

Interactive  
Comment

# ***Interactive comment on “Synchronizing the Greenland ice core and radiocarbon timescales over the Holocene – Bayesian wiggle-matching of cosmogenic radionuclide records” by F. Adolphi and R. Muscheler***

**Anonymous Referee #1**

Received and published: 31 August 2015

## **Overview**

The Greenland Ice Core Chronology 2005 (GICC05) and the radiocarbon dating calibration curve (IntCal13) are benchmark paleoclimate timescales covering the past several tens of thousands of years. The IntCal timescale is constructed from radiocarbon measurements on annually resolved tree-ring records. The GICC05 timescale is based on annual layer counting in Greenland ice cores. Relative dating differences between the IntCal and GICC05 timescales hinders ability to compare the timing of events recorded on these different timescales.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Adolphi and Muscheler construct a transfer function between the two timescales to allow more straight-forward comparisons of records on the two time scales during the Holocene. Their transfer function makes use of the fact that cosmogenic radionuclide in ice cores Be-10 is (like 14-C in tree rings) modulated by changes in solar and geomagnetic activity. Their method de facto assumes the tree ring chronology is correct and that the difference between IntCal and the ice core chronology can be eliminated by adjusting the ice core timescale to obtain the optimal match between centennial-to-millennial-scale variations in Be-10 and their counterparts in C-14. A complication with their approach is that variations in Be-10 can be caused by changes in atmospheric transport and accumulation rates, and variations in C-14 can be caused by changes in the carbon cycle. The authors are well aware of this and go to lengths to account for and quantify the sensitivity of their method to climate and carbon cycle effects. Their transfer function ultimately identifies a timescale difference between GICC05 and IntCal13 of less than 30 years throughout most of the Holocene and up to 70 years by the Early Holocene. These differences appear reasonable in the light of other similar findings and are largely within the reported counting error of the GICC05 timescale.

My overall assessment is that the treatment of the C-14 and Be-10 records is very well done and that the transfer function will be extremely useful to the paleoclimate community. The authors are experts on C-14 and Be-10 and I congratulate them on applying their knowledge to this issue. I recommend publication in *Climate of the Past* after some minor (but important) revisions and clarifications.

### General points

#### Climate influences on Be-10

The discussion and treatment of climate influences on Be-10 is generally good, however there are a few places where I think it could be improved:

- PP2936, L13: It should be noted that most sites receive Be-10 from a combination of wet and dry deposition processes. For example, the detailed treatment of

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- wet and dry deposition processes in the recent paper by Elsässer et al., [2015] suggests 32
- PP2936, L18: Please add some more specific language on processes which affect Be-10 concentration in ice e.g. revise to something like “. . .Be-10 transport paths, including stratosphere to troposphere exchange and air-mass precipitation history and can cause climatic imprints..”
  - PP2936, L18: Elsässer et al., 2015 should be added to the list here, they suggest a modest polar bias, similar to the results of Field et al.
  - PP2942, L22: Note that the Pedro et al., result refers to the coastal East-Greenland ice core site Das2 (not GRIP or GISP).
  - PP2945:L5: The statement here assumes that the regression and linear detrending with respect to other proxies in the ice core data does in fact remove all centennial scale climate influences on Be-10. While I agree that the linear detrending is a good step, it is not clear that it would remove all climate influence on Be-10. For example, changes in stratosphere to troposphere exchange are expected to influence Be-10 but not necessarily the other proxies that have been used in the detrending. Please add something to the effect of “a caveat is that climate influences specific to Be-10 will not be removed by the detrending technique”.
  - PP2945:L5: “little climatic influence on Be-10 even over large deglacial climatic transitions”. This statement could be misinterpreted. It is well established that glacial to interglacial climate transitions leave a very large imprint on Be-10, mainly due to accumulation rate changes e.g. Finkel and Nishiizumi [1997]. Please revise the wording.
  - PP2945, L3 and Figure 2. “The centennial changes in the GRIP and GISP2 Be-10 versions, however, are highly coherent and indicate a limited climate influence

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

on Be-10 on these timescales”. Clarify if you refer to coherence between the GISP2 and GRIP records or coherence internally within the GISP2 and GRIP records. I would agree that there is good coherence between the curves from the same site but it is not clear that there is good coherence between the records from GRIP and GISP2. A panel in Figure 3 should be added to the main text or at least to the review response showing the Be-10 concentration anomalies at both GRIP and GISP2. The authors do not necessarily need to explain the differences between the records but they should at least be acknowledged given the statement “indicate a limited climate influence on Be-10 on these timescales”. If the authors intend to say that there is good coherence between GRIP and GISP2 records, it could help clarify the section to explain explicitly what is meant by “coherence”, i.e. that the records share the main peaks but not the smaller variations.

### Inferred C-14 production rates and IntCal13–GICC05 transfer function

I will state up front that I do not have expertise in the Bayesian techniques used here and hence I cannot critically review that aspect of the methodology. Nevertheless, I have some questions and comments about the data treatment that I feel are important to be addressed and which may help make the manuscript accessible to a wider audience.

