F. Adolphi & R. Muscheler: “Synchronizing the Greenland ice core and radiocarbon timescales over the Holocene – Bayesian wiggle-matching of cosmogenic radionuclide records”. Reply to Reviewers.

First of all we would like to thank both reviewers for their insightful and helpful comments. Below we will address each comment point by point, showing the reviewers comments in black and our response in blue. Changes to the original manuscript are highlighted in **bold**.

Reviewer #1:

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.

Thank you.

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 wet and dry deposition processes in the recent paper by Elsässer et al., [2015] suggests *32 (?Apparently a part of the original sentence was cut off here? We hope that our answer below addresses the question correctly.)*

We agree. In fact, we address this point in line 15-16 (pp2936) by writing “While today wet deposition is dominating over Greenland (Heikkilä et al., 2011) the ratio between wet and dry deposition has likely changed over time (Alley et al., 1995).” Furthermore, by using concentrations and fluxes separately, we account for both endmembers (wet/dry deposition) of these processes. We show that these lead to consistent results (e.g., figure 5). Any mixing between the two modes of deposition would hence, likely not change our results.

We changed PP2936, L13 to:

“In reality, both modes of deposition contribute to the accumulation of 10Be on the ice sheet. Today, wet deposition processes dominate over dry deposition which accounts for about one third or less of the deposited 10Be in Greenland (Heikkilae et al., 2011, Elsaesser et al. 2015). However, this dry/wet deposition ratio has likely been variable over time (Alley et al., 1995).”

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...”

We changed PP2936, L18 to: “Secondly, a variety of climatic influences can leave an imprint in ice core 10Be records. Atmospheric circulation changes and air mass precipitation history (i.e., 10Be scavenging by precipitation prior to the arrival of the air mass at the ice core site) may, for example, modulate the transport path and efficiency of 10Be delivery to the ice core site (Heikkilae & Smith 2013, Pedro et al. 2011, 2012). Furthermore, changes in the exchange rates between stratospheric (high 10Be concentrations) and the tropospheric (low 10Be concentrations) air masses can affect the tropospheric 10Be budget (Pedro et al., 2011).”

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.

We added Elsaesser et al. 2015 to the reference list in PP2936, L 26-27.

PP2942, L22: Note that the Pedro et al., result refers to the coastal East-Greenland ice core site Das2 (not GRIP or GISP).

Yes. However, we feel that PP2942, L17-23 should be seen as a short review of the range of partly disagreeing results that have been published on the issue of a potential polar bias in 10Be records.

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".

A good point. We changed PP2945, L3-6 to:
"This is in agreement with Adolphi et al. (2014) who showed that centennial GRIP ^{10}Be variations are dominated by solar activity changes and indicate only little ~~climatic influences on 10Be~~ ***sensitivity to the assumed mode of 10Be deposition*** **even over large deglacial climatic transitions.** ***Other potential climatic influences on 10Be such as changes in the stratosphere-troposphere exchange rates are, however, difficult to assess from climate proxy data and will thus, not be removed by our 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.

Agreed. We added:
"It should be noted that this statement solely refers to the filtered centennial 10Be variations investigated here."

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 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.

We changed figure 3 to the one shown below. To minimize the effect of sampling resolution differences we bandpass-filtered both records [120-500 yrs], i.e., the spectral band that is continuously resolved by both records.

We refer to this figure on P2945, L17:

"Similarly, the $\Delta^{14}C$ anomalies modelled from GRIP and GISP2 ^{10}Be agree within ± 2.5 ‰ (figure 3, c, f)."

To further clarify that P2945, L1-3 refers to each ice core separately we changed it to:

"The centennial changes in the GRIP ^{10}Be versions, however, are highly coherent and indicate limited climate influence on these timescales and the same holds true for the GISP2 ^{10}Be versions."

