The authors would like to thank the referees for their valuable comments which helped to improve the manuscript.

Anonymous Referee #1

1. Text: There are various repetitions throughout the draft, mainly concerning the principle modelling setup. For example, on page 3683, last paragraph is a brief description on the methods (although this is written in the Introduction, where I believe other things should be written down), the description of the simulations, section 2 on page 3685 find more or less similar informations on the methods (similar to the Intro), but they are also partly repeated in the first and second paragraph on that page. Brief versions of the methods are also found in the abstract and in the final summary. While abstract and summary surely need to contain some information on the methods, the Intro and methods needs major streamlining in that respect.

We carefully went through the manuscript and removed repetitions. Details of the models are now explained in section 2: "Description of the model simulations". We kept the short introductions of the models in the end of the second paragraph of the Introduction in order to explain why this model setup was chosen for this study.

2. Results and Figures: All results are shown in anomaly to the pre-industrial control run. I understand, that this is a typcial way to communicate such simulation experiments (and I have nothing against it), but I am missing the results of the control run here. To get an idea about the importance of these anomalies, it is very useful to have the control results also available, maybe even briefly men- tioned in the text. Because most figures (Figs. 2–4, 6–10) consist of 3 subfigure with the anomalies, I strongly suggest to use the available white space for a 4th subfigure each to show control run results (then of course with other units and color bar).

We find this a good idea and have now included the results of the control simulation into Figures 2-4 and 6-10.

3. Wet versus dry depostion: Since Fig 8 shows only the changes in the wet ^{10}Be deposition it is hard to tell, what this means for the absolute numbers in dry deposition. Do they increase? I therefore suggest to split the analysis of the ^{10}Be deposition (also the suggested correlation with precipitation, see next point) into two analysis, one only calculating wet, the other only dry deposition. The total changes in deposition (and the relative share of the wet fraction), now Figs 7 and 8 are only the subsequent results of that split analysis.

Figure 8 shows the wet fraction, i.e. the wet deposition divided by total deposition. We have now included a better description in the figure caption. We also include a figure showing only the dry deposition and sedimentation (Figure 9).

4. Results: My reading of the figures tells me that the ^{10}Be deposition is to a large extent depending on precipitation (e.g. compare Figs 7 and 8 with Fig 4). I would expect some more in-depth analysis of this dependency, e.g. correlation coefficient of precipitation with total ^{10}Be deposition and with wet ^{10}Be deposition for every grid cell. However, since the plots showe only the anomalies, and not the absolute numbers, it is hard to judge if this correlation coefficient or relationship is similar stong for absolute values, please check and discuss.

This observation is correct. The spatial distribution of 10Be deposition largely follows 1) precipitation rate and 2) the location of the stratosphere-troposphere exchange, i.e. to transport the 10Be signal into ice firstly precipitation is required but also the atmospheric concentrations need to be significant. The connection is complex: some precipitation is required to wash particles out of the atmosphere, but if the precipitation event is too strong, 10Be concentrations in precipitation become diluted. In dry areas with nearly no precipitation (roughly < 0.5-1 mm/day) dry deposition processes become dominant. A full assessment of the dependencies, including correlations, between precipitation rate and 10Be deposition rate in space and in time has been made by Heikkilä and Smith, 2013. They show that 10Be deposition variability is mostly driven by precipitation variability on sub-annual (monthly) time scales. On longer time scales (annual) the production variability dominates, however on decadal time scales the precipitation variability might become important again in case long-term trends are present. The temporal variability, however, will be addressed in the second part of the manuscript. We have now added these explanations into the manuscript (section 3.3, third paragraph), and also show the absolute values of precipitation and 10Be deposition in the figures, following the referee's suggestion.

5. Results: The description of results in the text is often very vague, one would expect more hard numbers, e.g. p 3687 "... stratospheric and tropospheric resi- tence times are both increased ..." By how much? Whenever numbers can be given, they should be mentioned in the text, not only that there was an increase or decrease in a certain variable, but also by how much.

We have included the numbers into the text and refer to Table 2. More numbers have been added throughout the text. Where the changes are too complex (too much spatial diversity) to put into numbers, we refer to the figure discussed.

6. Title: Calling the study in the title and throughout the text a study of "deglacial" climate influence on ^{10}Be deposition is slightly misleading, when only climate at 12, 11, 10 kyr BP are investigated. If its called "deglacial" I would expect also scenarios earlier in the last termination. I therefore strongly suggest to refine the wording here, maybe to something like "Influence of Younger Dryas to Eary Holocene climate on ^{10}Be deposition..." or "Late deglaciation climate anoma- lies...".

