Reply to the RC1's comments on "Observation and modeling of high-^{7}Be events in Northern Europe associated with the instability of the Arctic polar vortex in early 2003" by Erika Brattich et al.

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We thank the reviewers for their useful comments. Below are the reviewers' comments in italic followed by our replies in blue text.

RC1

The manuscript presents the results of an analysis of the atmospheric conditions, in particular, a stratospheric sudden warming (SSW) event in late February 2003, leading to an observed increase of 7Be concentration in the near-ground air. The qualitative analysis is comprehensive, and the association between the SSW event and the observed 7Be increase is convincing and worth publishing. However, the quantitative model contains a serious flaw and needs to be corrected before the manuscript becomes acceptable since the modelled 7Be concentrations cannot be trusted.

This reviewer was deeply surprised by the rough and inappropriate way the production of 7Be was modelled. The authors state that they used production estimated by Lal & Peters (1967, called LP67 here) for 1958 (was it based in Fig. 20 there?), which is unacceptable for several reasons:

i) the model of LP67 is greatly outdated as based on a very rough and approximate approach (an analytically estimated rate of nuclear "stars" in the atmosphere converted with the mean production yield of 7Be per star). This approach is quite uncertain as compared to modern full Monte-Carlo simulations of the cosmic-ray-induced atmospheric nucleonic cascade. Instead, the most recent and accurate production model by Poluianov et al. (2016, doi: 10.1002/2016JD025034), based on the GEANT-4 Monte-Carlo tool, is highly recommended for use. Comparing to the full Monte-Carlo model, the results by LP67 OVERESTIMATE the 7Be production by 30-50% (cf. Tab.3 of LP67 and Tab.1 of Poluianov et al., 2016).

ii) The level of solar activity and the corresponding modulation of cosmic rays (hence 7Be production) in 1958 was significantly higher than that in 2003, as the authors realize (see line 295). Accordingly, by applying the 1958 production to 2003, the authors UNDERESTIMATE the production.

iii) The authors ignore the change of the geomagnetic field strength, which was reduced by ~4% between 1958 and 2003. In this way, they also slightly UNDERESTIMATED 7Be production.

Altogether, the three errors work in opposite directions making the quantitative result unreliable. The authors are requested to redo modelling using an appropriate 7Be production model. In case this would require too much work, the authors can make a compromise: the present LP67-based model results can be scaled to the correct global (or polar) production estimated by an appropriate model. However, this would be only an approximate temporal solution. In all further works, the authors are required to use a relevant production model. Before this flaw is corrected, the manuscript cannot be accepted for publication.

We thank the reviewer for his/her comments and suggestions. We have improved the discussion on the use of the Lal and Peters (1967) ^{7}Be production rates in the revised version at lines 186-194. Indeed, published estimates of ^{7}Be production rates (Lal and Peters, 1967; O'Brien et al., 1991; Masarik and Reedy, 1995; Masarik and Beer, 1999; Usoskin and Kovaltsov, 2008; Poluianov et al., 2016) greatly differ, with global mean column production rates ranging over an average solar cycle from 0.035 atoms cm^{-2} s^{-1} (Masarik and Beer,

1999), 0.063 atoms cm^{-2} s^{-1} (O'Brien et al., 1991), to 0.081 atoms cm^{-2} s^{-1} (Lal and Peters, 1967). Previous studies (e.g., Koch et al., 1996; Liu et al., 2001) found that using the O'Brien et al. (1991) ^{7}Be source (0.063 atoms cm^{-2} s^{-1}) in global models yields a consistent underestimate of observed surface and stratospheric ^{7}Be concentrations. The ^{7}Be production rates provided by Usoskin and Kovaltsov (2008) and Poluianov et al. (2016), with global mean values of 0.062 atoms cm^{-2} s^{-1} and 0.065 atoms cm^{-2} s^{-1}, respectively, are broadly consistent with those of O'Brien et al. (1991)) and are about 25% lower than those of Lal and Peters (1967). We use the ^{7}Be production rates recommended by Lal and Peters (1967) for a maximum solar activity year (1958), which has been shown to produce the best results compared to aircraft ^{7}Be observations in the stratosphere where ^{7}Be concentrations mainly result from a balance between production and radioactive decay and their observations can be used as a constraint on the ^{7}Be source (Koch et al., 1996; Liu et al., 2001). Although using the most recent production rates of Poluianov et al. (2016) is planned for a future modeling study (e.g., Golubenko et al., 2021), using those rates would likely result in model ^{7}Be concentrations biased low, especially in the stratosphere as aforementioned. In addition, it is noted that the focus of this paper is not on the perfect agreement between model simulations and observations, but rather on the use of model simulations to investigate the processes responsible for the ^{7}Be peak observed at different northern latitude stations in Fennoscandia in early 2003.