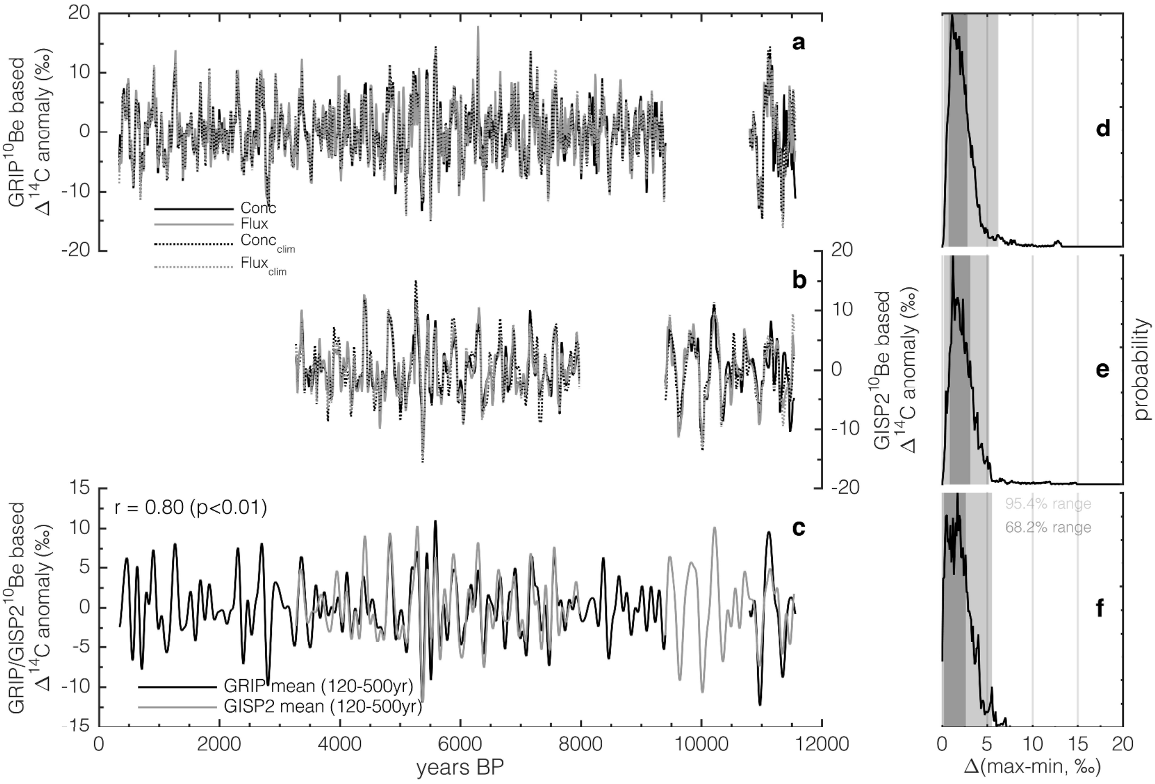

Figure 3 (revised). Centennial (<500 years) $\Delta^{14}C$ variations modelled from GRIP and GISP2 ^{10}Be data. Panels a and b show the modelled $\Delta^{14}C$ variations from ^{10}Be concentrations (solid black), fluxes (solid grey), "climate corrected" concentrations (dotted black), and "climate corrected" fluxes (dotted grey) for the GRIP (a) and GISP2 (b) ^{10}Be records. Panels d and e on the right side depict the probability density functions for the maximum $\Delta^{14}C$ difference between curves shown in panels a and b, respectively. Panel c shows the mean of all GRIP (black) and GISP2 (grey) ^{10}Be based $\Delta^{14}C$ anomalies shown in panels a and b, respectively. Panel f shows the corresponding probability density function of the maximum $\Delta^{14}C$ differences. For this comparison both ice core records have been band-pass filtered [120 – 500 years] to minimize inconsistencies arising from their different sampling resolution. The correlation between the GRIP and GISP2 records is given in panel c together with its p-value.

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.

See above. We added a panel to figure 3.

