Reply to reviewers

We would like to thank both reviewers for their helpful comments and suggestions. In the following we respond to all comments in detail; please see the discussion paper for references (some additional references are given at the end of the reply).

Review 1.

The authors put a lot of effort into validating and improving their model with different relevant data sets. I think this is good but the authors are probably well aware of the fact that a calibration in space (and today's climate) might not work for ice age climate. For example, the Antarctic-Greenland difference is interesting (figure 5). However, it might hint that during the ice age different relationships between accumulation and ^{10}Be apply. So can we confidently infer a dry deposition velocity for ice age climates? Wouldn't the dryer ice age climate tend to support the use of the Antarctic calibration?

We agree that our model calibration describes the observational period conditions of atmospheric transport and deposition and does not simulate ice age climate conditions. So far the influence on climate changes on radionuclide transport is not known. Therefore, our aim was not to reproduce glacial conditions of transport and deposition but to investigate if multi-millennial changes of measured ^{10}Be ice concentration could be explained by changes of polar precipitation/snow accumulation and cosmogenic production, only. Latest studies on ^{10}Be-based reconstructions of snow accumulation or the geomagnetic field (e.g. Wagner et al., 2001b; Muscheler et al. 2005) do not consider any climate related changes of the ^{10}Be transport or deposition processes. In fact they use relative variations of the ^{10}Be flux to infer production or snow accumulation changes. With our quantitative model of the ^{10}Be cycle we show that this procedure needs review.

We acknowledge the reviewer's hint regarding the Greenland-Antarctic calibration difference. Indeed our model would be well suited to investigate different calibration scenarios based on assumed climate scenarios. However, given the added complexity this attempt is beyond the scope of the present publication.

It does not get clear which production rate models are used for the calculations shown e.g. in figure 7. The authors state that the widely different models (Masarik & Beer versus Kovaltsov and Usoskin) have similar implications for the polar ^{10}Be deposition (I guess the latitudinal differences must somehow be compensated due to atmospheric mixing). I think this discussion should be expanded so that one can follow why different input data leads to similar results. Obviously, this is somehow surprising and I was wondering if the same compensation can also be expected in an ice age climate (e.g. with expanded polar vortex). Later on in the manuscript it does not get clear which calculations are used. I think this should be stated and the uncertainties connected to it should be included.

Indeed, a main result of our atmospheric transport model calculation is that – different to the global scale - both production rate calculations show comparable geomagnetic modulation of the polar ^{10}Be air concentration. This finding is addressed in section „2.1.4 The ^{10}Be production signal in polar areas". We redraft this section into:

„Certainly, this discrepancy is based on different geomagnetic modulation of the global mean ^{10}Be inventory: Both production rate calculations differ in their latitudinal shape of the ^{10}Be production (i.e. their dependency of ^{10}Be production from cutoff rigidity) which implies a different geomagnetic modulation of the global mean ^{10}Be production. However, this difference is related to mid and lower latitudes and there is obviously no difference at polar latitudes (no geomagnetic shielding, i.e. zero cutoff rigidity at polar latitudes). Indeed, our atmospheric transport model results show that – different to the global mean ^{10}Be - the geomagnetic modulation of polar ^{10}Be is similar in case of Masarik and Beer (2009) and Kovaltsov and Usoskin (2010) production rates. The reason for this finding is that polar areas do not receive a globally mixed production signal. This atmospheric transport effect makes polar ice core ^{10}Be-based reconstructions of past geomagnetic activity less sensitive to the choice of ^{10}Be production rate calculations."

In addition, we now explicitly state that we use Kovaltsov and Usoskin (2010) production rate calculations

throughout in our long-term model runs:
„Since geomagnetic modulation of polar ^{10}Be is not very sensitive to the choice of different production rate calculations (see Sect. 2.1.4) we use the production rate calculations of Kovaltsov and Usoskin (2010) throughout.“

Regarding figure 1 it would be interesting to see if Masarik and Beer's results only underestimate the mean ^{7}Be concentrations or if also the latitudinal gradient is disagreeing with the measured/modeled data.
We decided not to show another line in this plot which is already very complex. However we inserted the following sentences into the text:

„Different model results based on either Masarik and Beer (2009) or Usoskin and Kovaltsov (2008) production rate calculations differ by a factor of 2.40 on average (global average weighted with box sizes: 2.16). The differences show a latitudinal trend ranging from a factor 1.7 (tropics) to 3.2 (polar latitudes) higher ^{7}Be model results in case of Usoskin and Kovaltsov (2008) production rate calculations.“

The model tuning (page 11): Is the model tuned to reproduce the data using the Usoskin/Kovaltsov curve? When the authors refer to model-data differences does this also refer to the Usoskin/Kovaltsov production dependency only? On which production rate calculations do the GRACE model results depend (right part of figure 1).
We inserted the following sentences:

“In doing so, we use the Usoskin and Kovaltsov (2008) ^{7}Be production rate calculations for model simulations.” (in 2.1.2 Modifications of the GRACE model)

“(based on Kovaltsov and Usoskin (2010) production rates)” (in 2.1.3 Model validation)

“...based on Usoskin and Kovaltsov (2008) production rates.” (Fig. 1 caption)

The seasonal and longer-term variations in ^{7}Be. Are the model results based on the Usoskin/Kovaltsov results? It would be good to have a quantitative measure of data model agreement/disagreement. I guess changes in snowfall are not considered. It would be interesting to investigate if model/data disagreements can be explained by weather patterns (e.g. NAO during a certain year that might have influenced the measured data but that is not included in the model).
The model results are based on the Usoskin and Kovaltsov (2008) production rates which is now explicitly stated in the figure caption. Detailed investigations of the model-measurement residuals e.g. in terms of weather patterns are definitively interesting. However, given the added complexity to the paper we refrain from including this analysis into the present paper and refer to future publications.

Page 4 lines 8-10: "Even in case of minor climate changes of air mass transport, the degree of atmospheric mixing of ^{10}Be has major influence on the production signal recorded in ice core ^{10}Be." This is a very vague sentence in my opinion. It needs to be substantiated or rewritten. E.g. what is the definition of a "minor climate changes of air mass transport"? What degree of mixing has which influence?
We changed this section into:

„Measurements of ^{10}Be (and short-lived ^{7}Be) in polar air show that its boundary layer concentration is very sensitive to seasonal changes in atmospheric circulation processes like the stratosphere to troposphere exchange or vertical tropospheric mixing (Elsässer et al., 2011). So far it is up to debate how these processes are subject to longer-term climate changes and modulate the ^{10}Be ice concentration. In addition to direct effects of atmospheric transport on ^{10}Be, atmospheric mixing has major influence on the production signal recorded in ice core ^{10}Be: While geomagnetic changes...“

Section 2.1.3 concludes with "The model is thus also capable of simulating atmospheric ^{10}Be, since the atmospheric concentration of both (cosmogenic) radionuclides are governed by similar atmospheric production and sinks." This statement might be too optimistic. Due to its short half-life ^{7}Be is more sensitive to shorter-term processes compared to ^{10}Be. Therefore, a model that works well for ^{7}Be might not work so well for ^{10}Be. In addition, the long-term climate effects are not investigated by the model validation (as the

authors hint with the PTB data). However, these are important for the following discussion. Therefore I recommend to tone down the optimistic conclusions in this section.
We agree that the different half-life of ^{7}Be and ^{10}Be results in different atmospheric behavior. However, our model is calibrated using measurements of ^{137}Cs and ^{90}Sr (both half-lifes are reasonably long in terms of atmospheric residence time) and ^{7}Be is used only for model validation. Moreover, in comparison to longer-lived nuclides, the fact that ^{7}Be is indeed more sensitive to shorter-term atmospheric processes complicates its climatological modeling (and model-measurement comparison). It is thus likely that a model which is capable to reproduce ^{7}Be is promising tool for simulations of ^{10}Be. Indeed we agree that the reverse is not true.

