Revision of "Chemistry-climate model SOCOL-AERv2-BEv1 with the cosmogenic Beryllium-7 isotope cycle" by Golubenko K. et al.

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General comments

The authors have replied to all my comments. However, in some cases the response is not completely satisfactory. Below I report my previous major comments in normal style, followed by their response highlighted in yellow, and followed by my new comment in italic.

First of all, the authors talk in and there of beryllium isotopes, but when it comes to the presentation of the results, one can find only results for beryllium-7 (which is also in the title), which generates a big confusion in the reader. This is not a major technical flaw, but truly does not help the reader to follow the work. But now let's move with more significant revisions. The Introduction section does not convey successfully the need of this work and in particular of such a model for beryllium isotopes in the atmosphere. I mean, I cannot find there any technical information on the accuracy of previous models, or on the easy-to-use of such models, so there is no way for the reader to compare the model presented here with previous ones, and conclude about its improvement in one or more directions. I would suggest including more details on how previous models present significant gaps (with some data, if possible) so that the reader can understand immediately how this work goes in the direction of filling those gaps.

We agree with this comment. We indeed are primarily focused on modelling of 7Be isotope as it provides a direct test for the beryllium production+transport+deposition model. On the other hand, in the future, we aim at full modelling of 10Be isotope which is produced and transported similarly to 7Be with only different decay time. That is needed for reconstructions of long-term solar variability and extreme SEP events. We are not aware of any directly validated full production+transport+deposition model of beryllium isotopes, that can be readily applicable to an analysis of past records, viz. without known meteorological data fields. We report such a model here, where the validation is performed vs. 7Be data. Since our primary goal is a validation of the beryllium model with the eventual application for 10Be data, we are focused mostly on high-latitude regions and annual time scales. We have *revised the Introduction* accordingly and added *Section 2 "Summary previous and existing models"*. We hope it is clearer for a reader now.

In this case I am rather satisfied with the revision. There is still confusion regarding the application of the model to beryllium isotopes and then its presentation and validation considering just Be-7.

Also, I do not completely understand the connection between a beryllium model with eventual application to Be-10 data with the choice of high-latitude regions and annual timescales. Could you please better specify this? As commented below in the specific comments, there are still some deficiencies in the added Section 2. Indeed, the review of earlier models is far from being complete, missing important works on this subject. It is true that this field is still widely open for improvements, but the state-of-the-art in this research field has to be consistently described. Secondly, to me the newly added Section is part of the Introduction section, being fundamental for the reader to understand the need of this particular work and its scope. Therefore, to me the introduction of this as Section 2 generates confusion and does not fully help to address the above reported issues.

Moving to the validation of the model comparing the simulated values with observations at four stations in both hemispheres, which to me should be presented before the results of a particular event such as the SEP, I can see some limitations in the discussion and in the presentation of the accuracy, which is far from being fully validated as stated in the abstract and in and there in the manuscript. Indeed, the plots comparing simulated and observed values highlight that the model is not fully capable to catch the interannual variability, and especially in the southern hemispheres does not describe the observed pattern. The analysis of linear correlation coefficients and their significance is limited in this sense, since it does not provide information on the presence of biases but only on the similarity of the reproduced patterns. Additional statistical parameters would be needed to correctly conclude about the presence of biases. In addition, the discussion of the correlation coefficients for stations located in the southern hemisphere is affected by significant flaws. Indeed, the low correlation coefficient found at these stations does not derive from the absence of a seasonal pattern, but instead highlights that the model is completely uncapable to correctly describe the variability of near-ground concentrations at those stations. The reasons of these disagreements, which may actually depend on a number of physical factors including an incorrect reproduction of deposition or transport, are not sufficiently investigated.