The advantage and influence on uncertainty of going from 50 year Ps_{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 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 *(?Again, something is missing here, but we assume it should read "… between the 95% confidence intervals.")*

We do provide a direct comparison of Ps_{scaled}(ts) to the transfer function in figure 9. The approach to the transfer function is outlined in section 2.5. The key difference between Ps_{scaled} and the transfer function is, that Ps_{scaled} contains high-frequency variability in the derived IntCal13-GICC05 difference which we do not believe we can reliably reconstruct. Since Ps_{scaled} is estimated on 1,000yr windows every 50 years, neighbouring windows contain about 95% of the same data. Hence, they cannot be used as independent likelihood estimates of the timescale difference. To account for this oversampling each transfer function samples only independent Ps_{scaled} estimates (i.e., based on windows that are 1,000 years apart). Each iteration of the MonteCarlo method starts from a randomly selected Ps_{scaled} estimate and then uses only every 20th window (i.e., one Ps_{scaled} estimate per 1,000 years). Hence, every individual transfer function has a lower sampling resolution than Ps_{scaled}(ts) itself. Through interpolation of the transfer functions to annual resolution, each likelihood estimate at each point in time will consist of direct sampling of Ps_{scaled}(ts) at this point in time as well as interpolated values between neighbouring windows. This can be seen in figure 9 – the transfer function is much smoother than Ps_{scaled}(ts) but it encompasses the full range of uncertainty. Consequently, the transfer function has larger uncertainty during periods where Ps_{scaled} implies rapid changes in the timescale difference such as e.g. around 7-8 kaBP, because we cannot reliably resolve the exact rate of change at this point.

Whether we interpolate the transfer functions to annual or 50yr resolution has no effect on the uncertainty, and was merely done for practical reasons, i.e., to be able to provide an annual transfer function to potential users.

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?

Yes, these are the 2sigma intervals. **We changed the figure caption and legend of figure 9 accordingly**.

Regarding the disagreement between Ps_{scaled} and the transfer function we hope we could explain the effect 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 rmsbinned) 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.

Good point. We agree, our previous formula was not complete and could be misinterpreted. **Therefore, we clarified our method by including the rearranged equation 4 into the main text:**

$$\partial(t)_{Be} = \sqrt{\partial(t)^2 - \partial(t)_{IC}^2}; \quad \partial(t) > \partial(t)_{IC}$$

$$\partial(t)_{Be} = 0; \quad \partial(t) \leq \partial(t)_{IC}$$

We think that this method is more appropriate than simply minimizing the RMSE between 14C and 10Be, since it accounts for the fact that 14C errors are increasing back in time due to the relatively short half-life of 14C. We cannot evaluate whether this uncertainty estimate is a lower bound of the true uncertainty, but it is the required error to bring 14C and 10Be into statistical agreement, which is crucial for our methodology. However, to clarify that this uncertainty estimate is only valid for the centennial (<500 year) variations in 10Be-based $\Delta^{14}C$ we added to P2950, L1-3: "In conclusion we use a ^{14}C : ^{10}Be ratio of 1.1 : 1 and an uncertainty of 4‰ for the modelled Δ14C record to derive a final IntCal13-GICC05 transfer function in the next section. **It should be noted that this uncertainty estimate is only valid for the centennial (<500 year) variations and the period studied here."**

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.

We changed PP2940, L3 to: **"For each window we test for time scale differences *(shifts)* of ± 150 years *without stretching or compression of the time scale within this window*."**

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.

Good point. We did indeed test this effect. We added on PP2940, L 4: **"We tested different window sizes between 500 and 2,000 year length and the corresponding results are consistent within error. The choice of a 1,000 year window represents a trade-off between i) an increasing statistical robustness and hence, smaller uncertainties, and ii) a loss of detail (variability) in the final transfer function (see also section 2.5) with increasing window length."**

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.

Yes this is correct. We feel that we do point this out in P2954, L 3-6 where we write:

“It can, however, not be assumed that the counting error continues to be systematic beyond this period, since the parameters used for layer identification as well as the sources of uncertainty (e.g. melt layers) differ back in time under changed climatic conditions (Rasmussen et al., 2006).”

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.