- PP2945 L17: Add a panel to Figure 3 showing the comparison between time series of the GRIP and GISP based C-14 reconstructions. This would be useful for the reader to see directly the coherence between the two, otherwise explain why this direct comparison is not needed.
- The advantage and influence on uncertainty of going from 50 year  $P_{s_{scaled}}$  to the annual resolved final age transfer function could be better explained. It would help to clarify how the interpolation affects the uncertainty if you could plot the

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

$Ps_{scaled}(ts)$  (i.e on its 50 yr spacing) against the probability distributions for the final age transfer function. The authors should address if the proposed Monte Carlo method adds information compared to the simpler approach of interpolating between the 95

- Fig 9: The thin black lines are not defined (2-sigma on transfer function?). Also please clarify the difference between the thin black lines and the  $Ps_{scaled}(ts)$ . Why do the  $Ps_{scaled}(ts)$  and transfer function deviate from each other in places (e.g. 7.5 to 8.0 ka). Is this the result of the 50-yr to annual Monte-Carlo interpolation, which goes back to my question above?
- P2948, eq. 4: The difference between the tree-ring and Be-10 based delta C-14 values is sometimes zero (Fig. 7), and as the IntCal term is always positive, the equality in eq. 4 is not satisfied. The statement may be true if using  $<$  sign instead. But the whole section appears a little convoluted. It seems to me that what you do is to adjust the Be-10 scaling factor to minimize the (rms or rms-binned) difference between the tree-ring and Be-10-based Delta C-14 values. After obtaining the best value of the scaling factor, you can use the rearranged eq. 4 to estimate (what I would say is the lower bound of) the uncertainty of the Be-10-based Delta C-14 values.
- Section 2.2: Please specify whether you stretch or only shift the timescale of the ice core Be-10 data to get an optimal fit with the IntCal C-14 data within each 1000-year window. This may be clear to those familiar with the Bronk Ramsey et al. (2001) paper, but it would be good to make it explicit here.
- Section 2.2: The authors tests the method for robustness in many ways, but the 1000-year width of the correlation window is not tested. That test should be added or at least the authors should discuss why 1000 years is the best choice.
- Section around P2954, L4. Please note that the main part of the estimated

IntCal13-GICC05 difference builds up during the period 8 – 10.3 ka BP, which is the section where the dating is based on GRIP CFA data that have fewer components and lower resolution than the NGRIP dataset employed from 10.3 ka BP downwards [Rasmussen et al., 2006]. The difference curve (Figs. 9-11) levels out in the section between 10.3 ka BP and the onset of the Holocene, corroborating that there are much smaller systematic counting errors in the section based on NGRIP CFA data.

- Fig 7 and Section 2.2 and 2.5: The fit is very impressive. It would be help the reader to see how your method has reduced uncertainly between the timescales if you could also show some comparisons before synchronisation. I would suggest to show at least one, and preferably 2-3 examples (e.g. best, typical, worst) of 1000-year long sections of wiggle-matched records to allow the reader to evaluate the robustness of the fit.
- In the final version please specify where the IntCal13–GICC05 transfer function (and relative and absolute uncertainties) will be made available.
- PP2953, L15-20: Worth to specify that the difference is in the direction of systematic over-counting of years.

### Technical points

- Many of the figures have multiple lines overlain that become hard to distinguish, e.g Fig 5 has 4 lines plus shading. Use of color would probably improve clarity. The figures also appear small. I had to zoom in on the screen to see important details. Can you make the figures bigger?
- In general, 'both' is overused. When it is clear that you are talking about two things it is mostly not needed to say both. An example: PP2945, "Both, changes

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

in ocean ventilation [and] air–sea gas-exchange can cause  $\Delta C-14$  anomalies larger than the amplitude of  $\Delta C-14$  anomalies induced by C-14 production rate changes only”. Drop the “both”, it only confuses things here. Also: “One method to compare and synchronize both timescales is the use of cosmogenic radionuclide records”. Here, “both” is misleading unless you are synchronizing (both) time scales to a third one.

- P2935, L22: “ideal tool” is overstating things given the climate and carbon cycle influences.
- P2936, L11: Delete “On the other hand”.
- Section 2.2: Typos: Bronk not Bonk.
- Please state what dating of GISP2 was used: obviously it should be GISP2 on GICC05.
- Figure 1 Caption: Key data not Key-data. I also noticed some other examples of funny use of hyphens. Please check usage throughout.
- Figure 2 Caption: I can’t make sense of the second last line, please revise.
- Figure 5 caption: Description of panel b) appears to be referring to an earlier version.
- Note some inconsistency in x-axis labels: sometimes yrs BP and sometimes years BP.
- Add space between ka and BP, eg on PP2947.

---

Interactive comment on Clim. Past Discuss., 11, 2933, 2015.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)