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

MS No: gmd-2021-56

MS Type: Development and Technical paper

General comments

This work presents a chemistry-climate model to simulate beryllium isotopes in the atmosphere. The capability of the model to simulate such beryllium isotopes concentrations is proven through a direct comparison of the simulations with near-ground observations at four stations. While the manuscript represents a valuable contribution to modeling science within the scope of Geoscientific Model Development, the presentation of the findings suffers from a number of significant flaws, for which reason it cannot be accepted for publication in its present state, but needs thorough substantial revisions.

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.

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.

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.

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.

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.

Specific comments

1) Title: I suggest including additional information in the title, such as: “Evaluation of…” or “Chemistry-climate …: description and evaluation”, so that the title is more self-explanatory.
2) Line 1: is “probe” the most appropriate term? Wouldn’t it be better to talk about “tracer”?
3) Lines 3-4: What do you mean by “such ready-to-use model”?
4) Lines 4-5 and following: Here you are talking about “isotopes of beryllium”, but previously and in the title you were just referring to beryllium-7. Please check and modify as appropriate.
5) Lines 5-6: Which isotopes of beryllium?
6) Please use either ^{7}Be either beryllium-7 all along the text to be consistent throughout the article.
7) Line 10: It is not clear which meteorological fields were used (from which model/reanalysis/…)

8) Lines 13-14: perhaps you could insert some statistical parameters proving your statements about the agreement of model simulations with the observations.
9) Line 16: it is not clear what you mean by "dominating data in the Northern Hemisphere".
10) Lines 26-27: Rephrase, this sentence is not clear.
11) Lines 34-36: The two sentences are quite obscure. Perhaps you could rephrase them as: "However, the transport of beryllium (isotopes?) in the atmosphere and its deposition on the surface or into the medium where it is measured may significantly affect the relationship between the production of the isotope and its content (or concentration) in the measured samples."
12) Lines 45-46: Could you provide some more details of the comparison of the model simulations with measurements (e.g., how many locations were compared?) and about the agreement between the model and the observations? This would provide the reader with indications and needs (or not) of the model presented in this paper.
13) Lines 47-48: Same as above, could you provide more details on this experiment and its results?
14) Lines 50-51: This sentence is quite obscure. In particular, it is not clear to me which feature is shared by the works of Pacini et al., 2011 and Brattich et al., 2020: indeed, while the first one presents an investigation of the depositional processes of ^{7}Be-carrying aerosols in the troposphere using a combination of isotopic data with the numerical CRAC:Be7 model of cosmogenic production, the second one investigates the relationship between advection pathways and atmospheric composition (including natural radionuclides of terrestrial and cosmogenic origin) at a high mountain station, using back-trajectory cluster analysis. Possibly, the authors were referring to works like the ones of Liu H. et al. (e.g., Liu, Hongyu, Daniel J. Jacob, Isabelle Bey, and Robert M. Yantosca. 2001. "Constraints from 210Pb and 7Be on Wet Deposition and Transport in a Global Three-Dimensional Chemical Tracer Model Driven by Assimilated Meteorological Fields." Journal of Geophysical Research: Atmospheres 106 (D11) (June 16): 12109–12128. doi:10.1029/2000jd900839; Liu, H., Considine, D., Horowitz, W., Crawford, J., Rodriguez, S., Strahan, M., Damon, S., Steenrod, X., Xu, X., Kouatchou, J., Carouge, C., Yantosca, R. M., (2016). Using beryllium-7 to assess cross-tropopause transport in global models. Atmos. Chem. Phys. 16, 4641-4659, doi:10.5194/acp-16-4641-2016) or to other works from Brattich E. et al. (Brattich, E., Liu, H., Tositti, L., Considine, D. B., and Crawford, J. H.: Processes controlling the seasonal variations in ^{210}Pb and ^{7}Be at the Mt. Cimone WMO-GAW global station, Italy: a model analysis, Atmos. Chem. Phys., 17, 1061–1080, https://doi.org/10.5194/acp-17-1061-2017, 2017; Brattich, E., Liu, H., Zhang, B., Hernández-Ceballos, M. Á., Paatero, J., Sarvan, D., Djurdjevic, V., Tositti, L., and Ajtić, J.: Observation and modeling of high-^{7}Be events in Northern Europe associated with the instability of the Arctic polar vortex in early 2003, Atmos. Chem. Phys. Discuss. [preprint], https://doi.org/10.5194/acp-2020-1121, in review, 2021). If not, better clarifications of the links between the two cited papers from Pacini et al. and from Brattich et al. should be provided in the text.

