**Review on:**

**Simulations of 7Be and 10Be with the GEOS-Chem global model v14.0.2 using state-of-the-art production rates by Zheng et al. (2023) submitted to GMD**

***Summary:***

*Zheng et al. are presenting an update of the GEOS-Chem global model, that aims for a more realistic representation of 7Be and 10Be production rates and with that a more realistic representation of the radionuclide transport and deposition. By comparing simulations with fixed production rates (represented by the year 1958 i.e., the year of the strongest solar maximum throughout the instrumental era) and experiments including time-varying solar modulation in the production calculation to observations, Zheng et al. clearly show the importance of the solar variability when trying to understand the measured near surface radionuclide content. According to their analysis, this is especially relevant for the middle and high latitudes.*

*The manuscript is generally very well structured, clear and easy to follow. The main statements are supported by the numerical experiments and the results are depicted by the aid of appealing figures.*

*I just have some minor comments and a few open technical questions (especially with respect to the aerosol model and the dry deposition and wet deposition ratios (please see below)) that should be addressed in a revised version of the manuscript. Afterwards, I recommend publication in GMD.*

***Minor comments:***

**L17: are useful aerosol tracers –** *The radionuclides 7Be and 10Be are actually not aerosols. They get attached to aerosols quickly and are then transported and deposited as such. The authors explain this later and I would remove "aerosol" here.*

**L48: removed by the wet deposition.** – *Not only wet deposition. Dry deposition, and sedimentation (see comment below) are also relevant deposition processes. Maybe "removed by different deposition processes" is more appropriate here.*

**L50: atmospheric atoms** – *Mostly oxygen and nitrogen, I guess. Please specify.*

**L69: and vertical transport** – *What about horizontal transport e.g., by storm-tracks in the middle latitudes?*

**L78-79: In comparison to other atmospheric models (e.g., Golubenko et al., 2021; Heikkilä et al., 2008b)** – *Spiegl et al. (2022) are using another approach based on EMAC and WASAVIES. I guess this paper deserves to be mentioned like the Sukhodolov et al. (2017).*

**L94: Previously –** *Maybe "In earlier studies".*

**L116-117: with a detailed description of stratospheric and tropospheric chemistry –** *Is a detailed chemistry code really relevant for the presented experiments? I guess the considered radionuclides may not be part of the chemistry scheme. If so, could the authors please comment on that?*

**L123-124: 7Be and 10Be are carried by ambient submicron aerosols after production and are removed by dry and wet deposition processes (Liu et al., 2001) -** *I think some more details, especially on the aerosol submodel, are essential here. Is the process of attachment explicitly modeled or did you treat the radionuclide loading physically like an aerosol directly? What distributions of aerosols did you use for the different domains (stratosphere vs. troposphere) with respect to diameter size and*

*distribution? Are volcanic effects included as well or is the size distribution constant over time? How is re-evaporation of aerosols treated in the model? I guess a profile (maybe to be included to the supplement) showing the considered mean diameter size as a function of height would be very helpful here. Also, is the process of sedimentation included in the experiments? While this process might be negligible for a small diameter size (stratospheric aerosol) it could become relevant for much bigger tropospheric aerosols. Could the authors please comment on this?*

**L127-128: Precipitation formation and evaporation fields from reanalysis data are used directly by the model wet deposition scheme. –** *If I understand correctly, the experiments are driven using a specified dynamics approach concerning 3d temperature and wind structure as well as 2d precipitation. What about the underlying SSTs. Could the authors comment on that?*

**L139: "stars" –** *Please explain more here for the readership that is not familiar with this terminology.*

**L140: multiplied with the mean production yield of 0.045 –** *Where does this number come from? Please specify.*

**L147-148: The P16 production model is regarded as the latest and most accurate production model for 7Be and 10Be –** *I believe that the approach presented here delivers accurate results. However, I do also think that alternative approaches could also deliver results that are just as convincing. Please rephrase to "one of the most accurate".*