We agree that neither cross-correlation nor wavelet coherence can provide information about possible biases, and we have added a new plot (*see Fig. 11*) showing the distribution of the residual difference between the modelled and measured 7Be data. One can see that the null hypothesis of no bias (the mean difference is indistinguishable from zero) cannot be rejected at any significance level, implying no bias even for the southern-hemisphere stations. Thus, both the time variability (coherence) and the absolute levels (no bias) of the data are reproduced by the model.
As commented below, the histograms shown in Figure 11 actually indicate that apart from the mean value of the bias, there are many cases, especially in southern stations, when the bias is actually higher and close to 1. Indeed, the mean low value could result from the distribution of the bias. In addition, the histogram may not be the best way to represent the bias, since the shape of the histogram is dominated by the choice of the bin width. As I previously mentioned, there are a set of statistical parameters to calculate and to validate a model. In addition, it would be interesting to evaluate the pattern of the bias to investigate if it is higher during particular seasons or if conversely the high biases are randomly distributed.

Also, the authors did not present anything of the meteorological data used in this chemistry-climate model, on which some of the disagreements between simulations and observations may actually depend. Indeed, even though the authors state that a gaseous deposition was adopted for beryllium isotopes, which is not sufficiently explained given that in reality beryllium isotopes travel attached to fine-sized aerosols and is thus mainly removed by wet deposition, they have searched for aerosol data when it came to explain some biases.

CCM SOCOL uses ECHAM5 (*see lines 174-175*) nudged with ERA5 (eraiaT42L39) reanalyses. ERA5 is the fifth generation ECMWF atmospheric reanalysis data of the global climate covering the period from January 1950 to the present. ERA5 data are available on the Copernicus Climate Change Service (C3S) Climate Data Store: https://cds.climate.copernicus.eu/#!/search?text=ERA5&type=dataset. We improved the description of the transport as well as dry and wet deposition (*see lines 270-273, 275-278*). It can be found in the updated text and answers to the second reviewer.

Ok with this addition. However, it is still not clear which kind of meteorological data have been used.

The discussion of the SEP event is far from being reasonable and well-given. Indeed, the fact that near-ground concentrations remain quite low, and that high beryllium concentrations increase only in the upper atmospheric layers, probably result from the particular meteorology of the period, which probably did not favor the transport of such high concentrations to the lower tropospheric layers. In addition, the discussion of the dependence on transport is achieved only by a shift of the date of the event to a different season, without giving additional details about the particular synoptic situation characterizing those days, which leaves the interpretation of the differences between the results mostly qualitative and somehow arbitrary.

It is known that extreme SEP events in the past can be studied using 10Be isotope in polar ice cores (e.g., Usoskin et al., 2006; Mekhaldi et al., 2015; Sukhodolov et al., 2018) that typically have the (pseudo)annual resolution. Here we wanted to check, both theoretically and experimentally, whether a weaker (strong but not extreme) SEP event can be observed in high-resolution beryllium data. As far as we know this question has not been fully addressed earlier. Moreover, as our modelling shows, the effect of a SEP event on the near-ground beryllium concentrations slightly depends on the season, because of the different patterns of the large-scale dynamics. During Summer-Autumn, the low tropopause and decreased static stability of the troposphere permit a more direct coupling with the upper atmosphere opening a path for the input of the polar stratospheric beryllium to lower levels. In contrast, in Winter-Spring, the tropopause rises, and intense radiative cooling stratifies the lower troposphere closing this route.