15) Lines 51-52: I would assume that the knowledge of the wind field together with other meteorological parameters is a requisite for all dynamical atmospheric model, including the one presented here. Considering the temporal extension of current reanalysis (e.g., ERA5 climate reanalysis covering the period from 1950 on, or the NCEP/NCAR reanalysis from 1948 onwards), I cannot see how this actually limits the analysis of transport of beryllium isotopes, whose archives are not that longer.
16) Lines 59-69: This paragraph contains lot of technical data which are not fully pertinent to an Introduction section, while they should be moved to the Methods. Here you should provide a description of how the work presented here fills the gaps that you have just presented in the literature review above.
17) Lines 67-69: Referring to what I suggested at the previous point, I suppose that this should be moved to the Methods. In any case, information on the kind of observed meteorological data used in the model is missing, and should be also provided.
18) Lines 68-69: But why you purposedly chose to compare the model simulations just with measurement of ^{7}Be (and not ^{10}Be) at high-latitude stations? There are a lot of stations measuring cosmogenic isotopes located at midlatitudes or in the tropical/equatorial regions.
19) Lines 84-86: Is this information on the aerosol module connected with beryllium-isotopes? In the real atmosphere, there is a strong connection between 7Be and aerosol size distribution because it is known that after production 7Be rapidly attaches to submicron-sized particles, which is also very important for its removal through wet and dry deposition processes. This information is only partially provided in the text, with a partial description is given at lines 142. Better connection of these mechanisms with the way used in the model to simulate them should be provided.
20) Lines 92-93: There is no version number/year indicated, so there is no way for the reader to understand where the update is, the name is just the same as at line 89.
21) Line 108: Which isotope of beryllium?
22) Lines 109-110: To which time period are you referring to classify the strength of the event?
23) Lines 111-113: I understand that this information is from another recent paper by some of the authors, but I cannot clearly see how the shifting of the SEP event to another date can provide information on the seasonality of beryllium transport, if no information on the different transport or stability condition occurring during these dates is provided.
24) Lines 114-124: Any references for these sentences?
25) Lines 156-160: I am not convinced that the use of a gas deposition scheme instead than an aerosol deposition scheme is correct. Indeed, even though it is true that beryllium-isotopes attach to submicron-sized particles, it is well known that while precipitation scavenging is the dominant removal mechanism for aerosols (especially fine), the same is not totally true for gaseous species.
26) Lines 173-180: Rephrase, not clear.
27) Lines 181-188: Why are you using a multi-year mean? This way you are removing the interannual and seasonal variability. Wouldn’t it better to investigate separately two seasons?

Any other studies showing similar observed/simulated patterns in deposition fluxes of ^{7}Be to compare with?

