Point to point replies:

Referee 1) Interactive comment on "^{10}Be in late deglacial climate simulated by ECHAM5-HAM - Part 2: Isolating the solar signal from 10Be deposition" by U. Heikkilä et al.

2 Specific comments

2.1 Imprecise objectives: "Distortion of the solar signal" needs detailed specification

The paper "focuses on the level of distortion of the solar signal in ^{10}Be deposition due to deglacial climate changes" (p. 5631, l.5-7) which is also outlined in the paper title. However, although being the overall objective of the study, this "distortion" of the ^{10}Be production signal remains unspecified. Two different kinds of climate noise could distort the ^{10}Be production signal involving very different implications. At first place, an obvious distortion would arise from climate processes which explicitly modulate the ^{10}Be deposition in a frequency range comparable to production changes (and thus mask or simulate production changes). Second, climate and meteorological processes may significantly reduce the signal-to-noise ratio through modulating the ^{10}Be deposition on time-scales different to the production signal (i.e. sub-annual). In case of the former, the ^{10}Be production signal cannot be separated from ^{10}Be time series without detailed information on the climate processes. In case of the latter, the noise hampers the detection of the production signal but time series analysis methods basically allow for elimination of high-frequent climate noise. In the present manuscript, it is not easy to figure out which kind of distortion (or both?) is investigated. So far I cannot decide if this arises from unclear presentation of the objectives (and imprecise language) or a deficient strategy.

Summarizing the study in their abstract, Heikkilä et al. state that "the production signal varies on lower frequencies [...] climatic noise is of higher frequencies" (abstract, l. 11-13). It is however unclear if this is a main finding from the model simulations or an a priori assumption. In the methods part the authors state that "climate related changes [...] act on sub-annual time scales. Long-term trends in climatic variables are also possible but were not found during the relatively short simulations of 30 yr each" (p.5632, l.21-24). But then in the summary part, the authors note that the "climatic distortion [...] is assumed to be represented by the highest frequencies" (p. 5640, l. 3-4). Either way it remains unclear where this assumption/finding comes from since simulated precipitation rates - both globally (Figure 3) and locally (Figures 5 and 6) - indeed show significant multi-annual variability up to the decadal scale. In addition, features of the simulated ^{10}Be deposition (like the delayed second production minimum in the 11k simulation at GRIP, p. 5636, l.9-11) are attributed to multi-annual features of the precipitation rate. Nevertheless, within EEMD analysis, sub-annual variations are attributed to climatic noise whereas multi-annual changes are generally considered as solar signal (p. 5636). It thus seems that the authors focus on high-frequent climate noise which "distorts" the ^{10}Be

production signal. However, if this is true, it seems odd that model results are generally smoothed with a running mean filter previous to data analysis (see also specific comment 2.2 for the validity of running-means filtering). In doing so, the share of the ^{10}Be deposition variability explained by the solar signal is of questionable validity since a large share of variability (not only seasonal) is eliminated previous to analysis. On the contrary, climate modulation of the seasonal cycle in the ^{10}Be deposition (the dominant "noise") would be of major interest but is not investigated.

In summary, I strongly recommend revising the presentation of the objectives and the results. Does the study aim at investigating the entire frequency range of climate modulation or the high-frequent distortion only? Or else, are both aspects investigated in different parts of the study? In either case main assumptions and findings should be clearly separated and presented in detail.

We would like to thank the referee for pointing out that the background of the analysis was not explained clearly enough. We now listed a detailed description of the "noise" we're ridding the data of, and what kind of frequencies it can consist of. In short: "noise" in this study is any variability which is not caused by the solar signal.

Time scales related to the components:

1) Signal (solar):
1.a) sub-annual (monthly): none (filtered out by the long residence in the stratosphere)
1.b) annual and longer: the solar signal. The 11-year cycle has an amplitude of ca. 30%. The cycle becomes identifiable over roughly 3 years.