We added Figure 10 to section 3.4 (see below). We picked the sections between 3,500 – 4,500 BP (a section of relatively low amplitude Δ^{14}C changes, i.e., the variations are close to the estimated 10Be RMSE), between 7,000-8,000 BP (i.e. a section with larger Δ^{14}C variations, but not continuously good agreement between 10Be and 14C), and 10,000-11,000 BP (i.e. a section with large Δ^{14}C variations and a near perfect fit between 10Be and 14C).
We expanded the main text on P2950 (L18 onwards):
“Figure 10 shows three examples of GRIP 10Be based Δ^{14}C anomalies before (grey) and after (black) synchronization to IntCal13 (red). The examples encompass (i) a period of relatively low Δ^{14}C variability (±5-7‰) but good agreement between GRIP and IntCal13 (figure 10, a), (ii) a period of large Δ^{14}C variability (±10‰) but less good agreement between GRIP and IntCal13 (figure 10, b), and (iii) a section of large Δ^{14}C (±10‰) variability and excellent agreement between GRIP and IntCal13 (figure 10, c). It can be seen, that in all cases the fit between GRIP and IntCal13 is improved when applying the proposed GICC05-IntCal13 transfer function. However, figure 10 (b) also shows, that short periods of disagreement (i.e., around 7,250 – 7,500 years BP) may remain, as they cannot be reliably resolved by our method which matches 1,000 year-long sections. It should, however, be noted that matching these short sections would i) represent a serious violation of the GICC05 counting error which is minimal over these short periods of time (±6 years at 2σ between 7,250 – 7,500 years BP), and ii) not account for the possibility that 10Be and 14C may simply not agree due to the caveats outlined in the introduction. Furthermore, the applied shift of GICC05 in figure 10 (b) leads to an improved agreement between 14C and 10Be after and prior to 7,250 and 7,500, respectively. Hence, we consider it unlikely that for this short period of time the timescale difference deviates significantly from the estimate for the entire window.”

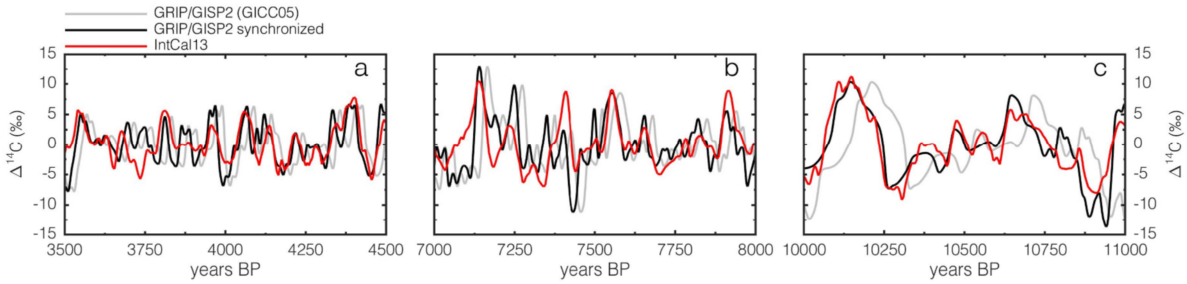

Figure 10. GRIP/GISP2 ^{10}Be based Δ^{14}C before (grey) and after (black) synchronization to IntCal13 (red) for the sections a) 3,500-4,500 years BP, b) 7,000-8,000 years BP, c) 10,000-11,000 years BP.

In the final version please specify where the IntCal13–GICC05 transfer function (and relative and absolute uncertainties) will be made available.

We will provide the transfer function as a supplementary file to this paper and on NOAA.

PP2953, L15-20: Worth to specify that the difference is in the direction of systematic over-counting of years.

We added on P2953, L17**: “…(i.e., a 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?

We **added color** to the lines in Figure 5. Regarding the size of the figures: We will provide them in full A4 size.

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 in ocean ventilation [and] air–sea gas-exchange can cause _C-14 anomalies larger than the amplitude of _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.

Agreed. We reworked the use of “both” carefully.

P2935, L22: “ideal tool” is overstating things given the climate and carbon cycle influences.

We exchanged “ideal” with “powerful”.

P2936, L11: Delete “On the other hand”.

Done.

Section 2.2: Typos: Bronk not Bonk.

Fixed.

Please state what dating of GISP2 was used: obviously it should be GISP2 on GICC05.

On P2938, L7, we added: **“We used the GISP2 ^{10}Be record on the GICC05 timescale (Seierstad et al. 2014).”**

Figure 1 Caption: Key data not Key-data. I also noticed some other examples of funny use of hyphens. Please check usage throughout.

Changed figure 1 figure caption and we will check the rest of the manuscript for similar mistakes.