28) Line 199: What do you mean by “caught by the air dynamics”?
29) Lines 203-208 and 209-212: Here you are describing the event from the point of view of model simulations, but is there any observations for you to document and compare your findings with? In this sense, Figure 5 shows that the ^{7}Be concentration produced by the event were probably not transported at lower atmospheric layers, since probably notwithstanding the strength of the event, there was no transport of stratospheric-upper tropospheric air to the surface (at least in the model), which explains at least partially why the modelled activity of ^{7}Be did not reach elevated values in near ground air in Finland.
30) Line 213: As described previously, this statement is not correct. Indeed, transport depends on many factors which can be seasonally dependent, but definitely depend on other characteristics that are not the seasons. If you do not provide information on the synoptic situation of the period, there is no way to conclude definitely that the transport of e.g., mid-autumn differs from the one of e.g., mid-spring.
31) Lines 213-223: But apart from the description of 7Be vertical cross sections in the different seasons, could you analyse the different transport mechanisms/synoptic situations occurring in the different seasons?
32) Lines 228-229: I understand that data availability is an important issue for any kind of model, but the use of weekly observations, which smooths lots of physical processes dominating the variability of beryllium-isotopes concentrations in the atmosphere poses great limitation to this comparison, which should be at least cited in the text.
33) Lines 228-233: Any references for the description of these measurements?
34) Lines 231-232: Please provide some additional details on the procedure to perform “standard correction for decay”.
35) Line 235: Why do you use a set of stations for Finland while you use just one measuring station for the other locations. Could this then lead to a different result of the comparison? Explain.
36) Lines 228-264: Here the text comprehends also description of the measurement methods, which should be provided in a different (previous) section than this one.
37) Lines 247-248: As reported previously, the weekly information actually smooths the original signal, so I believe it could be important for the authors to show whether the model is able to catch the daily pattern of observations or not.
38) Lines 267-268 and below: The significance level and the value of the correlation coefficient provide information on the temporal coherence between the observed and simulated beryllium patterns, but does not provide information on the presence of a bias between observations and simulations. Below you provide a comparison between overall simulated and observed mean, but again this does not provide a true measure of bias. Please consider the inclusion of additional parameters for the comparison.

39) Line 273: Considering that the paper from Brattich et al. (2020) focuses on a mid-latitude high-altitude station, I doubt that there is any references in this paper with SSW events.
40) Lines 274-276 and below: Could you include a comparison of simulated and observed standard deviations?
41) Lines 277-279: Could you explain better why you suppose that such discrepancies between model and observations relate with atmospheric aerosol properties, especially since you described previously that you applied a gaseous deposition scheme? Couldn't the difference be related with a problem in the meteorological field (wind, precipitation, …)? In any case, what do you mean by "anomalies" in the "atmospheric aerosol properties"?
42) Lines 287-288 and 293: The absence of a seasonal pattern in the observed time series is not a justification of the absence of correlation between measurements and simulations (also, please note that the significance of the correlation, like of any other statistical parameters, is provided by the p-value, and not by the value of the coefficient) by itself, while it suggests that the model does not reproduce correctly the observed time pattern at these two stations, contradicting the statement in the text.
43) You talk about "orography" but how high is the sampling site? Did you compare model topography with real data?
44) Lines 298-299: Based on the reasonings provided above, it seems that the model reproduces correctly the patterns in the northern hemisphere, while the capability is more limited in the southern hemisphere, which may be related with additional factors than the orography and the spatial resolution (which should apply to all stations but in Finland where a set of different stations was used.
45) Figure 8: The figure shows very clearly how the model is able to catch the overall pattern, but is affected by some biases in reproducing some episodes, like for instance: a consistent overestimation for 2008 in Finland; a consistent underestimation of 2007 data in Finland; a period of underestimation in 2008 in Canada; general disagreement of the patterns for Chile and Kerguelen data. All these disagreements are not totally caught by the statistical parameters presented in the discussion, but need thorough investigation and discussion.
46) Figure 9: Also deposition data show similar disagreement not caught by the presented statistical parameters, and are not investigated.
47) Figure 10: The comparison of wavelet coherence between modeled and observed data is not sufficiently explained in the text, therefore the reader cannot properly understand the meaning of the panels.
48) Lines 309-310: You have presented results only for 7Be, so I am wondering you can claim the validity for all cosmogenic beryllium isotopes.
49) Line 315: I cannot see any error bars in the figures about comparison of the model with the observations.
50) Lines 316-317: Again, you talk about orography but there is no description of the model representation of the topography.
51) Lines 317-319: Based on my comments above, this sentence needs thorough revision.

52) Lines 324-325: Again, I suppose that the fact that you are not able to observe this event at near-ground is related more with the absence of transport from the stratosphere-upper troposphere than with the strength of the event.