2) Noise (mainly precipitation as shown in section 3.1):
2.a) sub-annual (monthly): seasonal cycle related to the seasonal variability present in virtually all atmospheric variables, precipitation, temperature etc. Amplitude ca. 400-500%.
2.b) annual and longer: possible long term trends or multiannual shifts from the mean in precipitation, or variables related to atmospheric transport.

Our aim is to isolate the solar signal from the ^{10}Be data. Because the seasonal cycle has no solar origin and it overrules solar variability we first remove it. The time resolution of ice core ^{10}Be data is typically annual or coarser and we want to establish a method to be applied for observational data as well. The corresponding way of treating model ^{10}Be data would be calculating annual means. Unfortunately with only 30 years of data this is not possible. We decided to use the 25-month running means (see also reply 2.2).

After removing the seasonal cycle we analyse ^{10}Be variability for various frequencies with the aid of EEMD. The resulting IMFs we identified as either signal or noise. The use of model data with a known solar signal allows us to compare the IMFs with the original production signal and thus separate the noise from it. With noise components removed, the remaining deviation from solar signal can then be associated with long-term variability of variables related to atmospheric transport, mostly precipitation.

We now specify in the text what is meant by signal and noise, and time components related to it. We also mention when a-priori assumptions are made.

2.2 Usage of simple running mean filters (in combination with EEMD)

Simple running mean filters are a straightforward tool to investigate multi-annual variability of climate time series at first order. However, they are unfeasible for low pass filtering since they produce a significant amount of high-frequent noise. In case of a running mean filter, the smoothing kernel is a box function and hence its Fourier transform shows significant oscillation at higher frequencies. In convoluting the box function kernel with the time series under investigation, these 'wiggles' preserve a significant part of the time series spectrum at high frequencies but also delete a major part of the respective spectrum. The high frequent oscillations which remain in the smoothed time series thus elude a straightforward physical interpretation. If this is true this would also hold for the first intrinsic mode functions which are attributed to "climate noise" as well as the variability explained by this data. I encourage the authors to show that usage of running mean filters is reasonable with respect to their subsequent time series analysis and interpretation of high frequencies.

We agree that choosing a way of removing the seasonal cycle in combination with EEMD is a tricky issue which we tested in many ways. The starting point of the analysis is the following: we have 30 years of monthly mean data which we want to analyse with EEMD for frequencies higher than 1 year. The monthly mean data is dominated by the seasonal cycle which overrules any other variability by a factor of ca. 5. Calculating annual means would be the obvious choice, but that would reduce our data to 30 points which is not feasible. For low pass filtering a fairly low frequency threshold should be used to get completely rid of the seasonal cycle. We however did not want to mix two different kinds of filters. We then picked running mean in order not to filter out frequencies prior to the analysis and to keep the number of data points large enough. The high frequency noise produced by the running mean, correctly mentioned by the reviewer, will then fall into the category of "high-frequency climatological noise". A 25-month threshold was chosen because shorter ones did leave some of the seasonal variability into the data. This method was also found successful in our previous work published in Heikkilä et al., JGR, (2013). This is now explained in the text.

2.3 Information on the methods applied

I agree that the authors refer to the accompanying paper for details on the meth- ods. However, the first paper (Heikkilä et al., 2013) lacks some basic information on the ECHAM5/CSIRO Mk3L model setup which becomes first relevant in the present manuscript. Different to the study on mean climate conditions and their influence on the [10]Be deposition, model performance regarding temporal climate variability becomes a significant information. Heikkilä and Smith (2013) have shown that ECHAM5 is capable to reproduce large-scale features of the NCEP reanalysis data (as e.g. the North Atlantic Oscillation or the Southern Annular Mode). However, their simulations were based on prescribed observational monthly mean sea surface temperatures and sea ice cover. For all readers being non-specialists on Global Climate Models: Can we expect the

same model performance if the model input is based on CSIRO Mk3L model results? Furthermore, detailed information on the ^{10}Be production variability is important but missing in the current manuscript. Finally, while EOF-analysis is indeed a kind of standard method in geosciences, this does not (yet) holds for EEMD. Here some more details on the method would be helpful (i.e. input parameters or assumptions influencing the results), especially since its handiness/simplicity seems to be a major advantage over other time series analysis tool (like e.g. wavelet analysis).