Figure 2 Caption: I can’t make sense of the second last line, please revise.

Ok. Actually this sentence may not be necessary at all, since we discuss the differences between the different ^{10}Be versions (concentrations, fluxes, climate corrections) later on in Figure 3 and the corresponding text sections. **We deleted this sentence from the figure caption.**

Figure 5 caption: Description of panel b) appears to be referring to an earlier version.

No, this is indeed the correct caption. To clarify: The patch shows probabilities based on GRIP ^{10}Be, where gaps in the GRIP record have been filled using GISP2 ^{10}Be data. Hence, GISP2 can only be used as an independent validation where GRIP and GISP2 have overlapping ^{10}Be data. The 95% confidence intervals based on GISP2 ^{10}Be during these overlapping sections is plotted as lines in comparison to the patch, which is the same in all panels (GRIP ^{10}Be with GISP2 ^{10}Be filling the gaps). We hope that this is clear from the first 5 lines of the figure caption as well as from P2946, L20-22.

Note some inconsistency in x-axis labels: sometimes yrs BP and sometimes years BP.

Changed consistently to "years BP".

Add space between ka and BP, eg on PP2947.

Done.

Anonymous Referee #2
The manuscript is well-written and easy to follow. The authors have great attention for detail, which results in realistic uncertainty estimates that reflect all the probable causes of uncertainty.

Thank you.

My only concern is that it's hard to discern what is really new here. A very similar paper was published last year by the same authors (Muscheler, Adolphi and Knudsen, 2014), MAK14 hereafter. The manuscript under review uses the same 10Be and D14C data as MAK14, and while the mathematical details differ, the approach is conceptually identical (i.e. converting ice-core 10Be to 14C using a carboncycle model, and then wiggle-matching it to IntCal D14C). Unsurprisingly, the transfer function the authors derive is essentially identical to the one derived by MAK14 – only smoother due to the choice of a 1000 year window length. The main improvement is a reduction in the uncertainty estimates, suggesting that MAK14 were too conservative in estimating their error.
The aim of this manuscript is twofold.

1. We present a novel approach to synchronize radionuclide records from different archives that is applicable to a multitude of records. We anticipate that the methodology itself will be applied in further studies, and hence, we describe in detail its underlying assumptions and caveats. In addition to MAK14 we also test our results using different 10Be records (GRIP and GISP2) to illustrate the robustness of our results and methodology. Furthermore, we assess uncertainties arising from the geochemistry of 10Be and 14C explicitly which is a significant improvement compared to MAK14 which also has general implications for the interpretation of these records (i.e. for solar activity reconstructions). Hence, we think we can provide a significant conceptual advance on how to link cosmogenic radionuclide records and lie out a framework that will be of great value for future studies.

2. We provide a transfer function between GICC05 and IntCal13 for the Holocene. Compared to MAK14 this function is less (not more, see figure 7 in MAK14) smooth than the proposed transfer function of MAK14 which is based on a 2,000 year window. More importantly, we do not only provide smaller, but also more robust uncertainty estimates on the transfer function (which we have to acknowledge were not satisfactorily defined in MAK14). This is a significant improvement as reliable uncertainty estimates are crucial if the transfer function shall be applied to determine robust leads and lags in the climate system between ice core and radiocarbon dated paleoclimate records.

The work is very thorough, and I have only a few minor comments that should be addressed in a revised manuscript. I leave out the first two digits ("29") in all listed page numbers.

- I think section 2.2 (statistical method) would fit more logically between the current sections 2.4 and 2.5. When reading the section on the statistical method, the reader has no idea what is meant by "10Be-based D14C anomalies" (P38, last line). This becomes clear after reading section 2.4. An alternative solution would be to add an introductory paragraph to section 2.2 in which the conceptual framework is laid out, so the reader understands that 10Be is converted to 14C using a carbon cycle model, and then filtered to isolate the centennial component.

This is a good point. We added an introductory paragraph to section 2.2:
"In the following section we will describe the statistics involved in the $^{14}C/^{10}Be$ comparison. To be able to compare both radionuclides quantitatively, we converted the ice core ^{10}Be records into $\Delta^{14}C$ variations using a box-diffusion carbon cycle model (Siegenthaler et al., 1980;Muscheler et al., 2004b). The details of this conversion and its uncertainties are addressed in more detail in section 2.4. In the following we will refer to the modelled $\Delta^{14}C$ variations as „^{10}Be-based $\Delta^{14}C$ anomalies"."

