

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2020-1121

Anonymous Referee #2

Referee comment on "Observation and modeling of high-⁷Be concentration events at the surface in northern Europe associated with the instability of the Arctic polar vortex in early 2003" by Erika Brattich et al., Atmos. Chem. Phys. Discuss.,
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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i) the model of LP67 is greatly outdated as based on a very rough and approximate approach (an analytically estimated rate of nuclear "stars" in the atmosphere converted with the mean production yield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.

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".
- 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)
- 3) Line 32: "stratospheric influence" on what?
- 4) Line 193: "previously archived restart files" – please specify what it is.
- 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.
- 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.
- 7) Line 273: the correlation of -0.32 for Ivalo implies a failure. This needs an explanation.
- 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.
- 9) Finland is not a part of Scandinavia. The analyzed region should be called Fennoscandia.
- 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.
- 11) Line 413: the model tends to underestimate 7Be concentrations – see the major concern 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.
- 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?
- 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.