We redrafted this section:

„In summary, the model validation shows that the model reproduces the climatology of ^{7}Be in the global atmosphere reasonably well. So far it is not possible to validate the model with ^{10}Be air concentration measurements since there are hardly any measurements available. However, given the model performance in terms of ^{7}Be, the model is likely also capable of simulating atmospheric ^{10}Be, since the atmospheric concentration of both (cosmogenic) radionuclides are governed by similar atmospheric production and sinks.“

The errors involved in this whole discussion are hardly considered. There are quantifiable errors (e.g. differences in the production rate estimates, errors of the geomagnetic field reconstructions). It would be good if the quantifiable errors would be included in the calculations.
Indeed we aimed at including a major section on model errors. However, it turned out that the most important sources of uncertainty are still not quantifiable (e.g. the reconstructed accumulation rate in case of our long-term model results). We thus refrain from giving the quantifiable errors (e.g. arising from the reconstructed geomagnetic changes) since these are mostly second order and would mislead the unknown reader.

Page 23 line 24: It is not clear to me what "In both cases, this divergence is dominated by the air–firn transfer model (4% and 5 %)" means. I guess the authors mean that one of the model parts is causing the divergence. Why do the authors come to the conclusion that this part of the model lies behind the difference?
Our air-firn transfer model uses the observed relation between ^{10}Be ice concentration and inverse snow accumulation rates (together with measured/derived ^{10}Be air concentration). This relation is derived from a linear fit to different measurements within several traverses in Greenland and Antarctica (see Fig. 5). At every single measurement site (i.e. in case of every snow accumulation rate) the measured ^{10}Be ice concentration differs from the overall linear fit due to statistical spread and second-order effects of air-firn transfer – which is the error of the air-firn-transfer model. In case of the Greenland Summit firn cores, the respective ^{10}Be ice concentrations differ from the linear fit to the traverse measurements by 4% and 5%. We have only very few measurements of the ^{10}Be air concentration at Greenland Summit (see p. 778, l. 1-4). It is thus not possible to give the detailed error of the atmospheric model. However, using both the overall model-measurement error and the air-firn-transfer error we assess „a good overall performance of the atmospheric model in reproducing the ^{10}Be air concentration in Greenland.“

We redraft this section:

„Due to lack of measurements, it is not possible to directly validate the simulated ^{10}Be air concentration at Greenland Summit (see Sect. 2.1.3). However, we may specify the model-measurement deviation of the air-firn transfer model (which is the difference of observed ^{10}Be ice concentration at a single site to the overall linear fit in Fig. 5). In case of both Greenland Summit firn cores, the overall model-measure ment divergence is dominated by this air-firn transfer model divergence (4% and 5%) which points to a good overall performance of the atmospheric model in reproducing the ^{10}Be air concentration in Greenland.“

Page 24: Holocene offset. This difference is surprising considering the good agreement for the snow pit data. The authors speculate about the reason (solar activity). I cannot see that this can realistically explain

this large divergence. Did the authors consider that the GRIP pit data was normalized with a different standard than the long GRIP record? In this case the Pit record and long records would show a similar agreement/disagreement.
We agree that the offset between the long-term GRIP Holocene ^{10}Be mean and the GRIP snow pits is indeed surprising. With respect to major differences in the AMS-calibration practice, we are well aware that comparison of different AMS-laboratory measurements is not easy. However, since recent studies focused on ^{10}Be-inter-laboratory comparison (e.g. Nishiizumi et al., 2007; Kubik and Christl, 2010) we corrected for this AMS-calibration-standard-effect to the best of our knowledge. In case of Greenland Summit, both the GRIP Holocene record (NIST SRM 4325 calibration standard (Yiou et al., 1997; Muscheler et al., 2004)) and the GRIP snow pit measurements (both based on BEST433/S555 (Stanzick, 2001; Heikkilä, 2007; Heikkilä et al. 2008a) have been adjusted differently to the Nishiizumi et al. (2007) calibration. This re-calibration could not account for the difference between the observational period ^{10}Be ice concentration (i.e. the firn cores) and the overall mean Holocene ^{10}Be ice concentration. We follow the reviewer that the solar activity is just a speculative idea. Indeed it may account for some of the difference and we are far from blaming solar activity to account for the whole difference. This is why we state: „the oversimplification of constant solar activity in the model simulations may significantly contribute to the model measurements difference.“

Details:
Abstract: It is hard to understand what this means: "However, model-measurements differences exhibit multi-millennial oscillations with amplitudes up to 87% of the mean observed Holocene ^{10}Be concentration". One can follow the exact meaning after reading the paper but rewriting would be useful to make it clearer in the abstract.
Reworded:

“However, model-measurement differences exhibit multi-millennial trends (differences up to 87% in case of normalized to the Holocene records) which call for closer investigation.”

Page 4 lines 16-18: "^{10}Be ice core records are definitely subject to climate modulation on longer timescales (e.g. Finkel and Nishiizumi, 1997)" The authors need to be more concise here. Is ^{10}Be the ^{10}Be concentration or the ^{10}Be flux? In addition, the reference is outdated since Finkel and Nishiizumi used a (now) outdated accumulation rate record.
We changed the sentence to "^{10}Be ice concentration is definitely subject to climate modulation on longer timescales (e.g. Finkel and Nishiizumi, 1997)". Referring to ice concentration without ambiguity, we keep referring to Finkel and Nishiizumi (1997) (who show the glacial-Holocene GISP2 ^{10}Be ice concentration record in detail).

Page 5 lines 12-14: "Indeed, monitoring of radionuclide air concentration in polar areas reveals that climatological features of atmospheric ^{10}Be have large spatial validity "What is a "climatological feature of atmospheric ^{10}Be"?
The sentence is changed into "Indeed, monitoring of radionuclide air concentration in polar areas reveals that climatological features (e.g. the decadal scale, mean air concentration or the average seasonal cycle) of atmospheric ^{10}Be have large spatial validity".

Figure 4: The Bard et al. reference refers to the Antarctic data. However, there is an earlier publication showing these data: Raisbeck, G.M., Yiou, F., Jouzel, J., Petit, J.R., 1990. ^{10}Be and d^{2}H in polar ice cores as a probe of the solar variability’s influence on climate. Philosophical Transactions of the Royal Society of London Series A330, 65–72
Reference changed

I was surprised not to see the reference: Variability of ^{10}Be and d^{18}O in snow pits from Greenland and a surface traverse from Antarctica by Berrgren et al. (NIMB294,568-572), 2013 in the context for the JASE traverse. I think these data should be included in the analysis
We did not include this data set since the sampling strategy of Berggren et al. (2013) is very different from our approach: While our samples comprise snow pits up to 2m which integrate more than ten years,

Berggren et al. (2013) sample only the upper 5cm. Their „samples contain less than one year's accumulation" (Berggren et al., 2013) and comparison to our data would be very misleading. We now give some more details on our samples and include a reference to their study into the supplementary information:
„15 snow pits of approx. 2m depth were measured for mean ^{10}Be concentration covering at least 10 years of snow accumulation....As in case of the Kohnen upstream traverse, ^{10}Be measurements were performed at integrated samples from approx. 2m snow pits. Time coverage of the samples is thus comparable to the Kohnen upstream samples. In the context of the JASE traverse, ^{10}Be measurements have also been performed by Berggren et al. (2013). However, these measurements cover the upper 5cm of snow, only, and contain less than one year's snow accumulation. A direct comparison to our measurements is therefore not reasonable."

Page 24 line 17: "overestimates the GRIP and GISP2" => "overestimates the GRIP and GISP2 data"
Included

Figure S.5 & S.7: labels seem to be missing
Done in case of S.5. Please give more details for S.7!

The authors should check for typos in the supplementary information
Done

Filling words such as "basically", "definitely",... should be removed from the manuscript
Done as far as possible

Review 2.

General reply:
We are particularly grateful to this reviewer for the extensive and very detailed comments on our work. Below, we edit all 92 comment in detail; however, it seems that a lot of the particular comments arose from few, basic misunderstandings. We here emphasize the four most essential sources of misunderstanding and to try to give clarification within the revised version of the manuscript (see replies to the detailed comments below).

