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Climate of the Past Discussions
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Interactive comment on "^{10}Be in late deglacial climate simulated by ECHAM5-HAM – Part 2: Isolating the solar signal from ^{10}Be deposition" *by* U. Heikkilä et al.

C. Elsässer (Referee)

christoph.elsaesser@iup.uni-heidelberg.de

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1 General comments

Heikkilä et al. investigate the ^{10}Be deposition flux during the late deglacial climate by using the Global atmospheric aerosol-Climate Model ECHAM5-HAM (forced with CSIRO Mk3L model results). Their study is sub-divided into two parts: In a first paper Heikkilä et al. (2013) addressed the time-averaged effect of different climate conditions on the global atmospheric circulation of ^{10}Be. The paper under review presents the second part of the study. It focuses on the climate modulation of the solar production

signal inherent to the ^{10}Be deposition flux, both, on the global scale as well as on two key ice core drilling sites.

Atmospheric ^{10}Be is a widely-used proxy in paleoclimate research tough its interpretation can be significantly improved by model simulations. While ^{10}Be ice core records covering the last glacial period still lack proper understanding, ^{10}Be global circulation model attempts were so far restricted to the Holocene period (e.g. pre-industrial or Maunder Minimum period). The current study by Heikkilä et al. is thus an important contribution to scientific progress and fits the scope of 'Climate of the Past'. In my opinion, it is a good idea to sub-divide the study into two papers since both aspects (time-averaged effect on ^{10}Be and its temporal modulation) are complex and challenging objectives. However, while I enjoyed reading the first paper of the study (Heikkilä et al., 2013), I raise major concerns on the here-reviewed second part. Although I basically recommend that the model results should be published, the manuscript requires major revision. From my point of view, imprecise presentation of the objectives and results (specific comment 2.1), significant shortcomings of some methods (specific comment 2.2) as well as further details (specific comments 2.3 and 2.4) require major review. Nevertheless I want to encourage the authors to present a revised version of the manuscript since the presented model simulations are so far one-of-a-kind.

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

CPD
9, C2790–C2800, 2013

^{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

CPD
9, C2790–C2800, 2013

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.

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

CPD
9, C2790–C2800, 2013

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.

2.3 Information on the methods applied

I agree that the authors refer to the accompanying paper for details on the methods. 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).

CPD
9, C2790–C2800, 2013

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.

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"

CPD
9, C2790–C2800, 2013

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!

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?

p.5628, l.24-17

"These results firstly show that the ^{10}Be atmospheric production signal is preserved 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 climatic noise is of higher frequencies" (p.5628, l.13) it is quite obvious that the signal is preserved. See also comment 2.1!

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?

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.

CPD

9, C2790–C2800, 2013

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 precipitation rate (Fig.2, 5 and 6)?

p.5632, l.27-28
"We aim to analyse the raw data without applying any averaging of filtering. However, 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!

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!

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.

p.5635, l.22-23

CPD
9, C2790–C2800, 2013

"The mean value of ^{10}Be deposition only varies by ca. 5% between these stations."
Values in Figure 4 suggest higher variations. Do you mean between 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 represent 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!

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 reconstructed 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?

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 standardized variability.

p.5637, l.25
"Both noise components seem correlated". Be more precise. Are they correlated

CPD
9, C2790–C2800, 2013

or not?

p.5641, l.12-15
"The EEMD method [...] was successful in noise reduction and resulted in a deposition 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?

3 Technical corrections

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

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"?

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..."

p.5635, l.2-3
Replace "the method" with "this method" to enhance readability

p.5635, l.9
"Typically these complications..." This reference across two sections degrades readability

CPD
9, C2790–C2800, 2013

p.5637, l.16
Replace “flattens” with “smoothes”?

4 Literature

Heikkilä, U., Phipps, S.J., and Smith, A.M., 2013. ^{10}Be in late deglacial climate simulated by ECHAM5-HAM - Part 1: Climatological influences on 10Be deposition. Clim. Past, 9, 2641-2649.

Heikkilä, U., and Smith, A.M., 2013. Production rate and climate influences on the variability of ^{10}Be deposition simulated by ECHAM5-HAM: Globally, in Greenland, and in Antarctica. Journal of Geophys. Res., VOL. 118, 2506-2520.

Pedro, J.B., Heikkilä, U.E., Klekociuk, A.,Smith, A.M.,van Ommen, T.D., and Curran, M. A. J., 2011. Beryllium-10 transport to Antarctica: Results from seasonally resolved observations and modeling. Journal of Geophys. Res., VOL. 116, D2312.

Interactive comment on Clim. Past Discuss., 9, 5627, 2013.