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Interactive comment on "Simulating ice core ^{10}Be on the glacial–interglacial timescale" *by* C. Elsässer et al.

Anonymous Referee #1

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Elsässer et al. present a 2-D box model for atmospheric transport and deposition of 10Be. With this atmospheric transport and a local air-firn transfer model they produce a theoretical ice core 10Be curve based on reconstructed geomagnetic field changes, snow accumulation rate reconstructions and theoretical 10Be production rate estimates. The model is validated by observational 7Be and 10Be snow pit data. Elsässer et al. conclude that the geomagnetic modulation effect is strong dampened when compared to the global average. They also infer large differences between model results and measured 10Be data. They conclude that unaccounted changes in climate could explain the observed differences.

I think this is an interesting modeling approach that can shed some light on the discussion of production versus climate effects on the 10Be deposition. The model appears to have a reasonable degree of complexity to capture the major processes but it is simple enough to run long model runs. I am slightly concerned about the tuning of the model with the help of recent data. There is of course the question if the tuning produces good model results also for completely different climate states. The authors are aware of this problem and discuss it but they do not show systematic sensitivity tests to investigate the reasons of the model-data disagreement in detail. Even if much more could be done in this respect I recommend publication of the manuscript after some revisions as the authors present a useful model that could be used in future investigations of the 10Be transport and deposition.

Comments:

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 todays 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 10Be 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?

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 10Be 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.

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Regarding figure 1 it would be interesting to see if Masarik and Beer's results only underestimate the mean 7Be concentrations or if also the latitudinal gradient is disagreeing with the measured/modeled data.

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).

The seasonal and longer-term variations in 7Be. 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).

Page 4 lines 8-10: "Even in case of minor climate changes of air mass transport, the degree of atmospheric mixing of 10Be has major influence on the production signal recorded in ice core 10Be." 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?

Section 2.1.3 concludes with "The model is thus also capable of simulating atmospheric 10Be, 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 7Be is more sensitive to shorter-term processes compared to 10Be. Therefore, a model that works well for 7Be might not work so well for 10Be. 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.

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Interactive Comment

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.

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?

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.

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 10Be concentration". One can follow the exact meaning after reading the paper but rewriting would be useful to make it clearer in the abstract.

Page 4 lines 16-18: "10Be 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 10Be the 10Be concentration or the 10Be flux? In addition, the reference is outdated since Finkel and Nishiizumi used a (now) outdated accumulation rate record.

Page 5 lines 12-14: "Indeed, monitoring of radionuclide air concentration in polar areas reveals that climatological features of atmospheric 10Be have large spatial validity"

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What is a "climatological feature of atmospheric 10Be"?

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. 10Be and d2H 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

I was surprised not to see the reference: Variability of 10Be and d18O 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

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

Figure S.5 & S.7: labels seem to be missing

The authors should check for typos in the supplementary information

Filling words such as "basically", “definitely”,. . . should be removed from the manuscript

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Interactive comment on Clim. Past Discuss., 10, 761, 2014.