Point I) Basic idea of the study
Our work is neither a GCM study, nor does it try to compete with the performance of GCMs. Our basic idea is to present a custom-made, measurements-based tool for the interpretation of ^{10}Be ice core records by combining a model for polar BL ^{10}Be air concentration and a model for the local transfer of ^{10}Be from polar air to ice (see also point III for terminology). In doing so, the model involves a lot of parameterizations and simplifications (in fact as every model) and we emphasize that we do not aim for simulating all physical processes involved. Nevertheless, the model is specifically tailored to the particular questions which we are addressing within the study (e.g. the atmospheric footprint of polar ^{10}Be in a climatological sense, see detailed reply below).

Point II) Long-term model simulations
State-of-the-art in ^{10}Be ice core research is to use relative variations of the ^{10}Be ice concentration or the derived ^{10}Be flux to reconstruct variations of e.g. the geomagnetic field (Muscheler et al., 2005) or polar snow accumulation (Mazaud et al., 1995: Steig 1996; Wagner et al., 2001b). These studies do neither account for any climate-driven changes in atmospheric circulation nor for climate-related processes of ^{10}Be deposition (e.g. changes in the ratio of dry versus wet deposition fraction). We aimed at showing that this (standard) practice needs review and ^{10}Be-based reconstructions have to account for climate changes in detail. In doing so we use a measurement-calibrated model (see point I) which is able to reproduce the observational period ^{10}Be cycle and show that net precipitation/snow accumulation and geomagnetic changes (i.e. the quantities which are reconstructed from ^{10}Be records) are not sufficient to reproduce

observed ^{10}Be changes on the glacial-interglacial time-scale in detail. Given the standard practice in ^{10}Be ice core research, this finding is new. We emphasize that it is not the aim of our model approach to simulate the glacial conditions of atmospheric transport and deposition in detail. This would indeed be hopeless with our basic model.

Point III) Terminology

Again, please note that our study is rather different from GCM (or Lagrangian atmospheric transport model) studies – it is therefore obvious that the terminology used for different model parts differs as well. Within our paper, we try to fill the gap between atmospheric transport (modeling), ice core research and glaciology. So, it is inevitable, that some of the terms used in the paper are uncommon for one or the other community. We apologize for this but it is not possible to adapt our terminology to all these communities at once.

Point IV) The GRACE model of atmospheric transport

We use the GRACE model for simulating ^{10}Be transport in the atmosphere. This model is not developed for the present study but is designed and continuously improved at IUP Heidelberg for nearly 20 years. It is based on the work of Hesshaimer (1997) (see also Hesshaimer et al. (1994)), substantially revised by Naegler (2005) (see also Naegler and Levin (2006)) and Levin et al. (2010b) (see also Levin et al. (2010a)) and emerged as a valuable tool for investigating the atmospheric circulation of greenhouse gases. The model is calibrated with an extensive calibration strategy using a great deal of atmospheric measurement data (^{14}C, SF_6, ^{10}Be/^{7}Be). We apologize that it is not possible to recapitulate all details of the model calibration which are published in earlier studies. Given the complexity of today's model setups, we think that it is standard practice to refer to earlier publications. The most recent version of our model is extensively described in the supplementary information of Levin et al., (2010b): http://onlinelibrary.wiley.com/doi/10.1111/j.1600-0889.2010.00456.x/full).

This study presents a modeling approach of climatic influences on ^{10}Be ice core concentrations between the Holocene and glacials. The authors employ a two-dimensional box-diffusion model of atmospheric (stratospheric and tropospheric) circulation which incorporates a parametrized particle removal scheme. Different modeling approaches to interpret ^{10}Be records are definitely welcome and can be used for different purposes and time scales, however the manuscript should be explicit about what are the shortcomings of each approach and to what extent the results can be interpreted. I'm missing plenty of details and a thorough discussion of the most important parameterisations and simplifications and how they will affect the results of this study right in the beginning of the manuscript. Now the authors merely mention that this model is coarsely simplified but do draw strong conclusions although these are likely to be dependent on the simplifications.

See general reply points I, II and IV !

The way the model is set up now for the glacial simulation suggests that atmospheric circulation is kept "constant", precipitation rate is prescribed, ^{10}Be production follows the geomagnetic field reconstruction and the model deposition responds linearly to precipitation changes. The outcome of the simulation therefore shows that ^{10}Be snow concentration is modulated by precipitation/snow accumulation changes as well as geomagnetic and solar modulation on the ^{10}Be production. This is already clear at the first glance on the data and one might ask what added value does the model bring. In reality, there will have been large changes in atmospheric circulation during glacials due to changes in sea ice cover, topography, greenhouse gas concentrations etc. which will have caused large changes in precipitation rate as well. If these are not taken into account, not much can be said about climatic forcing on ^{10}Be during glacials.

See general reply point II !

I am well aware that the atmospheric circulation of the model, controlled by exchange times which are not even well-known for the present, cannot be expected to be realistically estimated for the glacials. What I do miss is a critical discussion of these shortcomings, especially how they influence the conclusions. Too

quantitative conclusions, such as the numbers in Table 1, would be better omitted.

I would recommend a publication of the manuscript after some major corrections have been made.

1) Atmospheric circulation, both stratospheric and tropospheric, in the model. How are they parameterised? I presume that the model simulates diffusive transfer between boxes, which is controlled by prescribed residence times. It is essentially important to know what are the residence times used in the model. The actual exchange times between atmospheric 'boxes' are by far uncertain due to limited observations, especially in the stratosphere. Estimations can be made based on observations of the bomb peaks, as made for this model, but the number of observations is fairly limited, and doesn't tell us anything about the transport within the stratosphere. Therefore this is a large uncertain in the model and should be mentioned clearly in the beginning. Furthermore, interpretation of the results which are based on atmospheric circulation should be made with extreme care.
See general reply point IV !
It is not feasible to give atmospheric residence times for all 41 boxes, since our model considers not only diffusion between adjacent boxes but also Brewer-Dobson circulation as well as variations in the tropopause-height.
We do not agree that our atmospheric model approach involves large uncertainty regarding atmospheric transport of ^{10}Be. Still, atmospheric processes like the Stratosphere-Troposphere-Exchange are not understood completely. It is thus a meaningful approach to calibrate airmass transport by using atmospheric tracer measurements. Certainly the calibrated transport is valid only for the features of the calibration-tracer applied. Here, bomb radiocarbon is an excellent tool for simulating (the airmass transport fraction of) ^{10}Be transport in a climatological sense: (1) The main source of both nuclides is the (high latitudes) stratosphere. Both boundary-layer ^{14}C and ^{10}Be are thus strongly affected by Stratosphere-Troposphere-Exchange. (2) The temporal scale of the decline in the ^{14}C bomb peak resembles the 11-year solar signal of ^{10}Be.

2) The terminology used in the paper is rather uncommon in places. What exactly is meant by air-firn transfer? Is this referring to the removal of ^{10}Be from the atmosphere into the ice (i.e. "sink", which is also used in the manuscript), including wet and dry deposition and sedimentation? Or is it merely local transfer from air to firn at surface (which is most commonly referred to as dry deposition)? Following the discussion can be confusing because the reader is unsure of what is meant. Typically there are three removal processes considered for ^{10}Be: a) wet deposition, which means the removal by rain. It takes place at the level of clouds (rainout) or below clouds (washout). Washout is typically estimated to be of minor importance. b) dry deposition, which means a local friction based capture of particles at surface, and c) sedimentation or gravitational settling, which takes place in the entire atmospheric column, including the stratosphere. My impression was that in the model, all these processes are considered to take place at the same altitude (PBL). In reality, wet deposition strongly influences the vertical mixing by removing ^{10}Be from the level of clouds and by vertical transport of ^{10}Be within the clouds. In contrast, dry deposition and sedimentation have a minor effect on tropospheric circulation of ^{10}Be. I would recommend a clear statement of what processes are considered and where do they take place. I would also suggest defining a terminology for this manuscript and sticking to it (sink/air-firn transfer/deposition are all being used).
See general reply point III !
As evidently confined to (permanently) snow covered areas, the expression „air-firn-transfer of aerosol particles“ comprises a lot more processes than addressed here by the reviewer (e.g. including for the here relevant dry sow zone, air pumping, and snow drift scavenging). It summarizes all processes eventually incorporating particles into the near surface firn layer and may be collapsed into a purely observational number, relating (on the climatological time scale) surface air to firn concentrations of the aerosol species in question. Upon first use we added a respective, brief explanation: „Here, air-firn transfer comprises all processes incorporating particles into the near surface firn layer (e.g. wet and dry deposition, air pumping, snow drift scavenging and sedimentation)“ (see Sect. 1).
On the other hand, we use also the concept of wet and dry deposition (including the processes correctly mentioned by the reviewer) to simulate the large-scale atmospheric ^{10}Be sinks in case of our global

atmospheric model.
The use of both “terminological concepts” reflects the split-up of our model approach into a global atmospheric and a local polar air-firn-transfer part.