We agree. These are time window simulations, i.e. snapshots, which do not cover the entire period of deglaciation. However, they also do not describe a transition, such as "from Younger Dryas to early Holocene" might suggest. We modified the title to: "10Be in late deglacial climate...".

7. Results: Maybe I missed it: Which data were analysed for the plotted results? The final year of the simulation, or the full 30 yr? Please state.

We used the entire 30-year period. This is now stated in Section 2.

Minors:

1. Intro: Changes in production rate during the Holocene were recently published in *Roth and Joos* (2013), please discuss, might have an impact on the 10k scenario.

The study by Roth and Joos (2013) is very useful and improves the reconstruction of the 14C production rate from previous estimates by employing a fully dynamic carbon cycle model. Especially the fact that the model distinguishes between the hemispheres is an improvement from the global box models. Their results do not directly affect our study, which uses the same production rate in all simulations to isolate climate influences from the deposition. We however acknowledge the valuable results by Roth and Joos (2013) in the end of the Introduction to describe the state of our understanding of the cosmogenic radionuclide production during the early Holocene.

2. Table 1: If I got it right: greenhouse gases are the FORCING used, temperature and precipitation are model results, right? If so, please say so.

Correct. This is now explained in the Table caption.

3. Table 2: Please use full words for stratosphere and troposphere in the table. How can production be in %, should not it be number of atoms or grams per time unit?

The full words are included. The table shows the fraction of stratospheric production of total, which is given in %. This is now clarified in the table.

4. Fig 1: Units of Deposition is "atoms per m^2 per s". Please delete "o" and "x" in y axes labels.

The units in the figure are indeed atoms per m2 per s. The 'x' and 'o' have been removed.

5. Figs: X and y axes need labels and units, x is most of the time latitude, y pressure?

Correct, labels and units have been added.

6. Fig 6: All other anomalies have blue for positive and red for negative changes, herein Fig 6 it is the opposite, I suggest to used the same throughout the draft.

The colors have been changed.

H. Fichtner (Referee #2)

Major:

(1) What is the reason to include both cases "10k" and "11k" for the simulations, given that their characteristics as provided with Table 1 are almost identical? From those values one would certainly not expect to find any significant differences in the corre- sponding 10Be simulations, which appears to be confirmed by the results described.

The main reason to study these periods were new, unpublished, 14C and 10Be data measured between 12-10k BP by another group. Their results show that 14C and 10Be match well at 10k BP, at 11k both of them exhibit a similar large peak, but deviate strongly at 12k BP. We considered these features interesting: a period during near-Holocene conditions (10k), a period with a significant production increase and a potential climatic distortion of one or both records (11k) and a clear deviation between the records, caused by a large climatic shift (12k). Furthermore, the different orbital parameters and greenhouse gas concentrations could have produced results which are significantly different from control. Our results: no significant climatic distortion at 10k or 11k from control, but at 12k the changes become more meaningful, agree with these findings in general terms despite of the theoretical production rate used in the model. However, because the new 10Be data has not been published yet, we do not discuss these details in the manuscript.

(2) In the last sentence (page 14) it is stated that "the reconstructed snow accumula- tion rate [...] adds some uncertainty". It would be helpful to a reader to quantify this uncertainty.

This depends very strongly on the quality of each reconstructed snow accumulation record, for example how well the snow and weather conditions at the site are known. This translates into dating uncertainty. This varies with each record and one has to refer to the actual publications of the records to quantify the uncertainty. In any case, it does increase the uncertainty of the 10Be flux record, however a general quantification is not possible. We added this explanation into the text.

Minor:

(a) The title would better (?) read "10Be in the last deglacial...".

Following a suggestion by Referee #1 we changed the title to "10Be in late deglacial climate"

(b) A few abbreviations (EOF, SLP, SAM, NH, SH) appear to be undefined or to be de- fined too late in the manuscript. Although their meaning might be obvious, a manuscript should be self-contained. The abbreviation "GHG" is introduced on page 4 but not strictly used in the following text.

We have now included an explanation of the abbreviations at the first occurrence, and use only "GHGs" instead of "greenhouse gas concentrations" after introducing the

abbreviation.

(c) Shouldn’t the sentence "... sea ice cover to be described." (page 3) better read "... sea ice cover to be prescribed." ?

Changed.

(d) The x- and y-axes in Figure 2 and the x-axis in Figure 3 need labels.

We have now included labels into the figures.