Review of the manuscript "High-resolution stratospheric volcanic SO2 injections in WACCM", Axebrink et al

Dear Editor, dear Authors,

The manuscript "High-resolution stratospheric volcanic SO2 injections in WACCM" by Axebrink et al discusses large-scale modelling of the Sarychev eruption in 2009, with the aim of investigating the importance of input injection parameters and other model set-up aspects in the description of stratospheric volcanic eruptions, towards the estimation of radiative impacts of such events. The topic of the manuscript is important and of certain interest for the ACP readers. There is an active ongoing scientific debate, at the international scale, about how to represent volcanic plumes (stratospheric but also tropospheric), and their impacts, with numerical modelling. The consistency of confined plumes (volcanic emissions, wildfires, etc) observations and modelling is still to be achieved, to be honest. For this reason, this manuscript has the potential to be an important contribution to this debate. Unfortunately, I have fundamental concerns about the model set-up and cannot recommend this manuscript for publication as it is, see Specific Comments 20 and 23-29. Based on these comments, I rather recommend clarification or re-design of the experiments before I can fully evaluate this manuscript. For this reason, for the moment, I have not evaluated the Results section, and I'm waiting for such clarifications before going further in this review. In addition, I have found the manuscript severely lacking in text quality (i.e. different statements without justification) and the literature citation (knowledge?) is also to be strongly improved, see Specific Comments 1-22.

Please find Specific Comments in the following. Please address all these comments and I will be happy to review a further manuscript version, if the Authors decide to resubmit it to ACP.

I am sorry if I cannot be more positive this time but I strongly encourage the Authors to address my comments, improve the manuscript, re-design and re-run the simulations if needed, and then resubmit a new manuscript version.

Regards.

We thank the reviewer for the valuable comments that helped us improve our manuscript. We have improved the description of the model simulations to make the model set-up clearer. We have also provided a more thorough explanation for our choices in the set-up of the model simulations and the implementations of the different datasets. See more details regarding this under our answers to Specific Comments 20 and 23-29. We have also improved the text quality in response to the reviewers' comments. For more detail see answers to Specific Comments 1-22.

Please see the answers to the reviewer's comments below.

Specific Comments:

1) L22-23: why a full stop between the two sentences?

We have changed this according to the reviewer's suggestion.

2) L23-24: "Aerosol emissions...greenhouse gases", please add one or more references for this statement.

We added a reference to Hansen et al. (2023).

3) L23: "These effects result in a net cooling...", not always! See the case of black carbon aerosols, e.g.: https://www.nature.com/articles/s41467-020-20482-9 or https://acp.copernicus.org/articles/22/9299/2022/ and others

Yes, in specific settings the aerosol effect can be warming or cooling, but this is not of relevance in our study of the global climate effect of volcanic SO2 injection to the stratosphere. We have no wish to go into details on wildfire emissions in this manuscript, which is covered in our previous work, e.g. Martinsson et al., 2022 and Friberg et al., 2023.

4) L25: "...natural sources", why only natural aerosol sources? What about anthropogenic aerosol sources?

We agree that both natural and anthropogenic sources are of importance. Here we decided to mention only the natural, due to the topic of the paper (volcanic SO2 injections to the stratosphere).

5) L28-29: Please rephrase: the SO2 emissions do not have a direct impact on the radiative balance, the subsequently formed sulphate aerosols have (it is said right after).

Yes, in its molecular form SO2 has very limited impact on the climate. Thank you for noticing this mistake in the phrasing. We have modified the sentence to clarify that we refer to climate effects of volcanic eruptions.

6) L29: the SO2 actually converts to binary solution droplets of sulphuric acid + water (e.g. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015RG000511). Maybe this sentence can be rephrased accordingly.

Yes, it does form sulfuric acid which attracts water and form particulate matter. We have deleted the word ‘aerosol’ in the sentence.

7) L30: "...which can create years of...", please mention the very long lifetime of sulphate aerosols in the stratosphere

We agree that this should be mentioned. We have made changes according to the reviewer's suggestion.

8) L33: "explosiveness" --> "explosivity"

The reviewer is correct – explosivity is the correct word. We have changed accordingly.

9) L33: "the SO2 mass" --> "the mass of the injected SO2"

We have changed according to the reviewer's suggestion.

