Review of "Simulating ice core 10Be on the glacial-interglacial timescale" by C. Elsässer et al. submitted to the Climate of the Past

This study presents a modeling approach of climatic influences on 10Be 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 parameterised particle removal scheme. Different modeling approaches to interpret 10Be 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.

The way the model is set up now for the glacial simulation suggests that atmospheric circulation is kept "constant", precipitation rate is prescribed, 10Be production follows the geomagnetic field reconstruction and the model deposition responds linearly to precipitation changes. The outcome of the simulation therefore shows that 10Be snow concentration is modulated by precipitation/snow accumulation changes as well as geomagnetic and solar modulation on the 10Be 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 10Be during glacials.

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, interepretation of the results which are based on atmospheric circulation should be made with extreme care.

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 10Be 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 10Be: 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 10Be from the level of clouds and by vertical transport of 10Be within the clouds. In contrast, dry deposition and sedimentation have a minor effect on tropospheric circulation of 10Be. 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).

More detailed comments:

Abstract:

l. 2-3: "ice core measurements" .. "a tool to study" "reconstruction of past solar activity or variation in the natural 14C production rate" - I think the main purpose to study the natural 14C production rate is to reconstruct the solar activity, too, and to compare it with 10Be, but can 10Be be called a "tool" for it?

l. 11: "Being specifically configured for polar 10Be" - This sounds rather limiting. The authors seem to use the model for all latitudes, especially when validating it.

l. 13-14: "We find that the polar 10Be 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 10Be 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"

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?

l. 24-25: "In fact, the 10Be ice concentration is very sensitive to snow accumulation changes" - The flux of 10Be 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 10Be flux? This is of course reasonable because snow accumulation reconstructions based on d18O have been shown to vary more strongly than the 10Be production rate.

Introduction

l. 7-8: "and is thus deposited to polar glaciers" - 10Be is deposited everywhere, not only to polar glaciers. Also remove the word "thus".

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.

p. 764, l. 5-> "Measurements of 10Be (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.

l. 8: Next sentence "Even in case of minor climate...." this is a strong statement. Do you have evidende? The fact that for example the 11-year cycle is found in virtually all data rather suggests the opposite.

l. 10: "While geomagnetic changes primarily affect atmospheric" -> remove the word primarily, they only affect 10Be production at low latitudes

l. 15: "While during the Holocene.... production changes related to solar activity" - 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 10Be deposition is altered.

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 10Be

production there can't be a change in the 10Be deposition in a global sense. The 10Be ice concentration will be altered if snow accumulation/precipitation changes even if 10Be deposition flux is constant, but this would be trivial.

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).

l. 3: Climate modulation occurs on long and short time scales

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.

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?

l. 17: "hand-over from 10Be air concentration to ice concentration" - here a more physical explanation is needed, see comment 2) in the beginning of the review.

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.

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?

p. 767, l. 6: STE and especially precipitation exhibit enormous variability on zonal scale, too.

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 10Be 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?

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.

l. 16: observed 10Be/7Be at the surface? See comment *)

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.

l. 19-20: "given the climatological approach" - climatological usually refers to long-term and interannual variability, not seasonal.

Figure S1 gives the distribution of air mass between the boxes, but the distribution of 10Be would be helpful, too.

l. 28: "significant differences": what are they?

l. 5: 222Rn: there are very large differences in 222Rn 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.

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?

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 7Be observations this could make a difference. One should be careful, the global coverage of the observations is still very limited.

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?

l. 19: Aren't the residence times in the model tuned to reproduce the observations? Therefore the good match would not be surprising.

p. 771, l.1: ...or the deposition strength or atmospheric circulation

l. 11: "low data basis" - what do you mean?

l. 12: could only the months be compared when you have observations?

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 222Rn emission. If your model is averaged over all longitudes, this could be important.

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

l. 5-6: precipitation also varies very largely over the North Atlantic versus the continents at the same latitudes.

l. 19: the production should be well established by Usoskin et al., 2011.

l. 22: "well" -> "reasonably" or similar

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.

l. 22: Usoksin -> Usoskin

2.2 Local air-firn transfer

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

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

l. 10: The Greenland ice sheet is also 3km thick

l. 15-18: air concentrations and thus flux into the ice are constant -> snow concentration depends on precipitation rate which varies spatially

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

p. 775, l. 3: 10Be concentration - where? Should be the concentration at the surface

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?

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.

p. 776, l. 3: "three parameters governing the hand-over of 10Be 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.

l. 16: Now you're also assuming a global mixing of 10Be production signal?

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.

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.

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 10Be 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.

p. 778, l., 2: "constant 10Be/7Be ratio" - this might be risky.

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.

p. 779, l. 1: again, the rainout processes are very complex, depeneding 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.

l. 9: "10Be ice concentration is less influenced by accumulation rate" - your figure 4 seems to contradict this.

l. 10: "10Be ice core concentration a primary reference for atmospheric 10Be and related production changes" - This is not correct. 10Be 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.

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?

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.

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.

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 millennia-scale cycles. We do not know them, so it is fair enough to leave them out, but just mention this.

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

2.3.2 Climate variability

l. 12: "concentrations" ... nor atmospheric circulation

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.

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

l. 26: atmospheric concentration might be robust in the present but hardly during glacials.

l. 26-2: These two sentences seem a contradiction?

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 10Be.

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?

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

l. 3-4: not sensitive, but in the Results you show large changes (figure 8)?

l. 11: "Greenland and Antarctica" - the d18O looks completely different in Antarctica than in Greenland, and does not respond to precipitation changes in Antarctica as well as in Greenland.

p. 784, l. 5: Assuming that the data is all perfect, your difference between model and observation is basically the solar modulation

Discussion

The results basically indicate that the 10Be concentration is close to being a linear combination of d18O 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.

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.

l. 20: geomagnetic changes less decisive. This could be found out by analysing the variations, say standard deviations of the data.

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

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

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?

l. 24: as you say, a linear scaling of precipitation and 10Be is risky.

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.

p. 787, l. 3-4: "Vertical atmospheric ...." Indeed.

l. 9-> We will never know them that precisely, not even for the present time. This is why physical models are used.

Summary and outlook

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

l. 2: "quantitative" is a dangerous word in light of the uncertainties and non-considered processes.

l. 22-> see previous comments. A table showing how much of 10Be 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.

l. 14-15: how can you differentiate between long solar cycles and geomagnetic changes if they act on the same time scale?

l. 23: Does this study shed more light to the interpretation of the 10Be data? I think the climatic modulation issues remain unsolved.

l. 25: prescribed climate changes - how can you prescribe them by a few parameters?