- Due to their proximity, the GISP2 and GRIP sites should experience identical atmospheric 10Be loading; yet GISP2 receives slightly more accumulation than GRIP (about 5%). Could this help in partitioning out wet and dry 10Be deposition? The lower 10Be concentrations at GISP2 (by 0.12 atoms/g), as well as the higher 14C/10Be scaling factors (Figs 6 and 7) are both consistent with a fraction of dry deposition. I fear that the accumulation difference may be too small to do this reliably, though.

This is an interesting thought. However, to assess this reliably a detailed evaluation of the 10Be concentration / ice accumulation rate relationship would be required. For the purpose of clarity of the manuscript we would prefer not to discuss this issue extensively. We show, that GRIP and GISP2 10Be records yield similar synchronization results to IntCal13 (figure 5, b). Furthermore, 10Be concentrations and fluxes give consistent synchronization estimates (figure 5, a) indicating that the assumed mode of 10Be deposition is of minor importance for the results of this study. Regarding the slightly different 10Be scaling factors of GRIP and GISP2 records (figure 7), we think that this difference should not be over-interpreted. The GISP2 10Be record has a lower sampling resolution and a slightly higher scaling factor may just result from this difference in smoothing. Last but not least, due to slight differences in the 10Be sample preparation of both ice cores (see Finkel and Nishiizumi 1997, JGR) we cannot exclude that the small difference in the 10Be concentration reflects an interlaboratory difference.

- Section 2.1: please indicate the data resolution for the D14C data also.
We added in P2938, L18: "…and presented in IntCal13 **in 5-year resolution while the underlying data has typically a resolution of 10 years for most of the Holocene**"

- P37 L1-L4: I think the reality is more fluid than portrayed. I suspect that in practice the 10Be-14C synchronization is dominated by a few prominent events, and therefore somewhat "discrete". Likewise, continuous (rather than discrete) CH4 synchronization has also been achieved between ice cores (Mitchell et al., Science 342 964-966, 2013).

We think that a strength of the methodology applied here lies in its lower sensitivity to single events than the methodology of MAK14. The methodology of MAK14 is based on correlation analysis, which relies on the covariance of two records. Hence, this type of analysis can be dominated by single events of large amplitudes. In comparison, the methodology applied in the manuscript presented here does compare 14C/10Be data pairs in a reduced Chi2-like fashion. I.e., the values are compared directly irrespective of their covariance. This allows for example also to exploit the information "a 500 year long section of zero D14C anomaly" – while the covariance of such a section would be zero. Therefore, common variability as well as common non-variability go with similar weight into the comparison. Obviously, D14C anomalies are needed to achieve a synchronization and (in the example above) constrain the length of a section with small D14C anomalies. And we agree, that a slight dominance of these larger D14C anomalies can be expected, since these anomalies will exceed the uncertainty of the 14C/10Be records, maximizing equation 1. However, as outlined above we do think, that also relatively "flat" sections of 14C/10Be contribute significantly to the synchronization i.e. we evaluate all available information.

To tone down our statement in P2937, L 1-4, and also with respect to the findings by Mitchell et al. (2013), we changed P2937, L 1-4 to:

"… has the advantage that it can provide near-continuous estimates of the time scale differences […] or changes in atmospheric trace gases during Dansgaard-Oeschger events."

- P40, L25: "....and may thus diminish the climate influences in the 10Be record". Could it also increase the climate influences in the 10Be record, if the observed correlations are spurious?

Yes, in theory this could happen. However, the removed correlations were indeed significant. In addition, we hope we explain sufficiently well on P2940 (L26) – P2941 (L5) that these corrections are not meant to be interpreted as improved "climate free" versions of the 10Be records, but as sensitivity tests to our methodology.