Other minor comments and suggestions are listed below:

1) The title would sound more correctly if "high-7Be events" was replaced with "high 7Be concentration events".

We thank the reviewer for the suggestion. The title was changed accordingly.

2) It would be worth to refer to previous works on full atmospheric dynamical models applied for studies of 7Be transport/deposition: the ECHAM-HAM5 (Heikkilä et al., ACP, 2008, doi: 10.5194/acp-8-2797-2008) and the GISS model (Usoskin et al., JGR, 2009, doi: 10.1029/2008JD011333)

We thank the reviewer for this suggestion. Both references are now cited in Introduction (lines 35-40).

3) Line 32: "stratospheric influence" on what?

We have revised the sentence to "^{7}Be is considered a tracer for intrusion of stratospheric air to the troposphere and large-scale subsidence (e.g., Liu et al., 2016; Chae and Kim, 2019; Heikkila et al., 2008)".

4) Line 193: "previously archived restart files" – please specify what it is.

We have revised the text at lines 229-231 to "All model simulations are conducted for the period of January 2002 – March 2003 with initial conditions from a previous five-year simulation. Hourly and monthly mean outputs for January-March 2003 are used for analysis."

5) Line 218: NMSE does not provide an estimate of whether the difference is statistically significant or not. Z-test is recommended instead, which gives a measure of the statistical significance of the difference.

Agreed. The evaluation of the Z-test has been added to Table 3 and discussed in lines 363-365.

6) Line 223: the statistical significance of the correlation coefficient should be evaluated. With so short

analyzed series, even a high correlation coefficient can be insignificant.
Agreed. All values but the one at Risoe were significant, and this information has been added to Table 3.

7) Line 273: the correlation of -0.32 for Ivalo implies a failure. This needs an explanation.
An explanation for this observation was added in lines 315-320: "The low negative correlation at Ivalo is due to the fact that while the GPCP-observed precipitation at this site is similar between January and February with a general tendency towards lower values from January to March 2003, the model simulates a decrease from January to February with a small increase in March. However, the statistical parameters reported in Table 1 indicate that an overall small discrepancy between the GPCP and MERRA-2 precipitation at all sites."

8) Line 296: reference to O'Brien (1979) is not appropriate here. A review by Potgieter (Liv. Rev. Sol. Phys., 2013, doi: 10.12942/lrsp-2013-3) is recommended instead.
The reference was modified accordingly.

9) Finland is not a part of Scandinavia. The analyzed region should be called Fennoscandia.
We thank the reviewer for this suggestion. The revised version now properly refers to Fennoscandia instead of Scandinavia.

10) The term of the stratospheric fraction of 7Be needs to be strictly defined. Presently, it is presented as the ratio of stratospheric to global concentrations, which is vague. Are these concentrations mean global or polar regions, for what period (tropopause height varies in time). Please provide a formula.
We have revised the text (lines 224-229) to "In addition to the standard model simulations of ^{7}Be and ^{210}Pb, we separately transport ^{7}Be produced in the model layers above the MERRA-2 thermal tropopause (i.e., stratospheric ^{7}Be tracer) to quantify the stratospheric contribution to ^{7}Be in the troposphere. This approach was previously used by Liu et al. (2001, 2016). Stratospheric fraction of ^{7}Be is defined as the ratio of the stratospheric ^{7}Be tracer concentration to the ^{7}Be concentration from the standard simulation."