53) Code and data availability: from the statement it is not clear that the observations used in this work are not freely available. Indeed, the website at STUK present a service price list, which probably means that the reader has to pay if wants to obtain the data, while data from the CTBTO seem to be available upon request (I did not proceed, so I cannot confirm that they are available for free upon request). To me, this seems in contrast with that the Code and data policy of the GMD journal (available at: https://www.geoscientific-model-development.net/policies/code_and_data_policy.html), which explicitly states that the data and other information underpinning the research findings are "findable, accessible, interoperable, and reusable" (FAIR). Regarding the licence of the model, it is not clear whether they are conform to the Open Source Definition. In addition, the document also states that: "Where the authors cannot, for reasons beyond their control, publicly archive part or all of the code and data associated with a paper, they must clearly state the restrictions. They must also provide confidential access to the code and data for the editor and reviewers in order to enable peer review. The arrangements for this access must not compromise the anonymity of the reviewers. All manuscripts which do not make code and data available at this level are to be rejected. Where only part of the code or data is subject to these restrictions, the remaining code and/or data must still be publicly archived. In particular, authors must make every endeavour to publish any code whose development is described in the manuscript. Code and data access must be provided at the time that the discussion paper is submitted. Embargoes, whether pending acceptance or for a defined period, are not acceptable.

And more:

1. the source code for the complete model or module or other coded product described in the paper (must be provided for model description, development and technical, and methods for assessment paper types);
2. the manual and any other model documentation (applies to model description, development and technical, and methods for assessment, to the extent the editor considers applicable);
3. all configuration files, boundary conditions, and input data (must be provided for experiment description papers and any other papers in which results from model runs are reported);
4. data sets for forcing of models or comparison with model output (must be provided for papers describing such data sets or for papers in which model output are compared with such data);
5. preprocessing, run control and postprocessing scripts covering every data processing action for all the results reported in the paper (applies for all papers, to the extent the editor considers applicable)."

So, overall, it seems to me that the statements of this section are not compliant with the requirements from this journal.

Technical corrections

1) Line 11: Change "the measured" with "observations".
2) Line 12: Change "cadence" with "time resolution" and "ones in" with "at".
3) Line 13: Change "hemispheres" to "hemisphere".
4) Line 19: Change "real data" with "observations".
5) Line 32, 37, 127: Change "long-living" with "long-lived".
6) Line 37: Change "probe" to "analyse".
7) Line 44: Replace "Model" with "model".
8) Line 61: I suppose that you did not use all "the available measurements". So I would recommend to change this to "available observations from different stations around the globe in both hemispheres." Possibly also add the time period of the measurements.
9) Line 63: Delete one "the".
10) Lines 105-108: I would suggest rephrasing: "While GCR are always present near the Earth, sporadic solar particle events (SEPs), which can be sufficiently strong to … and to produce a large amount of cosmogenic isotopes (…), take place occasionally (…)"
11) Line 109: Change "studied" to "study". Add "one" after "strongest".
12) Line 110: Here and throughout the manuscript: check the acronym, is it SEP or SPE?
13) Figure 1, caption: Change "model's" to "model". The units of the color scale is the same for both panels so there is no need to repeat the information for both panels and could be provided just in the general description of the figure before or after the description of the two panels.
14) Line 132: Change "the" to "a".
15) Line 158: Change "utilize" to "utilizes".
16) Line 170: Change "Boreal" to "boreal".
17) Line 172: Change "hemispheres" to "hemisphere".
18) Figure 3, caption: You should describe also the vertical information which is provided in the Figure.
19) Line 209: Add "one" before "strongest".
20) Figure 6: Explain the unit in the caption.
21) Figure 6, caption: The caption is probably not describing the figure correctly, since if the x-axis depicts days from 0 to 120 in 2005, it is not possible that the plot reports just the modelled activity of 20-Jan-2005 as reported in the caption.
22) Figure 7: Explain the unit in the caption and use a consistent unit for atoms (either "atom" either "at").
23) Line 247: Change "cadence" to "time resolution":
24) Line 258: Change "CTBT" to "CTBTO".
25) Line 259: The information provided between parenthesis is redundant since it is already contained in the fact that you use "total-deposition". Please delete.
26) Line 289: Change "suggests" with "suggesting".
27) Invert Figure 9 with Figure 10, as deposition is the last commented.

28) Lines 337-340: Revise use of tense in this section (all the same).
29) Line 338: Delete one “the” and change “them” to “it”.
30) Line 339: Delete “real”.
31) Lines 339-340: A verb is missing in this sentence.