The state of climate during the last deglaciation is much less known than at present due to lack of direct observations. Large scale climatic oscillations which contribute to temporal variability on time scales from annual to decadal, as investigated in this study, are the ENSO, NAO and SAM, however their strength is typically investigated as a combination of proxy data and earth system model simulations driven by this proxy data, which obviously is uncertain. Therefore, earth system models, such as the CSIRO-Mk3L model climate in this particular study, cannot be validated against observations, as is common for simulations of modern climate. However, the CSIRO-Mk3L model is a state-of-the-art climate model and presents our best knowledge of climatic processes during glacial climate. The ability of the model to simulate climate beyond modern times has been evaluated against a mean state of climate as well as for the response to external forcings in Phipps et al., (2012a and b). This is now mentioned in the text.

As requested by both referees, more details on the ^{10}Be production and how the solar modulation function for this period was created have been introduced.

The EEMD method is introduced in the end of the Methods section. Further assumptions or free parameters have not been used for this analysis.

2.4 The ^{10}Be snow and air concentration

I understand that it is much more difficult to model the ^{10}Be snow concentration than the ^{10}Be deposition flux. However, it is not mentioned that this is still a major drawback of the model simulations which requires further work. The ^{10}Be deposition flux cannot be measured directly in ice cores but is deduced from ice concentration measurements and reconstructed accumulation rates. Indeed, the so-derived ^{10}Be deposition flux has successfully been used for ^{10}Be-based reconstructions of solar activity during the Holocene period. However, on longer-time-scales (i.e. the last glacial period) snow accumulation rates are difficult to assess and show major variations. Future model studies should therefore also work towards a proper understanding of the ^{10}Be snow concentration. Here, from my point of view, presentation of ^{10}Be air concentration model results could be very helpful for the understanding of ^{10}Be snow concentration changes. Atmospheric transport of ^{10}Be has been proven to largely influence the ^{10}Be snow concentration (e.g. Pedro et al., 2011). Climate modulation of e.g. the seasonal cycle of the boundary layer ^{10}Be air concentrations does most likely also hold for the ^{10}Be snow concentration. Please mention that model simulations of the ^{10}Be deposition flux are not the end of the story and give some notes/details on the ^{10}Be air concentration.

We agree that understanding the relationship between snow concentrations and

deposition flux is an important issue which requires much attention in the future. Globally, the deposition flux has to be equal to the production, independent of climate, and while local deviations are possible, on longer time scales these are likely to average out. This is the theory. Using ice core data, reconstruction of the snow accumulation rate is affected by uncertainties in dating which can lead to errors in the deposition flux. Snow concentrations are influenced by changes in the snow accumulation rate, which largely corresponds to the precipitation rate. This is seen as a shift in ^{10}Be snow concentrations in the transition from glacials to interglacials, for instance. Furthermore, post-depositional changes in ice flow and dynamics bias the snow concentrations as well. Uncertainties in dating can cause spurious peaks in the snow concentration. The two latter are questions which cannot be investigated with atmospheric modelling which comprise only the atmosphere, not the snow pack. They require models of the cryosphere.

The connection between air concentrations and snow concentrations is yet one step more complex. ^{10}Be is produced in the upper atmosphere, where it resides ca. 1 year before transport to the troposphere and ultimately deposition. It is obvious that atmospheric transport influences deposition patterns and finally snow concentrations for example due to the fact that the meridional distribution of production and deposition is completely different: production is at maximum at high latitudes whereas deposition is at minimum! The process of transport from the atmosphere into the ice is complex: air concentrations in the troposphere are largely influenced by weather processes, formation of clouds and vertical transport of particles within clouds, precipitation which ultimately washes out ^{10}Be from the atmosphere, dry deposition taking place at surface, or gravitational settling which takes place in the entire atmospheric column (they are all considered in the model and included in the deposition flux). Precipitation and thus ^{10}Be deposition takes place at the level of clouds, below cloud scavenging on the way down plays only a minor role (ca. 5%). Parameterisation of washout processes of aerosols in climate models is complex and depends on many atmospheric variables and is under heavy research by many groups based on both theory and laboratory experiments.