10) L35: please state clearly that the cooling of the Earth's climate system from volcanic eruption is *transient*

This information is now stated as: *"particulate matter, which can remain in the stratosphere for months or years inducing long-term negative radiative forcing by scattering incoming solar radiation"*.

11) L39-40: we are not at all in a background stratospheric aerosol condition, and the radiative effects of moderate stratospheric eruptions extends well beyond the "beginning of the 2000s": please mention, at least, more recent eruptions such as Raikoke 2019 (e.g. https://acp.copernicus.org/articles/21/535/2021/) and the very special case of the Hunga eruption 2022 (https://www.nature.com/articles/s43247-022-00618-z)

We agree – the present state is not a volcanically quiescent period. We decided to remove the sentence.

Our manuscript is focused on the June 2009 Sarychev eruptions. Other volcanic eruptions are covered in our previous work (e.g. Andersson et al., 2015, Friberg et al., 2018). We find post-Sarychev volcanic eruptions to be less relevant for our study, including Merapi, Nabro, Kelut, Calbuco, Ulawun, Manam, Ambae, Raikoke, Hunga Tonga-Hunga Ha'apai, etc.

12) L44-46: This sentence sounds like a repetition of what already said before and can be suppressed.

We wish to keep the sentence.

13) L44: "SPA, 2006" is rather "SPARC, 2006"? This looks like too generic as a reference and the Authors can easily find more specific references

We agree and now cite Deshler et al. (2008) and Robock et al. (2000) instead.

14) Please mention representative cases, like this one (plus others, in case): https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021JD035974

We do not understand in which sentence or paragraph the reviewer wishes us to add this reference.

15) L48-49: please briefly state how satellite observations are used by modelers. Synergies studies can be cited, e.g.: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021JD035974 and https://acp.copernicus.org/articles/16/6841/2016/ and others

We understand that our sentence was misleading and changed it to *"Global modelers often use satellite-based observations of volcanic SO2 as input when simulating the volcanic impact on the stratosphere and climate".*

16) L49-50: "Most SO2..." "most" or "all"? How SO2 can be measured with active observations? Also, the question of the vertical resolution of satellite observations (especially in a nadir geometry) is complicated and should be briefly discussed here (e.g.: passive sensors do not lack vertical measurements but rather have limited vertical sensitivity, etc)

Satellite based SO2 sensors retrieve vertical information indirectly. We changed the sentence in line with the reviewer's suggestion.

17) L51-52: "*Clarisse et al. (2014) showed that IASI can provide SO2 data with vertical resolution down to ~2 km,*", this sounds a bit overestimated for infrared observations at the nadir, please check

This is the resolution that Clarisse et al. (2014) present in their paper. Please see the abstract of their paper.

18) L58: "column" --> "total vertical column"

The standard approach is to run ESMs with altitude resolved SO2 data as input, and not with single values of the height integrated SO2 mass (as the term *"total vertical columns"* suggest). The SO2 profile within the (vertical) column may however, in some simulations,

consist of uniform distributions, as well as for example triangular or Gaussian distributions.

As this sentence seem to have caused some confusion, we have rephrased it and the sentence now reads: *"ESM simulations of explosive volcanic eruptions' climate impact are generally run with vertical SO_2 profiles released above, or in the vicinity of, the volcano site (Timmreck et al. 2018)."*

19) L65: "*implemented*" --> the Authors mean "used as input"? See also L83

We have changed according to the reviewer's suggestion. We have removed the word implemented from the entire manuscript.

20) L73-76: I honestly did not understand the difference between second and third data sets. Please clarify.

The third dataset has the same vertical profile as the second dataset but is released in single column (one lat x lon grid box only) rather than over several latitude and longitude grid-boxes. This is further explained in answers to the comments below.

21) L79-81: this three-lines introduction can be suppressed as it is redundant

We wish to keep this as is.

22) L86: SO2 (AIRS) and aerosol (CALIOP) vertical profiles do not "have the same height profile" but the Authors assume this is the case, which implies the fact that the Authors assume that the SO2 and aerosol plumes are collocated. This has to be stated and the chemical/microphysical implications of this assumption should also be briefly mentioned.

Our sentence on L85-86 (*"The SO2 and aerosol observed from these instruments were assumed to be co-located and therefore have the same height profile"*) refers to the co-location of SO2 and aerosol, i.e. the assumption used to produce the high vertical resolution SO2 data in Sandvik et al. (2021). CALIOP's role in Sandvik et al. 2021, was to provide vertical information for the SO2 AIRS data.