More detailed comments:

Abstract

l. 2-3: "ice core measurements" .. "a tool to study" "reconstruction of past solar activity or variation in the natural ^{14}C production rate" - I think the main purpose to study the natural ^{14}C production rate is to reconstruct the solar activity, too, and to compare it with ^{10}Be, but can ^{10}Be be called a "tool" for it?
We changed the sentence into: „^{10}Be ice core measurements are an important tool for paleoclimate research, e.g. allowing for the reconstruction of past solar activity or changes in the geomagnetic dipole field“.

l. 11: "Being specifically configured for polar ^{10}Be" - This sounds rather limiting. The authors seem to use the model for all latitudes, especially when validating it.
This sentence refers to the combination of the global atmospheric model and the local (polar) air-firn-transfer model (see also general comment I). We changed the sentence to „Being specifically configured for ^{10}Be in polar ice, this tool thus allows...“

l. 13-14: "We find that the polar ^{10}Be concentration does not record a globally mixed cosmogenic production signal" - this result strongly depends on the parameterisation of the circulation and the exchange/residence times used, as well as the distribution of ^{10}Be production in the atmosphere. I'm not convinced that such statements are possible with a coarse model like this. See also more detailed comments in the section where this result is discussed. The entire manuscript: correct all "model-measurements differences", "measurements-model-differences" etc. to "model-measurement differences"
See our comments to the reviewer’s point 1) above and the replies to the more detailed comments below!
We now use the term „model-measurement differences“ throughout.

l. 23-24: "unconsidered climate-induced changes could likely explain the model shortcomings" - This sentence is hard to follow. The shortcoming of the model is the very fact that it doesn't consider climate-induced changes?
Changed to „...could likely explain the model-measurement mismatch.“

l. 24-25: "In fact, the ^{10}Be ice concentration is very sensitive to snow accumulation changes" - The flux of ^{10}Be from the atmosphere into the ice/firn can be converted into snow concentration by a division by snow accumulation and vice versa. Therefore it is trivial that the snow concentration is sensitive to it. Or is the result here that the snow accumulation varies on a larger amplitude than ^{10}Be flux? This is of course reasonable because snow accumulation reconstructions based on $d^{18}O$ have been shown to vary more strongly than the ^{10}Be production rate.
We strictly deny this view: Mean ^{10}Be concentration in firn (ice) may be formally transformed into a deposition flux (density) by a simple multiplication with the relevant snow accumulation rate. This makes the particle flux immediately depending on snow accumulation rate. The dependence of the ^{10}Be concentration in firn (ice) on snow accumulation rate is, however, a matter of the typical ratio of the precipitation and non-precipitation related particle fluxes. Imaging the notional boundary case of no dry deposition – changes in the snow accumulation rate would have hardly any effect on the ^{10}Be ice concentration. There is still no agreement on the ratio of wet and dry deposition for single ice coring sites. Note that GCM studies predict wet deposition to be the dominant process in case of Greenland. Following this view the Greenland ^{10}Be ice concentration should not be sensitive to snow accumulation changes.

Introduction

l. 7-8: "and is thus deposited to polar glaciers" - ^{10}Be is deposited everywhere, not only to polar glaciers.

Also remove the word "thus".
Changed to „^{10}Be quickly gets attached to sub-micron aerosol particles and is removed from the atmosphere by wet and dry deposition processes, including deposition onto polar glaciers."

l. 15-18: "The question, whether to use...." - depends on the time scale, but also one or the other influences has to be filtered out.
This is true. However we don't want to complicate things here. The fact that one process has to be considered when reconstructing the other is implicated in lines 15-20.

p. 764, l. 5-> "Measurements of ^{10}Be (and short-lived.... ...sensitive to atmospheric circulation..." - here it's probably worth mentioning on which time scales these processes act, and that they should average out on longer time scales. If not, the modeling approach of this study would not be suitable.
We included: „seasonal changes in".

l. 8: Next sentence "Even in case of minor climate...." this is a strong statement. Do you have evidence? The fact that for example the 11-year cycle is found in virtually all data rather suggests the opposite.
We apologize for poor phrasing. Reworded as „So far it is up to debate how these processes are subject to longer-term climate changes and modulate the ^{10}Be ice concentration. In addition to direct effects of atmospheric transport on ^{10}Be, atmospheric mixing has major influence on the production signal recorded in ice core ^{10}Be: While geomagnetic changes..."

l. 10: "While geomagnetic changes primarily affect atmospheric" ---> remove the word primarily, they only affect ^{10}Be production at low latitudes
According to major production rate calculations, geomagnetic changes affect ^{10}Be production at latitudes lower than 60°. Indeed there is no exact definition of „low latitudes". However we feel that 0° - 60° is not „only low latitudes".

l. 15: "While during the Holocene.... production changes related to solar activity" - solar _and_ geomagnetic activity
Changed to „solar and geomagnetic activity"

l. 18: "Here, strong changes in snow accumulation alter aerosol deposition" - Please use more exact language as there is room for misunderstandings here. I would rather use precipitation rate than snow accumulation when talking about deposition. Aerosol is deposited from the atmosphere, similarly to precipitation (direction is towards the surface). The precipitated snow then accumulates at the surface (direction is upwards from the surface). Changes in precipitation can affect aerosol deposition (both taking place in the atmosphere), but not the snow accumulation at the surface which happens after deposition at the surface. Also the fact that aerosol deposition is altered does not per se implicate that ^{10}Be deposition is altered.
We deleted: „the aerosol deposition and thus" and stick to (net) „snow accumulation" since this is the relevant term here and the basic information obtained at the ice core drilling site.

l. 18 same sentence: "alter aerosol deposition" - more accuracy is necessary: it might alter the spatial deposition pattern, but if there is no change in the ^{10}Be production there can't be a change in the ^{10}Be deposition in a global sense. The ^{10}Be ice concentration will be altered if snow accumulation/precipitation changes even if ^{10}Be deposition flux is constant, but this would be trivial.
Deleted: „the aerosol deposition and thus"

l. 25: "adequate models do not exist yet" - GCMs with incorporated aerosol modules are fairly well suited for studying climatic impacts on particle deposition. Time slice simulations are possible. "Conceptual models which are _generally_ used" - to my knowledge conceptual models have actually been used less often than physics-based GCMs (in fact this is the first time I'm seeing one).
We apologize for poor phrasing. GCMs are indeed helpful tools to support analysis of ^{10}Be ice core records within time slice simulations. However – as stated in this section – they are still unfeasible for full

simulations of ^{10}Be ice core records. To avoid misunderstandings we changed this section to:
„Regarding these uncertainties in the interpretation of ^{10}Be ice concentration, there is a strong need for modeling attempts to simulate ^{10}Be ice core records. However, adequate models do not exist, yet.“

To our knowledge, the phrase „conceptual model“ is used for a combination and application of concepts and ideas but is not a model in a physical sense. In this definition, usage of a (estimated) ^{10}Be flux to reconstruct e.g. solar activity is such a conceptual model (the basic ideas or concepts are: 1) ^{10}Be wet deposition is negligible and 2) this holds for the entire period under investigation). In fact our model is an existing computer model and is not conceptual.

l. 3: Climate modulation occurs on long and short time scales
Changed to: „...since both production and climate modulation of ^{10}Be also occur on long timescales.“

l. 4-5: Using dynamical models for studying atmospheric particle transport is still a fair bit away. Currently Earth system models are used for studying climate but they are very restricted in resolution. To extend them to include aerosol physics will require a large increase in computational power and still they would not be resolving the atmospheric transport well.
We replaced „certainly“ with „likely“

l. 5-6: "custom-made models of lower complexity" to improve our "fundamental understanding" - not sure if our fundamental understanding can be improved with basic models which depend on user-set parameters?
„fundamental understanding“ replaced by „interpretation“

l. 17: "hand-over from ^{10}Be air concentration to ice concentration" - here a more physical explanation is needed, see comment 2) in the beginning of the review.
See general reply point I and III !