Therefore, air concentrations are connected to deposition flux/snow concentrations at the level of clouds at the time of precipitation events, but after precipitation events this air is rapidly mixed with other levels. Surface air concentrations, for example, are thus not directly connected with deposition flux. Hence the closest parameter to snow concentrations in atmospheric models is the deposition flux which gives the amount of ^{10}Be deposited into the snow. Transferring the deposition flux into snow concentration requires understanding of snow accumulation and ice flow, as discussed above, but air concentrations are not necessarily aiding in this. We now highlight the difference between deposition flux and snow concentration, and mention that snow concentrations are not as easy to interpret as deposition fluxes and difficulties connected to that (beginning of section Results: EOF analysis of global ^{10}Be deposition).

2.5 Detailed comments

p.5628, l.11-13

"The production signal varies on lower frequencies, dominated by the 11 yr solar cycle

within the 30 yr time scale of these experiments. The climatic noise is of higher frequencies" Is this statement on climate noise an assumption or a finding of the study? Specify "lower frequencies" and "higher frequencies"! See also comment 2.1!

Please see the reply 2.1, we have now clearly defined what the "noise" refers to in this study.

p.5628, l.22-23

"The high frequency components represent climate driven noise related to the seasonal cycle of e.g. precipitation..." This might be nitpicking but is there any evidence for the noise being related to the seasonal cycle?

Please see the reply 2.1. The solar signal does not vary seasonally, therefore the seasonal signal is noise.

p.5628, l.24-17

"These results firstly show that the ^{10}Be atmospheric production signal is pre- served in the deposition flux to surface even during climates very different from today's both in global data and at two specific locations." "Preserved" seems not to be the right expression. If it is assumed that "the cli- matic noise is of higher frequencies" (p.5628, l.13) it is quite obvious that the signal is preserved. See also comment 2.1!

Please see the reply 2.1. Furthermore, there still is a fair amount of doubt within the community whether cosmogenic radionuclide variability is modulated by climate only, or also the solar/geomagnetic signal. Therefore we find it worth mentioning that the solar signal is preserved.

p.5628, l.27-30

"Secondly, noise can be effectively reduced from ^{10}Be deposition data by simply applying the EOF analysis in case of a reasonable large number of available data sets, or by decomposing the individual data sets to filter out high-frequency fluctuations." Is this finding really that innovative to be stated in the last sentence of the abstract?

The position of this sentence in the abstract was by no means suggesting that this is the first time that PCA is used to analyse ^{10}Be data. It merely concludes the findings of our study, making it natural to finish the abstract with it. Still, applying PCA to such a large number of ^{10}Be data that meaningful spatial patterns are obtained is only possible using model data and this was the first time it was made. With observations this is not possible due to their limited number.

p.5632, l.20

"...but these are efficiently filtered out by the atmospheric transport from the stratosphere to the troposphere." The authors might like to cite Usoskin et al. 2009:

Short-term production and synoptic influences on atmospheric 7Be concentrations. Journal of Geophysical Research, 113, D06108.

We acknowledge the valuable work by Usoskin et al., (2009) investigating the propagation of SEP-produced ^{7}Be into the troposphere. This sentence, however, refers to the small-amplitude wiggles of the solar modulation function around the 11-year cycle, which cause equal wiggles in the ^{10}Be production. Our modelling finds that due to their small amplitude, none of these wiggles are visible in the deposition, or air concentrations. We now clarify that this is a finding of the model.

p.5632, l.22-24

"Climate related changes [...] act on sub-annual time scales. Long-term trends in climate variables are also possible but were not found during the relatively short simulations of 20 yr each." What about the multi-annual (up to decadal) variations of the simulated precipi- tation rate (Fig.2, 5 and 6)?