It is true that large-scale dynamics is strongly different in the different seasons, which on average can perhaps be described as you did above, even though there can be situations largely deviating from this average behavior. In addition, the situation that you depict is not the one that is observed *in reality and discussed in many scientific papers in the field. It is in fact well-known that even though the rise of the tropopause and the presence of intense convection during the warmest seasons favors the transport of upper tropospheric air to the surface, the dynamics and the instability of the coldest seasons tend to favor intense intrusions from the stratosphere to ground. However, these events of stratospheric intrusions and transport of air from the upper troposphere-lower stratosphere have to be rather intense to be observed at ground, and are usually observed only at high-altitude stations. In addition, these events do not represent the average situation, but rather a particular one extending for two or three days. Finally, even though these events are typical of the cold season, they sometimes occur even during the other season. There are lots of papers describing the dynamic of stratosphere-to-troposphere transport, a topic that though still not completely understood is not completely obscure and subject of many scientific projects/papers. Just to cite a few of the most important ones, you can see for instance: Cristofanelli et al., 2003. Stratosphere-to-troposphere transport: A model and method evaluation. Doi:10.1029/2002JD002274; Zanis et al., 2003. An estimate of the impact of stratosphere-to-troposphere transport (STT) on the lower free tropospheric ozone over the Alps using 10Be and 7Be measurements. doi:10.1029/2002JD002604; Stohl et al., 2003. Stratosphere-troposphere exchange: a review, and what we have learned from STACCATO. Doi:10.1029/2002JD002490; Tarasick et al., 2019. Quantifying stratosphere-troposphere transport of ozone using balloon-borne ozonesondes, radar windprofilers and trajectory models. Doi:10.1016/j.atmosenv.2018.10.040. My comment was aimed at clarifying exactly that if you do not present the synoptic situation characterizing the SEP event and the other seasons, you cannot be sure that the beryllium produced in the upper atmospheric layers is then transported to ground and therefore your analysis of the event during the SEP dates and in other dates in other seasons likely could be not capable of representing the effect of seasonality on beryllium concentrations.*

To conclude, the authors present lots of technical details which pertain to the methods sections (e.g., information on measurement methods for beryllium, but also modeling information) in the results. I would suggest restructuring the paper to include those details in the methods so that the results section contains only the findings of this work and their appropriate discussion.

We agree and have restructured the manuscript accordingly. Information on measurement methods for beryllium now in *Section 3*.

Ok with this modification.

Specific comments

1) Line 35: Change “from” to “form”.
2) Line 40: Delete the first comma. Change “cadences” to “time resolution” or “sampling resolution” or similar. Consider revise the sentence which is quite long and not totally clear.
3) Line 45 and throughout the manuscript: Again change the term “cadence”.

4) Lines 4-6 and 48-49: In general the models do not reproduce only the transport of beryllium isotopes, but also at least their deposition, and a parameterization of sources (maybe not as accurate as yours), and thus the simulated concentrations do not depend only on transport. This further means that in most cases, input meteorological fields should contain not only wind data. Please consider revise this.
5) Lines 49-50: Please check the two verbs, one is singular and the other is plural.
6) Lines 56-58: A reference is needed for this sentence.
7) Lines 58-60: This sentence, which introduces the scope of this work, is connected with the sentences at lines 107-109, 116-117 and further. I understand the need to introduce the new Section 2 for presenting the state-of-the-art models on this topic, but then wouldn't be Section 2 a part of the Introduction, and thus better presented as Section 1.1? This revision would need not only a change in the numbering, but also a better formulation of the scope of the article and its structure after presenting the gaps of past and recent works.
8) Line 82: Change "ModelE" to "model".
9) Lines 70-94: The review of earlier works is not complete. For instance, much of the work conducted by NASA with the GMI and GEOS-Chem CTMs is missing. Please consider revise conducting a careful review including a more complete overview of the modeling work to represent beryllium isotopes.
10) Line 99: The knowledge of the meteorological fields should be essential to all modeling simulations of this kind, so I do not see this as a significant gap of this methodology.
11) Line 104: Delete comma.
12) Lines 104-106: I cannot fully understand those requirements: do you mean that in general the climatological and beryllium communities do not collaborate?
13) Lines 175-176: Which data? Of which variables?
14) Line 229: Change to "with smaller absolute values".
15) Lines 291-296: References are needed.
16) Figure 11: If the caption is correct and the histograms reproduce the difference between modeled and observed 7Be values, then the title of the x-axis is not. Although the mean reported values are indicative of a low bias, the figure shows that there are cases in which the bias is high, almost equal to 1 in Chile and at Kerguelen, thus contradicting some statements by the authors. In addition, since it is known that the shape of histograms is dominated by the correct choice of the bin width, couldn't you think of a different method to represent the bias?
17) Line 348: The paper of Brattich et al. (2021) has not been added to the reference list.
18) Revise the acronym as being SPE and not SEP throughout the manuscript and figures.