2. Model setup

l. 6: "simulations under different scenarios on atmospheric transport" - if I understand the model correctly the atmospheric circulation and thus transport is constant in the model.
The atmospheric circulation is constant in every single model run. However we performed several model runs under different scenarios (e.g. to investigate sensitivities, see e.g. p.782, l.20-29).

l. 25: *) seasonal variability of STE - this is not very well understood. STE processes have been studied and found to have a seasonal cycle with a winter maximum. Stratospheric radionuclides measured closer to the surface exhibit a spring maximum. How did you parameterize the seasonal STE in the model?
Based on the non-adequate understanding of the STE it is a good approach to use a parameterized model which is calibrated with atmospheric measurements.
In our model the seasonality of the cross-tropopause transport comprises three different processes: 1) & 2) Brewer-Dobson circulation and diffusive transport are parameterized by a function of type: $f = A \cdot tanh(15 \cdot (sin(2\pi \cdot t + \varphi)))$. This allows a sharper transition between weak and vigorous exchange than the sine function. 3) The tropopause height varies sinusoidal. Calibration is based on measurements of tropospheric Δ^{14}C in the 1960s (northern hemisphere) and measurements of the ^{10}Be/^{7}Be ratio (southern hemisphere) (see general reply point IV and details in the supplement of Levin et al. (2010b))!

p. 767, l. 6: STE and especially precipitation exhibit enormous variability on zonal scale, too.
We do not deny that there is variability on zonal scale. However, the major variations (in a climatological sense) are on latitudinal scale. For clearness we add the term „climatological“.

l. 12: not atmospheric but tropospheric circulation cells. Here also more information is needed of how the exchange between FT and PBL is parameterised (and also how the exchange times between the circulation cells are defined). Is there vertical transport within the troposphere? Also, the height of the tropopause is

varied. Does this influence the distribution of ^{10}Be produced within the stratosphere and the troposphere? What is the fraction produced in each of the spheres and how does it compare with previous studies?
A detailed explanation of the FT-PBL parameterization can be found in the supplementary information of our paper (p. 6-7). The respective turnover times are shown in Fig. S.4.
See general reply point IV !
The troposphere is divided into boundary-layer and free troposphere. These boxes are assumed to be well mixed. Based on this model resolution, vertical transport is considered by model calibration.

l. 14: "calibrated" - by adjusting the residence times in the boxes? I wasn't able to find the supplement of Levin et al., 2010b. This information is so essential for this study that is should be given.
See general reply point IV!

l. 16: observed ^{10}Be/^{7}Be at the surface? See comment *)
„boundary layer“ included

l. 19: Same question regarding the Brewer Dobson circulation - is it controlled over the residence times? This information is essential for the results of this study.
In our model approach the Brewer-Dobson circulation is implemented as a uni-directional gross flux of airmass. Brewer-Dobson circulation would thus modify the single boxes airmass if it would not be balanced globally. We included a short, respective section on details of the airmass transport in the supplementary information:
„While (turbulent) diffusive transport between neighboring boxes is controlled by turn-over times, Brewer-Dobson circulation is implemented as a gross flux of airmass between the boxes. However, Brewer-Dobson circulation is balanced globally and does not change total box airmass. Both the diffusive transport as well as the Brewer-Dobson circulation are modulated seasonally. In case of the seasonal varying tropopause height, there is a net flux of airmass between the lower stratosphere and the free troposphere. Further details on the implementation of airmass transport is given in the supplementary information of Levin et al. (2010).“

l. 19-20: "given the climatological approach" - climatological usually refers to long-term and inter-annual variability, not seasonal. Figure S1 gives the distribution of air mass between the boxes, but the distribution of ^{10}Be would be helpful, too.
Climatological denotes the mean state of climate events (in meteorology at least covering 15 years of observations). This term covers also the mean seasonal variability. We added „(i.e. mean state of climate)“ (p. 766, l. 3)

l. 28: "significant differences": what are they?
We give references to the studies. A review of their results is beyond the scope of this paper.

l. 5: ^{222}Rn: there are very large differences in ^{222}Rn release in the zonal direction (land/sea). How are these taken into account? This might not be relevant for this study but could then be mentioned.
We now give further details in supplementary information (Sect. S.1.3):
„We use a temporally constant ^{222}Rn flux of 1 atom cm^{-2} s^{-1} for tropical latitudes which is linearly reduced from 30°N to 70°N according to Conen & Robertson [2002]. Implementing this ^{222}Rn source into the GRACE boundary layer boxes, we apply the ^{222}Rn flux map from Schery & Wasiolek [1998] to estimate the fraction of land (uncovered by ice) in every model box.“

l. 7-9: atmospheric boundary layer is an atmospheric sink for radionuclides?
Figure S4 d) shows the parameterisation of the PBL-FT coupling. Are there measurements from both layers or how can this exchange be distinguished from surface measurements? Also the exchange between global polar box and the ice sheet box is probably fairly difficult to estimate?
We changes the sentence to „Being the only atmospheric sink for aerosol-borne radionuclides in addition to radioactive decay, the atmospheric boundary layer boxes required major modifications.“
See S.1.5 for a detailed description of the calibration of PBL-FT coupling. The (turbulent) diffusive exchange

between the global polar boxes and the ice sheet boxes is inhibited. This assumption is based on radionuclide measurement studies (e.g. Dibb and Jaffrezo (1993); Elsässer et al. (2011)) which indicate that these air masses substantially differ. However we clearly state that this is a simplification (see S.1.5 for details).

2.1.3 Model validation

Are the observations averaged over longitude when compared with the model? Given the large zonal differences in precipitation and also ^{7}Be observations this could make a difference. One should be careful, the global coverage of the observations is still very limited.
We decided to use single measurement sites for model-measurement comparison (see Fig. 1) and not to use measurements averaged over longitude. This gives the reader the most direct evaluation of model performance and measurements data basis. For clearness we included „To do so we compare measurements at single sites with the model results for respective latitudes (see Fig. 1).“

l. 15-> the difference between U&K and M&B production rate based comparison of 2.4 is surprisingly large. How much do the actual production rates differ?
The number of 2.40 is the mean of the differences in all boxes. Since both production rate calculations imply a different latitudinal gradient, the global average (weighted with box sizes) is 2.16, only. The ratio of both production rate calculations in global mean ^{7}Be production (differences in solar modulation accounted for using Herbst et al. (2010)) is 1.7. However, in case of short-lived ^{7}Be, it is not surprising that production rates and air concentrations show different ratios: Small differences in the altitudinal gradient of ^{7}Be production rates have major impact on the ^{7}Be concentration since its radioactive half-life is smaller than stratospheric residence times.