Please see reply 2.1. In addition we corrected this sentence to: "long-term trends and variations"

p.5632, l.27-28

"We aim to analyse the raw data without applying any averaging of filtering. How- ever, seasonal fluctuations of ^{10}Be data are of much larger amplitude than solar modulation and have to be removed. We apply a simple 25 month running mean to smooth out the seasonal cycle..." I agree that disentangling of lower order oscillations inherent to time series is much easier if the dominant oscillations are removed. However, I cannot follow why this is done by using a smoothing filter which removes a lot more frequencies than the seasonal cycle. See also comment 2.2!

Please see reply 2.2.

p.5633, l.24 - p.5634, l.1

"We apply EOF analysis for the three-dimensional deposition field with all four simulations combined to produce the common EOFs for each simulation." I do not understand what is done here. Why are the simulations combined? Please explain more details!

The simulations can be analysed either separately or combined to a continuous time series. When the simulations were analysed separately the EOF analysis produced similar patterns for each simulation, but they were not equal. We wanted to force the same EOF patterns in order to analyse how much of the variability was explained by these particular patterns to be able to compare the simulations.

p.5634, l.3

"The first EOF (top panel) explains 64% of the variability..." See comment 2.2! What is

the validity of this number? It is neither the explained total variability since results are smoothed previous to EOF analysis, nor the explained variability of multi-annual data since the running mean filter does not eliminate all frequencies higher than 1 yr-1.

Statements of how much of the variability are explained by a process are common when observations are analysed. Any statement of variability explained by a signal is of course valid only in relation to the time resolution, in case of both observations and model results. We now stress the time scale when this results is mentioned: "... on annual scale with the seasonal cycle removed".

p.5635, l.22-23

"The mean value of ^{10}Be deposition only varies by ca. 5% between these sta-tions." Values in Figure 4 suggest higher variations. Do you mean between simulations?

Corrected to "between the simulations"

p.5636, l.27-29 and p.5637, l.1-2

"The IMF5 is closest to the ^{10}Be production signal, exhibiting the three ca. 11 yr solar cycles. However, the first cycle of IMF5 is shorter than the solar one for which the IMF6 contributes by creating the broader shoulder seen during the first third of each 30yr period. This suggests a stronger climate impact on the ^{10}Be deposition during this Period..." This statement contradicts the finding that "IMFs (4-8) are considered to repre-sent the solar signal" (p. 5636 l.16) as well as "the reconstructed production signal from the ^{10}Be deposition (IMF 4-8) ..." (p.5637, l.14)? Please explain why IMFs (4-8) are generally considered as production signal but could also give hints on climate modulation. See also comment 2.1!

Please see reply 2.1. The solar activity varies only on long time scales, the precipitation generally on shorter scales but could have longer term trends, too. This is now explained in the introduction.

p.5636, l.21-24

"We therefore aim to create a standard methodology based on physically justified thresholds which can be applied to any data without prior knowledge of the re-constructed signal." I do not understand to what item in the paper this sentence is referring to. Is this standard methodology applied in the paper or shall the paper provide this methodology?

The aim of this analysis is to provide such a methodology ("we therefore aim to create..."). It is tested in this study, and because the results seem reasonable, the methodology can be tested for observational data as well.

p.5637, l.24-25

"Generally the variability seems similar in all simulations and both stations". This

statement cannot be shown by Figure 10, since standardized data has stan- dardized variability.

The simulations were combined, and then standardised. Therefore, a potential difference in variability in one simulation could be identified. This is now mentioned.

p.5637, l.25

"Both noise components seem correlated". Be more precise. Are they correlated or not?

Corrected to: "are correlated"

p.5641, l.12-15

"The EEMD method [...] was successful in noise reduction and resulted in a de- position signal closer to production, explaining >95% to total variability in each simulation, than can be obtained by a simple low pass filtering or smoothing" I cannot follow this argument. If the EEMD method is used to cutoff high- frequency variability only, what is its advantage over low-pass filtering?

