radiocarbon calibration was released (IntCal20) which contains significant changes from IntCal13. I understand that it is beyond the scope of this study to redo all the calibrations, but maybe it is worthwhile mentioning that this adds extra uncertainty to the (by looking at the data, in my opinion very optimistic) 200 years.

P6, L29-33: Please do not use the term "climate events" here, when the event stratigraphy by the ice core community does not contain any events during this time. Generally, the climate wiggle matching in this section should be cross-checked by not only using 1 ice core, since these minor wiggles in d18O may be noise. They should be verified using GRIP, NGRIP and NEEM d18O.

P9, L15-16: Please add a citation to the figure for the fallout flux.

P9, L22: remove the "-" in the 0.6m.

P12, L1-3: This is only true if these inaccuracies are systematic. Are they?

P12, L9: Looking at figure 3 the +-0.05g/cm3 seem very optimistic. How would the regression in figure 3 change if the two summer varve measurements were excluded from the analysis?

P12, L20: replace "between" with "around"?

P14, L9: Is there a +1 missing from the right hand side of the equation? Otherwise, if

C/Cs equals 0.9, the right hand side becomes 0?

P16, L1-2: Earlier you write that S may be variable? Does this correction come with an uncertainty that is propagated throughout the study?

P18, Figure 9: Around 6700 NAVC years, there are large changes in MAR and 9Be, but not in 10Be. While it could of course be, that simultaneous and large production rate changes (however, of likely unphysical amplitude, see major comments) "counteract" this the changes in the delivery of inherited 10Be, this seems unlikely. Rather, this may highlight variable 10/9Be ratios in the inherited Be. Please add a few sentences discussing this feature.

P22, L8-9: "constant or normally distributed": But neither of these assumptions is true. It is obviously not constant (otherwise this study wouldn't be possible) and it is also not symmetric because i) if solar variability was normally distributed, the non-linear production rate relationship would still cause a skewed distribution of production rates, and ii) the transport and washout of aerosols from the atmosphere causes a logarithmic distribution of aerosol deposition even if the production rates were normally distributed. It is still ok, to use the model as is, but it should be highlighted, that these assumptions are not true, but sufficiently correct to not affect the validity of the results.

Figure 13: it would be interesting to see this figure in relative units, i.e., both 9Be and 10Be relative to their respective mean. If the assumption of a constant 10Be/9Be of the inherited Be was true, all data-points should scatter around the 1:1 line?

P26, L18: "factor of 2" it should be noted that also ice cores are affected by transport and deposition effects of 10Be, especially during periods of such variable climate. As mentioned earlier, there are physical constraints of what amplitude of changes we can expect.

Figure 18: "95% confidence" How is this determined? Do you take autocorrelation of the records into account?

P31, L3-4: I'd argue that the agreement is not that good? The amplitudes are different, and due to the similarity of both MAR records, the similarities in 10Be fluxes may well be cause by the flux calculation. Is it worthwhile discussing this option?

P31, L4-5: The general problem with matching these records is, that one will always find a match due to the periodicity of the signals. Hence, the amplitude discussion is important.

Figure 19: The doubling of 10Be/9Be between 6800 and 7200 NAVC cannot be production (unphysical amplitude). Please discuss.

Figure 21: See above

P34, L24-35: I'd argue that the sediment and ice core 10Be records simply don't look alike. Aligning a single peak can easily lead to erroneous results. Please find a measure for the similarity of the records that can be used to quantify the uncertainty in the match as well.

Figure 22: Please do not display the data as zscores. A lot of crucial information is lost that way, and it causes deviations between the ice core datasets that are merely due to resolution affecting the standard deviation of each record.

Figure 24: Please specify on which timescale the GISP2 d18O record is shown. GICC05 I suppose?

Figure 25: Consider plotting NGRIP, GRIP and NEEM d18O as well to get an idea about the robustness of those features.

P40, L7: According to the ice core event stratigraphy (Rasmussen et al., 2014) there are no "events" in the ice cores around this period.

Apologies for the lengthy review – I hope some of these comments are helpful to improve the manuscript and the efforts of dating the NAVC.

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