We included:
„Different model results based on either Masarik and Beer (2009) or Usoskin and Kovaltsov (2008) production rate calculations differ by a factor of 2.40 on average (global average weighted with box sizes: 2.16). The differences show a latitudinal trend ranging from a factor 1.7 (tropics) to 3.2 (polar latitudes) higher ^{7}Be model results in case of Usoskin and Kovaltsov (2008) production rate calculations.“

l. 19: Aren't the residence times in the model tuned to reproduce the observations? Therefore the good match would not be surprising.
The model tuning is based on ^{90}Sr and ^{137}Cs observational data (except at polar sites: Here we use ^{7}Be measurements but do use ^{10}Be instead of ^{7}Be for model validation). Indeed there is a source of error since e.g. both the ^{137}Cs/^{90}Sr (model calibration) and ^{7}Be (model validation) partly have origin at the same sampling sites (same meteorological conditions). That is why we additionally use CTBT ^{7}Be measurements for model validation (see Fig. 1).

p. 771, l.1: ...or the deposition strength or atmospheric circulation
We agree and suggest a more general explanation:

„This could be related to a deficient boundary-layer (BL) – free troposphere (FT) coupling or overestimated ^{7}Be deposition. So far, it is not possible to exactly validate the ^{7}Be air concentration in the free troposphere (see Fig. 1, right) and thus the BL – FT gradient. However, it is noticeable that the ^{7}Be model results for the southern free troposphere (not shown) are only 35% and 23% (in case of polar and mid latitudes) of the simulated concentrations in the respective northern free troposphere boxes. Different to the southern hemisphere, the model could thus have deficits in simulating the FT-BL vertical transport in the northern hemisphere.“

l. 11: "low data basis" - what do you mean?
Changed to „and the low data basis of 10 measurements“

l. 12: could only the months be compared when you have observations?
This is poor phrasing. We changed the sentence to: „Here, model results exceed single months measurements by 30% on average."

l. 28: STE seasonality in Greenland? STE mainly occurs at latitudes far from Greenland. There is also a lot of ocean at 30-50 deg with no ^{222}Rn emission. If your model is averaged over all longitudes, this could be important.
We redraft this section:

„Interpreting this result it is important to bear in mind that the model is forced to reproduce the seasonal cycle of Greenland boundary-layer ^{210}Pb (due to calibration of BL-FT diffusive vertical air mass exchange, see S.1.6). The models disability to reproduce the measured summer peak in Greenland boundary layer ^{7}Be may thus originate from deficient simulation of ^{210}Pb (e.g. boundary layer transport from the Arctic basin to the Greenland ice sheet). Indeed, further model measurements comparison of ^{7}Be seasonal cycles at globally distributed sites (see supplement S.1, Fig. S.5) reveals that the model performance is very good in mid- and polar latitudes (no ^{210}Pb involved in the model calibration). Major differences occur at low latitudes, only, where the model overestimates seasonal cycle amplitudes. Here, measurements in the tropics may be..."

p. 772, l. 3: do you mean Figure S.5?
Changed to S.5

l. 5-6: precipitation also varies very largely over the North Atlantic versus the continents at the same latitudes.
Again, we do not deny that there is major zonal difference in precipitation. However, our model results for mid and high latitudes (see Fig. S.5) show that - in a climatological sense - the 2D model approach works reasonably well for simulation of atmospheric ^{7}Be.

l. 19: the production should be well established by Usoskin et al., 2011.
We changed the sentence to „Excluding measurement artifacts, a possible explanation for this inconsistency are long-term climate effects (e.g. precipitation changes) which bias..."

l. 22: "well" -> "reasonably" or similar
Changed to „reasonably well"

l. 5-29: There are a number caveats in the analysis. Firstly, Field et al. and Heikkila et al. compared the deposition flux, not the entire atmospheric inventory. The deposition flux roughly corresponds to atmospheric concentrations at the level of the clouds. Surely the entire content of the atmospheric column would look different. Furthermore, the production difference, given here in %, is highly non-linear and not applicable as a percentage at any production change. And finally, the level of mixing is a result of the level of horizontal diffusion in the model, which again depends on the production distribution, scavenging strength plus the level of diffusion in the model. The level of diffusion again depends on the horizontal resolution, which in Field's case was coarser than in Heikkila's. The fact that the coarse 10-degree model produces even less mixing supports this finding. However, this all depends on your pre-defined exchange times between the boxes which is a step less realistic than a dynamical model solving the Navier-Stokes equations for circulation. In my opinion this model is not really suited for such estimations, especially with none of the above-mentioned factors influencing the result mentioned.
1. To account for the fact, that the GCM studies investigated the deposition flux, we changed the section to:

„In recent years, two GCM studies investigated the atmospheric footprint of the polar ^{10}Be deposition flux though achieved inconsistent results. Assuming that the polar ^{10}Be deposition is controlled by polar boundary layer ^{10}Be air concentration, our finding contradicts to GCM results from Heikkilä et al. (2009) (based on ECHAM5-HAM) but is in line with Field et al. (2006) (using the GISS ModelE)."

2. We find that the polar damping effect is not highly non-linear, but varies between 20% and 22% only (in

case of Greenland and Masarik and Beer (2009) production rates). To clarify this point we changed the last sentence to: „Using Masarik and Beer (2009) production rate calculations our Greenland model simulations quantitatively confirm this result: 20-22% lower geomagnetic modulation within the total range of analyzed global geomagnetic field changes."

3. Note that we find polar damping of 50% in case of Kovaltsov and Usoskin (2010) production rates only. Indeed, our model does not produce „even less mixing" but coincides with Field et al. (2006) – if we use Masarik and Beer production rates as done in the study of Field et al. (2006). Differences in the horizontal diffusion is just one of many possible explanations for the different model results regarding atmospheric mixing of ^{10}Be. Indeed, the fact that our really coarse-resolution model quantitatively confirms the finding of Field et al. (2006) (based on the comparatively high-resolution GISSModelE) contradicts to horizontal diffusion as being the main source of model difference.

l. 22: Usoksin -> Usoskin
Changed

2.2 Local air-firn transfer

See previous comments. Which type of deposition/removal are you discussing?
See respective reply!

l. 8: "Antarctica" ---> "Antarctic"
Corrected

l. 10: The Greenland ice sheet is also 3km thick
Changed to „up to 3 and 4 km (in case of Greenland and Antarctica)"

l. 15-18: air concentrations and thus flux into the ice are constant -> snow concentration depends on precipitation rate which varies spatially
Snow concentration depends on more than precipitation (e.g. dry deposition or aerosol scavenging - see respective replies above). So far there is no proof if the involved removal processes are constant in space and time. Indeed there are very different meteorological settings at coastal and interior Antarctica which could definitively influence e.g. aerosol scavenging. That is why we here use a generalized expression: „the processes delivering ^{10}Be from air to firn."

l. 23: "can be separated" - consists of
Changed

p. 775, l. 3: ^{10}Be concentration - where? Should be the concentration at the surface
Changed to „^{10}Be surface air concentration"

l. 5-7: wet and dry deposition fraction controlled by the same air mass - this is not correct, dry deposition acts at surface, wet deposition at the level of clouds. This model might not be able to distinguish between atmospheric layers but it should be made clear that it is a simplification. Where is sedimentation considered?
„Assuming that both, the dry and wet deposition fraction are controlled by the same air mass..." This is indeed a reasonable assumption, since precipitation over central ice sheets forms within low level clouds and even via diamond dust formation.
In our model approach, boundary-layer sedimentation is included in the dry deposition part since our concept of measurement-calibrated dry deposition velocity includes all processes that are not related to precipitation. We included „ It comprises all deposition processes which are not related to precipitation"

l. 14: wind drift and evapo-sublimation are probably averaged out in the 10-degree box. It might be of interest to see how the scavenging efficiency is parameterised in general circulation models and compare

your epsilon with it.
GCMs or atmospheric transport models use a very different approach for aerosol scavenging: The scavenging efficiency is accounted for with a kind of disintegration constant (unit: 1/time) describing the removal of aerosol particles from an airmass – usually distinguished in 'below-cloud' and 'in-cloud' scavenging (see e.g. Sportisse (2007) for a review). On the contrary, our scavenging ratio is a simple ratio between tracer concentration in air and tracer concentration in precipitation as e.g. extensively reported in aerosol studies from surface observations. A quantitative comparison between both concepts is not straightforward and would extend the scope of this work. However, we are interested in future model-inter comparison studies.

p. 776, l. 3: "three parameters governing the hand-over of ^{10}Be from air to firn" - surely this is a conceptual model but actually there are 3 completely individual processes (wet, dry and sedimentation) acting at different atmospheric levels and times. Calling it "hand-over" sounds rather coarse.
See above for our definition of „conceptual model".
The full sentence is „Following this basic air-firn transfer model, three main parameters govern the hand-over of ^{10}Be from air to firn." We agree (and state) that our model is rather basic (though it considers the individual deposition processes) and we appreciate discussion on its fundamental assets and drawbacks. However, the here-given finding is a direct model result. The question whether it is reasonable depends on the applicability of the model. Review has to address the model itself.

l. 16: Now you're also assuming a global mixing of ^{10}Be production signal?
We do not assume a global mixing of the ^{10}Be production signal but use our atmospheric model to account for this production effect. The procedure is described in detail in the supplementary information (see S.2.2): „two issues require recalibration (i)..... and (ii) observed ^{10}Be ice concentrations cover different periods of time and are thus modulated by different conditions of cosmogenic production variability.....Regarding (ii) we apply our atmospheric ^{10}Be model together with records of the cosmogenic production variability. To do so, ...The model results for the ^{10}Be air concentration in the boundary layer above the ice sheets are used to correct for different states of atmospheric production recorded in the different ^{10}Be ice core data."