The benefits of EEMD are listed in the Introduction: EEMD decomposes the data set in various frequencies, which can then be used in a way that serves best the process to be isolated. The frequencies of the components are not constant over the analysed time period. This does allow for a more sophisticated way of filtering data than using a pre-set constant frequency threshold. When preparing the manuscript we tested the EEMD results against lowpass filtered ones and found higher correlation coefficients with EEMD filtering.

3 Technical correctios

Figures

Please use axis labels! This is especially helpful since you switch between absolute, normalized and standardized data.

We agree that labeling axis is good practise. In our case including labels into the figures is tough because the figures consist of a number of fairly small subplots and the x-axis had to name the simulations as well. Text large enough to be readable did not fit between the figures. However, we now double-checked that all figure captions and titles explain the axis and no information is missing.

Fig.2 axis

Give the meaning of the right-hand axis (^{10}Be production rate?). Left-hand axis of the mid layer is misleading. Fig.2 and 3, label Are you sure that you mean "normalized" and not "standardized"?

The axis on the right hand side is now explained in the caption. The data is indeed

normalised, not standardised.

p.5631, l.3-4

Replace "used in time series analysis, such as surface temperature..." by "used to analyse time series such as surface temperature..."

Changed.

p.5635, l.2-3

Replace "the method" with "this method" to enhance readability

Changed.

p.5635, l.9

"Typically these complications..." This reference across two sections degrades readability

Changed to: "The complications discussed in the previous section"

Referee 2) Interactive comment on "[10]Be in late deglacial climate simulated by ECHAM5-HAM - Part 2: Isolating the solar signal from 10Be deposition" by U. Heikkilä et al.

General comments:

1. The authors analyze data simulated by their model. However, the issue of how well the simulated data represents real measurements is not addressed. In fact, what studied is propagation of the 11-yr signal throughout the authors' climate model. First, the authors need to show (or at least briefly discuss with references to more detailed papers) that the model is able to reproduce the observed variability of real beryllium data at this timescale, as made e.g. by Usoskin et al. (JGR, 2009). I do know that the authors have something to show here and that their model is quite reliable in this sense, but this should be done explicitly, for the benefit of a reader. Without showing that the model does reproduce [10]Be variability at inter-annual time scale, the study does not make sense.
The ability of the model to reproduce observed inter-annual variability of modern large-scale climatic modes, such as NAO, ENSO and SAM has recently been presented in Heikkilä et al., (2013). Other studies (e.g. Heikkilä et al. 2008, Pedro et al., 2011) address the ability of the model to reproduce observed [10]Be variability over a multi-year period. Phipps et al., (2012 a and b) discuss the CSIRO Mk3L model. Main findings of these studies are now summarised in the text.

2. Even though this is a Part 2 paper, the authors need to describe (at least briefly) the details of the model and [10]Be production.
See also comments to referee's specific comment 4. Details have now been included into the text.

3. The authors focus on the 11-yr solar cycle for the period of 12 kyr ago. Sufficient time resolution of ^{10}Be data is not yet available for that period. Typically, only 10-yr, 22-yr or 50-yr averaged solar activity is reconstructed from cosmogenic isotopes, without pretending for resolving individual cycles (see works by Steinhilber, Beer, McCraken, Usoskin, Solanki, Muscheler, etc.).

Correct, the time scales of observational and model data often do not match. Observed data of the last deglaciation is not well enough resolved to be analysed for the 11-year cycle. This is when model studies come in handy, allowing for studying signals which are not (yet) resolved in observations.