The present manuscript, Axebrink et al. (2024), focuses on the simulations with data produced in Sandvik et al. (2021). Please see Sandvik et al. (2021) for further description on how SO2 data were compiled, i.e. explicitly how CALIOP and AIRS data were combined using FLEXPART.

23) L124: "*M16 is a single column (1D) emission dataset with a vertical resolution of 1 km.*" How can a "single column emission dataset" have a "vertical resolution of 1 km" <-- this means that the emissions are not based on a single column but on different vertical layers (at 1 km resolution).

With this sentence we would like to point out that the M16 dataset is released in one lat x lon grid box rather than in several lat x lon grid boxes around the volcano site. A column consists of different vertical layers that has a vertical resolution of 1 km.

The terminology 'single column' is commonly used within global modelling of volcanic eruptions e.g. Tilmes et al, 2023 which was recommended to us by reviewer #1.

24) L125: why M16's and S21-1D emissions are released at a different time interval (15-16/6) than S21-3D (19/6)? If it is now known that Sarychev emissions were mainly on 19/6, why making simulations of the "wrong" days?

The Sarychev eruptions occurred mainly on the 15th and 16th of June. We have used the M16 dataset in WACCM in the same manner as in previous studies of this eruption with this model. The S21-3D dataset was released on the 19th June since the satellite dataset was recorded at that specific date. This is explained in Section 3.1.

25) L125-126: how SO2 is released during the 6-hours period? Is it a constant emission rate? Is there a peak at some time? Why only 12:00 to 18:00 for the two days and not before/after? This sound as an unphysical way of "erupting" for a volcano and should be fixed.

This is a standard approach for the WACCM modelling of Sarychev's eruption and in line with the methods in Mills et al., (2016). We use the same approach as previous studies to investigate the differences between the standard approach and our approach (different dataset). The emission rate is constant during these times.

26) L130-131: how SO2 is released here as well (cfr previous comment)? Same for the third dataset (L133-135)

The SO2 emission rate is constant during these hours.

27) L132-133: this is very puzzling. Do the Authors mixed-up the vertical and horizontal definition of "single column"? What's the actual meaning of "1D" here?

With "1D" we mean that the dataset is not spread over several latitude and longitude grid-boxes and is therefore not a 3D dataset. The single column dataset only has the vertical dimension and is therefore one dimensional. Se comment 23 with regards to the use of the term single column.

28) L151-152: using CALIOP data as a comparison data set is not completely satisfactory in terms of independence with the simulations, as one the simulation was partially initialised with CALIOP information (S21-3D)

We disagree. When compiling the high-resolution SO2 dataset, CALIOP data is only used to tell the vertical positions of the SO2 layers. No CALIOP aerosol data is used as an input to the model. It is of interest to investigate how well the model can simulate the transformation of SO2 into aerosol, the removal and transport of the aerosol in the model and the resulting AOD distributions over time. The high-resolution aerosol dataset from CALIOP is highly suitable to evaluate the latter. CALIOP's vertical resolution is more than a magnitude higher than that of other satellite borne stratospheric aerosol sensors.

29) Fig. 1 caption: In panel b, this is a daily average or at a specific hour? In panels c and d are also for S21-3D simulation? Please mention this in the caption.

This is the summed total emission. We have updated the figure caption to better describe this and to explain that figures c and d display the S21-3D dataset. The figure caption now reads: "(a) Vertical SO2 profiles for the three input datasets of each simulation. The vertical profile for M16 and S21-1D is the summed total injection for the eruption on the 15th and the 16th of June, whereas the vertical profile for S21-3D is the total injection on the 19th of June. (b) Vertically integrated total amount of SO2 for the S21-3D dataset. The red triangle marks the location of the volcano Sarychev Peak. (c) latitudinally integrated total amount of SO2 for the S21-3D input dataset. (d) longitudinally integrated total amount of SO2 for the S21-3D input dataset."

References:

Tilmes, S., Mills, M. J., Zhu, Y., Bardeen, C. G., Vitt, F., Yu, P., Fillmore, D., Liu, X., Toon, B., and Deshler, T.: Description and performance of a sectional aerosol microphysical model in the Community Earth System Model (CESM2), Geosci. Model Dev., 16, 6087–6125, https://doi.org/10.5194/gmd-16-6087-2023, 2023.