Outsourcing a lot of detailed information to the supplementary part of the paper is the only way to achieve a maximum of readability together with a maximum of information. We are well aware that this procedure results in a huge supplementary part which may be uncommon for a single paper. However, this review endorses us to give as many details as possible.

l. 19: "Hence, we conclude... spatial differences driven by accumulation changes"- accumulation changes in time or space? The sentence continues: "while spatial (and thus climate) differences ..." do you mean temporal? Consider reformulating the sentence.
We changed this section to „Hence, we conclude that spatial variation in the mean ^{10}Be ice concentration is driven by spatial accumulation changes at first order. The major differences between meteorological conditions at coastal and interior Antarctica seem to have less influence on the dry deposition velocity or aerosol scavenging."

p. 777, l. 9: "The latter (spatial scale of the traverse?) reduces spatial trends of atmospheric transport conditions" - the sentence is hard to understand. Atmospheric transport is a combination of physical processes and does not really have trends.
We changed the sentence into: „It is thus very likely that all sampling sites are affected by comparable atmospheric transport conditions."

l. 18-23: This result basically shows that the deposition flux is constant, or, varies on a much lesser scale than the precipitation rate. This is a nice result indicating that precipitation changes are not driving the ^{10}Be flux variability. The factors of 1.9 or 2.3 simply indicate different slopes in precipitation variability, depending on the orography which obviously never is the same.
This is essentially a misinterpretation: The results basically shows that the deposition flux is NOT constant but the dry deposition flux is fairly constant and obviously not driven by precipitation changes

(implying a rather uniform ^{10}Be concentration in near surface air). The factors of 1.9 and 2.3 indicate a different dry deposition flux which can be partly explained by different ^{10}Be air concentrations.

p. 778, l., 2: "constant ^{10}Be/^{7}Be ratio" - this might be risky.
We agree. However, this uncertainty is well-considered in the estimated uncertainty of the mean Summit ^{10}Be air concentration: While the overall mean ^{7}Be air activity concentration at Summit station is reasonably well known (due to 16 years of high-resolution measurements), the derived mean ^{10}Be air concentration has an uncertainty of 17%.

l. 13: "stronger dry deposition velocity caused by dried conditions (less wet deposition)". Dry deposition velocity depends on the roughness of the surface, type of soil etc. Estimated values can be found in the literature (approx. an order or magnitude less over ice than over land). The dry deposition of aerosols depends, besides the velocity, on the aerosol radius by orders of magnitude. Dry deposition velocity knows nothing of whether wet deposition is taking place or not. The surface of ice can vary largely, too, depending on the conditions, so the dry deposition velocity does not necessarily have to be equal, or constant, in Greenland and in Antarctica.
There was indeed a flaw. We reworded the respective section as:
„Thus, at first glance, our ^{10}Be measurements point to a stronger dry deposition velocity in Antarctica which may be caused by different glacio-meteorological conditions."

p. 779, l. 1: again, the rainout processes are very complex, depending on the type of the cloud and atmospheric conditions. They take place at the level of the clouds but at the same time, vertical transport within the clouds is essential in redistributing aerosol within the atmospheric column. I understand that these complex processes cannot be described in the model, but it should be made clear that the fixed scavenging ratio epsilon roughly simplified, and works on long time scales and does not consider climatic changes.
See general reply point I above and note that we already explained the simplistic made up of our „air-firn-transfer model" in detail.

l. 9: "^{10}Be ice concentration is less influenced by accumulation rate" - your figure 4 seems to contradict this.
This is a misunderstanding. We recapitulate here different conditions of either dominating wet or dry deposition and their consequences in general. We do not refer to particular sites or areas as shown in Fig. 4. For clearness we insert the term: „Generally speaking:"

l. 10: "^{10}Be ice core concentration a primary reference for atmospheric ^{10}Be and related production changes" - This is not correct. ^{10}Be in rain (be it concentration or flux), in air filters and snow concentrations have all been found to show the 11-year cycle, for example.
We do not doubt that ^{10}Be ice concentration shows variations due to production changes. ^{10}Be is produced in the atmosphere and than transported and deposited. Insitu-production of ^{10}Be in ice is insignificant. The physical relation between the ice concentration and the ^{10}Be production is atmospheric ^{10}Be. To avoid any misunderstanding we reword the sentence: „...making the ^{10}Be ice concentration a primary reference for atmospheric ^{10}Be production changes."

l. 22-> perhaps your estimated dry deposition velocity could be compared with the GCM based one to understand the discrepancy. Field et al., found similar fractions of wet to total deposition (above 90%). Models suggest that for other species as well. How are estimations of ice core based sulphate, for example?
The sentence is changed:

„Global circulation model results (Field et al., 2006; Heikkilä et al., 2008a) report on a larger impact of wet deposition in case of Greenland (i.e. dry deposition less than 10%)."

GCMs use dry deposition models based on resistance-in-series schemes. A detailed comparison based on published values is thus quite difficult. However, we look forward to detailed model inter-comparison in future studies.

At least in Antarctica, the seasonal and spatial pattern of sulphate is largely different to those of ^{10}Be, making a comparison not straightforward.

p. 780, l. 1: One important point to note is that all aerosol is eventually deposited to the ground, be it wet or dry. It can't stay in the atmosphere forever. This might lead to spatial differences in deposition, but the deposition will be equal to production averaged over some time, independent of climate.
On the global-scale it is definitely true that deposition will be equal to production averaged over some time. However, ice cores are always local and thus effected by e.g. much-suggested spatial shifts in storm patterns. Such climate events are definitely capable to enhance or decrease the ^{10}Be deposition locally and during a time interval long enough to misinterpret shifts in ^{10}Be deposition/ice concentration as production change.

l. 13: "variations in production and climate have to be taken into account" - we do not know the production rate, and the climate is described only as precipitation.
That is why we wrote „have to be taken into account“ and not e.g. "have to be calculated“.

l. 20: "only geomagnetic variations have been clearly proven" - only because the time resolution of long ice cores is too coarse. The Sun also exhibits millennial-scale cycles. We do not know them, so it is fair enough to leave them out, but just mention this.
Please beware that we talk about multi-millennial variations in the ^{10}Be production – time resolution of ice core ^{10}Be is enough to resolve multi-millennial oscillations. In our opinion there is too few evidence to state that multi-millennia-scale cycles in solar activity have been clearly proven. There may be long-term oscillations in the Holocene **production** of ^{14}C and ^{10}Be. However, on this timescale, disentangling of geomagnetic and solar activity still relies on basic assumptions. Knowledge on geomagnetic field changes during the Holocene is still insufficient.

p. 781: l. 4: "might show" -> shows
See comment above

2.3.2 Climate variability

l. 12: "concentrations" ... nor atmospheric circulation
GCM studies investigate implicit climate modulation of ^{10}Be based on explicit (manual operated) changes in climate conditions as greenhouse gas concentrations. Atmospheric circulation changes are not (explicitly) prescribed – as in case of greenhouse gases.