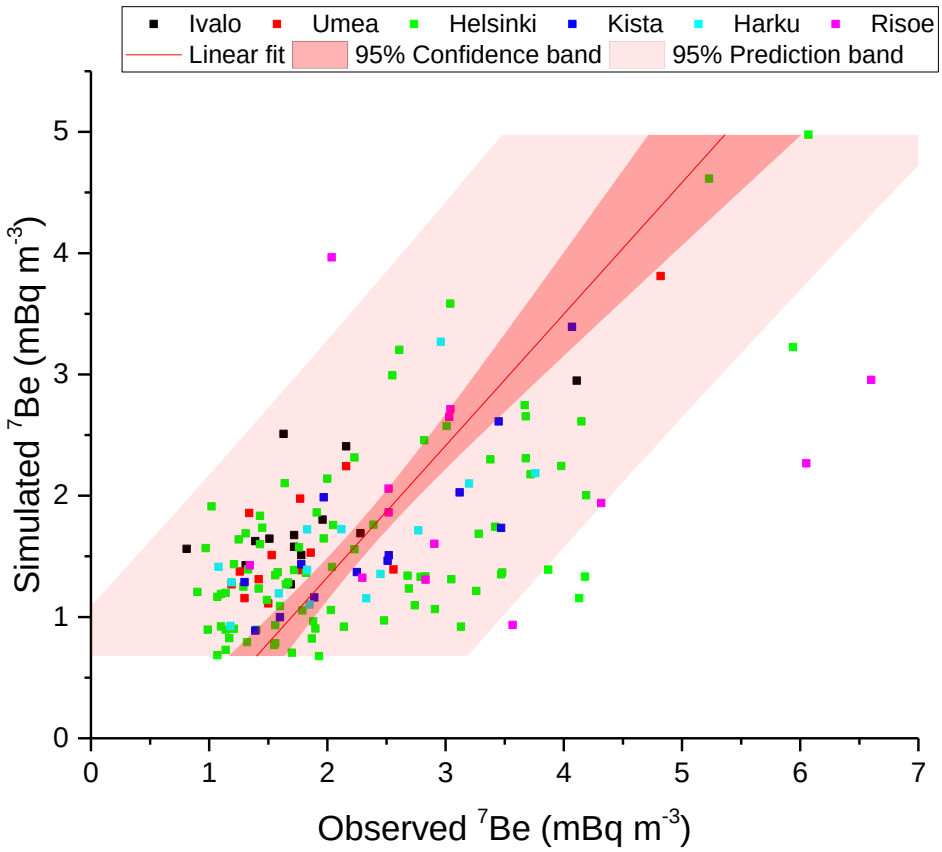
11) Line 413: the model tends to underestimate 7Be concentrations – see the major concern above.
Please see our response to the major comment above.

12) Figure 1b: for what periods were the 90% levels defined? The upper dotted line rises many questions: only two points lie above it, how many are overall? Why does the line lie between points? Dotted lines need to be marked as to which station they correspond to. It is recommended that the points are connected, otherwise, it is hardly possible to distinguish data from different sites.
The 90th percentile levels were defined over the 1995-2011 period, and peak events were defined as values exceeding this threshold. This information was added to the revised version of the manuscript (lines 276-277). In this sense, the time series in Fig. 1b shows only a three-month interval (January, February and March 2003) of the whole 1995-2011 period, with a small number of peak ^{7}Be events, which are the subject of this study. For instance, at Risoe, the procedure allowed to detect a total number of 25 peak events, of which only one was recorded in the boreal winter 2003 period. The figure was revised to contain the information of the sampling site corresponding to each 90th percentile line, and the points are now connected.

13) Fig.4b looks strange. This reviewer would place a linear fit at a shallower slope. Can the authors

specify how the fit was obtained?

Now we state in the Figure 4 caption that "Also shown are the linear regression line and …". It looks strange probably because the two plots are above each other and might "trick the eye". Indeed, when the plot is reproduced alone, the fit looks "normal" (see below).



14) Fig. 6. The authors are advised to use different colours for the lines. Also, the absence of a peak in Risoe data is worth more discussions.

The colours were modified to meet the request from the reviewer as well as the Editorial team to consider colour blindness in the colour choices. Additional discussions for the absence of the ^{7}Be/^{210}Pb peak in Risoe data were added at lines 397-399.