**L180: 2012 to 2018 –** *Why was exactly this period chosen? It covers half of the solar cycle 24 and stops before the minimum of solar cycle 25. This is difficult to understand. Please explain.*

**L180: four-year spinup (2008-2011) –** *Please give some more details on the spinup period. Why was it necessary and how was it modelled (e.g., with respect to boundary conditions).*

**L186-187: also conducted for the year 2012 –** *Why "also"? It's the only experiment that has been conducted solely for the year 2012 in my understanding. Please clarify.*

**L188: done on a –** *"conducted using a…"*

**L193: abnormal value** – *"outliers"*

**L195-196: Usually, if the scatter plot of the model and measurements is within a factor of 2 of observations, we consider the model with reasonably good performance. –** *Please specify why you used this value (2) as a benchmark. Does it mean if the model results is 2 times larger than the observations you would consider it as reasonably good performance?*

**L200: for surface air 7Be concentrations** – *Please explain how the near surface air concentrations from the model integrations have been calculated before comparing them to observations. Did the authors use the closest box to the surface? I guess this information would be necessary.*

**Results:**

***General comment:*** *From here on a different font has been used and the figures and the manuscript becomes rather blurry. Is there a reason for this?*

***L231-238:*** *If I understand correctly, the main differences between the LP67 and P16 models are a result of the different state of the Sun (1958=solar max vs. 2012=between solar min and max), while the general physics of both models are rather similar. Is this correct? Can you explain why you used the year 2012 as a reference? Did you like to capture the mean state of the Sun? Please leave a comment in the text.*

***Figure2:*** *The figure appears to be blurry. The axis and colorbarn labels should be bigger.*

**L277-279: … to capture local weather conditions –** *I agree especially e.g., the deposition fluxes over Greenland and Antarctica could be highly influenced by the model resolution used to capture the complex terrain (see e.g., Spiegl et al. (2022)). I think a comparison to individual stations at very unique locations is only possible using regional climate models. Also, the process of tropopause foldings could be influenced by the rather course resolution here.*

**Figure 3:** *What is actually shown here? Are these all the station data plotted against the model data? If so, did you use the closest grid box to a station for campariosn? Is this the annual mean surface air concentrations?*

**Figure 4 and 6:** *Axis labels are too small to read.*

**Section 3.2 and 3.3:** The authors nicely compare the model fluxes to the observations here and I don't have any comments. I just have one wish! Would the authors please be so kind and provide some map plots (maybe in the SI) that show the individual contributions of wet and dry deposition and (if applicable) sedimentation. I know from my own experience that getting a "correct" pattern is not an easy task. While the total fluxes may agree between different models, the individual contributions of different deposition processes (wet, dry and sedi) can be very different. E.g., compare Field et al. (2006), Heikkilä et al. (2013) and Spiegl et al. (2022). I do think the differences in the pattern are a result of different aerosol models and thus tracer treatment. This is why I would like to ask the authors to provide more details on this (see above). Seasonal map plots would be perfect, annual-mean sufficient. Thanks!

**L376: increasing trend** – *What is meant by increasing trend here?*

**Figure 7:** *This figure is blurry again and the axis and legend labels are too small to read. Please provide more information the caption on the data. Is it annual mean? Which period?*

**Page 14:** *Clear text again! From 15 on again blurry and different font.*

**L421: 2012-2018 while the measurements are based on the data availability during 2001-2015.** – *I think this is OK, since you only like to compare the general pattern.*

**Figure 8 and continued:** *This is a nice figure, but it needs some revision. The axis labels are to small, the grey text is difficult to read as well as the legend. And its blurry again.*

**On the "modelled stratospheric contribution"** – *Would the authors please specify how this was computed.*

**Figure 9:** *The grey text is difficult to notice.*

**L481: GIRP** – *GRIP*

**L481-488:** *I agree that the seasonal cycle is not well reproduced by the model with respect to polar latitudes and to some degree this could be related to the modelled period. However, I do think that the model resolution is just not high enough. Please see my comment on regional models above. Maybe this could be mentioned as a future perspective?*