4. How is natural quasi-decadal variability of the climate accounted for? How much it is known for the deglaciation?

The state of climate during the last deglaciation is much less known than at present due to lack of direct observations. Large scale climatic oscillations which contribute to temporal variability on time scales from annual to decadal, as investigated in this study, are the ENSO, NAO and SAM, however their strength is typically investigated as a combination of proxy data and earth system model simulations driven by this proxy data, which obviously is uncertain. Therefore, earth system models, such as the CSIRO-Mk3L model climate in this particular study, cannot be validated against observations, as is common for simulations of modern climate. However, the CSIRO-Mk3L model is a state-of-the-art climate model and presents our best knowledge of climatic processes during glacial climate. The ability of the model to simulate climate beyond modern times has been evaluated against a mean state of climate as well as for the response to external forcings in Phipps et al., (2012a and b). This is now mentioned in the text.

Specific comments:

1. Page 5629, line 5. A reference to a broad review is recommended here, e.g., a book by Beer et al. (2012) or a review by Usoskin (LRSP, 2013) rather than specific papers.

References added.

2. Page 5630, line 15. An idea that cosmogenic nuclide variability can be separated in the frequency domain into high-frequency solar and low-frequency geomagnetic components was proven invalid (see., e.g., Snowball Muscheler, 2007; Usoskin, 2013). There are low-f variations of solar activity and high-f of the geomagnetic field.

This is true. This information has been included into the text: "However, the separation is hard, because there are also low-frequency variations in solar activity and high-frequency variations in geomagnetic activity, too."

3. Page 5630, line 18. reference to Hathaway (LRSP, 2010) would be more appropriate here than Richardson et al. The cycle length varies not as 11 ± 1 yr but between 8-9 and 13-14 years (or even up to 16 yrs during Grand minima if we trust the results of Miyahara et al.).

A reference to Hathaway was added. We mean that the length of the 11-year cycle varied by +-1 year in this particular simulation. Of course, it can vary even more. This is now

clarified: "...the length of the cycles has been found to be non constant (from 8-9 to 13-14 years)".

4. The model used for ^{10}Be production is unclear. The production curve must be shown and fully explained. I was unable to find details of the production neither here nor in Paper 1, nor even in a paper referred to in Paper 1. The authors refer to ^{14}C reconstruction (page 5631, line 26) concerning the production during the last deglaciation, but ^{14}C-based reconstructions are not available outside the Holocene. In the next line it is said that the modern 11-yr cycle is added ON TOP of that. This sounds inconsistent. The 11-yr cycle should be added not on top of the mean production but around the mean. Is it what is meant? Moreover, it is unclear why modern cycles are added to some reconstructed level. Why cannot the authors use just the modern production?
The production curve is shown in Figure 2 (in green), which is now mentioned in the text as well. We did make an effort to reconstruct the ^{10}Be production in deglaciation to the best of the current knowledge, based on ^{14}C. Due to the coarse resolution of ^{14}C the general level of production is the very similar for 10k, 11k and 12k. The 11-year cycle is smoothed out, but still this production rate is our best guess up to date. However, there are still unresolved, climate-driven differences between ^{14}C and ^{10}Be during the last deglaciation. Our production rate is thus too uncertain to be called a new reconstruction for this period and we prefer to call it theoretical, however it is likely to be closer to the truth than just the modern production.

The production was interpolated to the solar modulation parameter Phi from the calculations by Masarik and Beer, (2009). The parameter Phi was reconstructed as explained: the mean was obtained from the levels of ^{14}C, and a modern 11-year cycle was added on top (the production rate was first normalised so that the mean value was not affected). These details are now included into the text.

5. In Figures 2 through 10, visual gaps are needed between the runs to guide the eye that the sub-panels represent different periods of time. Presently it looks like a continuous curve. The time scale should be shown in the X-axis. Presently it's totally unclear for a reader what is the time span of the panels.
The black lines were added as dividers between the simulations. We wanted to identify the simulations on the x-axis, which leaves us with limited space for the time scale. The time is the same as in the simulations: 30 years in monthly mean values. Because these are time slice simulations, it's just pseudo-time during the conditions given by greenhouse has concentrations in each of the simulations. Therefore we added an explanation of the time scale into the caption of the figures, mentioning that it is not continuous and that the black lines are dividers between the simulations.