l. 14-17: Precipitation rate is the only factor you can vary because only it is known, and not because only it would be important. Please say it as it is, otherwise readers will be mislead.
We changed the section into: „...changes which are comparatively well known. Precipitation governs both, the tropospheric residence time of ^{10}Be...“

l. 15: "atmospheric residence time" -> tropospheric residence time
Changed

l. 26: atmospheric concentration might be robust in the present but hardly during glacials.
We included „present day“

l. 26-2: These two sentences seem a contradiction?
We could not trace the background of this question, no changes made therefore. Please give more details...

l. 5: the advantage of using complex models is that these processes do not need to be understood, i.e. parameterised by constant factors, but are explicitly described by differential equations in a physical way. They would allow atmospheric conditions to vary depending on different climatic conditions (temperature,

humidity etc.) and give a more realistic description of formation of clouds and precipitation, leading to deposition of ^{10}Be.
We rewrote this section:

„Still, processes of ice sheet boundary layer atmospheric transport are not understood sufficiently and Global Circulation Models have issues with reproducing polar ^{7}Be sufficiently well. We are thus not sure if usage of complex climate models would significantly improve simulations of local conditions on the ice sheets."

l. 20: I understand the difficulty of parameterising the precipitation, but how could it be constant in the northern hemisphere during the D-O events, for example? Or do you mean that the model is insensitive to this?
We refer to results from the Paleoclimate Model Intercomparison Project (see http://pmip2.lsce.ipsl.fr/ or Braconnot et al. (2007) for a review): Precipitation changes are largest in polar areas (i.e. ice sheets and sea ice) – especially in terms of relative changes. In case of mid and low latitudes, the drying is less when averaged over longitudes. We agree that this a rough simplification. However, our model investigations show, that the ice sheet boundary layer ^{10}Be air concentration is not very sensitive to precipitation changes outside the ice sheet areas.

p. 783, l. 1: "supplied by" ---> supplied from
Changed

l. 3-4: not sensitive, but in the Results you show large changes (figure 8)?
We apologize for poor phrasing. We here refer to precipitation changes beyond the ice sheet box (see details of the sensitivity studies l.20-29). Changed to „...not very sensitive to precipitation changes outside the ice sheet box."

l. 11: "Greenland and Antarctica" - the d^{18}O looks completely different in Antarctica than in Greenland, and does not respond to precipitation changes in Antarctica as well as in Greenland.
We apologize for poor phrasing. We changed the sentence into:

„...for future investigations, model simulations can be easily expanded for different sampling sites in Greenland and Antarctica, if site-specific changes of snow accumulation are known sufficiently well."

p. 784, l. 5: Assuming that the data is all perfect, your difference between model and observation is basically the solar modulation
...and the unconsidered climate changes (see other comments by reviewer 2) which may also hold for the Holocene period.

Discussion

The results basically indicate that the ^{10}Be concentration is close to being a linear combination of d^{18}O and VADM, with the Laschamp event sticking out, and some noise, as nothing else was varied in the model. This is a nice result but might be found out even without running the model.
It is true that our long-term model simulations are driven by changes in snow accumulation and VADM, only. We did so far not address the degree of non-linearity in the relation between model input and model results – this question is indeed not straightforward but requires closer investigation. However, regarding our glacial-holocene model simulations, the decisive result of our study is that the model is not able to reproduce the observed ^{10}Be ice core records in detail. We thus conclude that standard-practice in ^{10}Be ice core research (i.e. in reconstructing the snow accumulation or VADM from ^{10}Be records) needs review. How can this result might be found out with observed ^{10}Be records, only?

l. 20: "precipitation and snow accumulation changes", precipitation rate essentially equals snow accumulation, at least so far both terms have been used interchangeably in the paper.
We use the term precipitation for our global atmospheric transport model and snow accumulation in case

of our air firn transfer model (see further replies above).

l. 20: geomagnetic changes less decisive. This could be found out by analysing the variations, say standard deviations of the data.
We agree. It does not need our model to figure out that the geomagnetic influence on ^{10}Be is less than the overall climate influence. Nevertheless, we hold that this finding is not clearly expressed in the literature, yet. In fact the standard practice of reporting estimated ^{10}Be deposition fluxes suggests the unknown reader that geomagnetic changes impose major variations on the ^{10}Be ice core record while climate-driven changes are less decisive.

p. 786, l. 3: remove the word "explicitly"
Done

l. 6: "more sensitive to solar than geomagnetic variability" this depends on the amplitude of their modulation.
Changed to:

"Indeed our model results show that the polar ^{10}Be ice concentration is more sensitive to solar activity than the global mean ^{10}Be air concentration (see 2.1.4)"

l. 19-20 & 26-27: again, I'm confused by the use of the terms "atmospheric sink" and "air-firn transfer", what is the difference between them?
See general reply point III and further replies above: Atmospheric sink refers to our global transport model while air-firn-transfer is related to the local model! We replace „sink" with „sink strength" and „air-firn-transfer" with „air-firn-transfer flux".

l. 24: as you say, a linear scaling of precipitation and ^{10}Be is risky.
As you say we mention it.

l. 29: "modulation of atmospheric transport" - atmospheric transport is not really a process one can modulate, it is a combination of many non-linear processes which interact with each other. Describing them as one with percentages of modulation (constant in time) is quite nonrealistic.
We replace „modulated" with „changes"

p. 787, l. 3-4: "Vertical atmospheric ...." Indeed.
Please give more details on this comment!

l. 9-> We will never know them that precisely, not even for the present time. This is why physical models are used.
Changed into: „Adequate knowledge of changes in..."

Summary and outlook

l. 1: move "within this study" to the end of the sentence
We deleted „within this study"

l. 2: "quantitative" is a dangerous word in light of the uncertainties and non-considered processes.
However it is true since we do not use any normalized data prior to the discussion of our long-term model results. In fact there is a strong need for this kind of „quantitative" approach in ^{10}Be ice core research to show the uncertainties involved. So far, ^{10}Be ice core studies oversimplify the processes in e.g. equating relative changes of observed ^{10}Be with relative production changes.

l. 22-> see previous comments. A table showing how much of ^{10}Be is produced within each box and how much of it is deposited in which box would help to quantify this finding. A further point is that the

Laschamp peak, found in either polar ice cores or lake sediments at lower latitudes always shows an increase of ca. a factor of 2.
We here refer to respective comments above. Lake sediments are an important contribution to ^{10}Be research. However, their resolution is generally too low to exactly estimate the amplitude of short events like the Laschamp geomagnetic reversal.

l. 14-15: how can you differentiate between long solar cycles and geomagnetic changes if they act on the same time scale?
This is just a sensitivity analysis in which the deviation is arbitrarily attributed either to solar or to geomagnetic activity. For clearness we inserted the following sentence: „We investigated potential contributions to the model-measurement mismatch within a sensitivity study:"

l. 23: Does this study shed more light to the interpretation of the ^{10}Be data? I think the climatic modulation issues remain unsolved.
See comments above! We give no detailed explanation of climate modulation but show that state-of-the-art reconstructions of precipitation/accumulation rate changes are not sufficient to explain climate modulation of ^{10}Be. The reason for this may be related to deficient (state-of-the-art) model input records or unconsidered climate modulation. In both cases, standard-practice in ice core research needs review. Eventually, we present a model tool which is well suited for testing of different hypothesis on ^{10}Be climate modulation – e.g. arising from GCM studies.

l. 25: prescribed climate changes - how can you prescribe them by a few parameters?
Within our paper we show that our basic model approach reproduces the observational period global atmospheric cycle of ^{7}Be (see Fig. 1-2). If this is true, it is very likely that the model is technically suitable to simulate aerosol-borne radionuclides during different states of climate. Model settings for different climate conditions differ in several parameters and these changes have to be prescribed. To avoid misunderstanding we changed the sentence to: ."..sensitivity studies on the effect of prescribed, climate-related changes of ^{10}Be transport and deposition (e.g. less/more Stratosphere-Troposphere-Exchange) on ice core ^{10}Be."

Additional references

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