- Section 2.4.1: What is the motivation to only investigate the sensitivity of the model to the oceanic carbon exchange? While the ocean is of course the largest carbon reservoir, the terrestrial carbon fluxes are actually larger than the oceanic ones. A recent paper also suggested that changes in terrestrial carbon reservoirs are more important during Holocene (Bauska et al. Nat Geo 8, 383-387 2015)

We agree that terrestrial carbon fluxes are a major component in the global carbon cycle. However, due to the short turnover rate of the terrestrial biosphere, the biologically stored carbon has essentially the same 14C signature as the atmosphere. Hence, changes in the biosphere – atmosphere CO2 exchange, do not exert a strong control on atmospheric 14C concentrations (i.e. a large flux of terrestrial carbon won't change the $^{14}C/^{12}C$ ratio in the atmosphere significantly).

- Section 2.6: I think it's important somewhere to point out that you're comparing the 14C anomalies, rather than 14C itself. These anomalies are not really well defined; from section 3.1 I assume you're using the centennial (<500 yr) variations. Please describe how your filter the records to separate the <500 and >500 yr variations.

We explain in section 2.2 (P2938, L23-26) why we use D14C anomalies rather than absolute values or 14C-ages. We consider the use of a 500 year high-pass filter a result rather than part of the method as it results from the climate and carbon cycle related 10Be uncertainties (section 3.1) which are timescale dependent and minimize for these short wavelengths.

We specified more clearly on P2946, L13:

"In the following we will compare the centennial **(i.e., <500 years separated by an FFT-based High-Pass filter)** D14C anomalies as reconstructed from tree-rings (IntCal13) and ice cores (GRIP/GISP2 10Be-based) with respect to their timescale differences. **The choice of a 500 year high-pass filter results from the climate and carbon cycle related uncertainties shown in section 3.1 which increase on longer timescales.**"

- P45, L16-17: How much is the uncertainty of 3 ‰ relative to the standard deviation of the data itself? In other words, what is the signal to noise ratio?

The standard deviation of the GRIP 10Be based D14C record is 4.5 per mille, yielding a signal to noise ratio of ca. 1.5. We'd like to point out that the uncertainty estimate is very conservative since we treat the climate influence on 10Be as a systematic uncertainty (i.e., Figure 3 shows the difference between the maximum and minimum D14C value at each point in time, as opposed to the standard deviation of all 10Be versions).

- P48, L21: "this would imply a strong polar bias". Please elaborate, this is not automatically clear.

We introduce and discuss the issue of a polar bias in the introduction and in section 2.4.2. To make it more clear we changed L21 on P2948 to: "**Assuming that the centennial 10Be and 14C production rate changes are mainly modulated through solar activity this low scaling factor would point to a strong polar bias of the GRIP GISP2 10Be records (see sections 1 and 2.4.2)**.

- The generated transfer function should be provided as a text / excel file in the supplement.

Yes, it will be.

- Typos / Language:

P35 L15 and throughout: acronym should be capitalized, so GCR instead of gcr.
Done.

P36, L4: 14C / 12C *ratio*
Added.

P38, L23: Please define what is meant by "D14C anomalies". I don't think this is done anywhere in the manuscript.
We define D14C on P2938, L16 and think that the term "anomalies" is a general term that describes deviations from a mean. As outlined above, we think that the use of centennial anomalies (i.e., deviations from a 500 year low-pass filtered D14C record) is a result of section 3.1, which may partly differ if the method was applied to a different 10Be record, with different climate influences. We do outline on P2938, L23-26 why absolute D14C values cannot be used and hope that our definition of anomalies becomes clear throughout the manuscript and with the additions on P2946, L13 (see above).
To increase the clarity of the manuscript we added on P2938, L 26:
"Given the results shown in section 3.1 we employ centennial (<500 year high-pass filter) Δ^{14}C anomalies of the tree-ring and the 10Be-based Δ^{14}C records for this comparison as shown in figure 3."

P39, L4 and L8: Bronk Ramsey ("r" omitted)
Fixed.

Throughout there are long sentences that would benefit from inclusion of a comma to clarify sentence structure. Some examples:
P35 L23: After production, ... P36 L11: On the other hand, ... P36 L28: ... synchronization tools, ... P40 L19: ... to the ice sheet, ... P41 L18: ... these effects, ... P45 L26: ... as before, ...
Reworked.