

We thank the reviewers for their helpful comments. In the following, reviewer comments are in black, our author responses in blue.

Reviewer 1:

General

The manuscript describes an algorithm for calculating temporal, latitudinal, vertical and wavelength dependent distribution of sulfate aerosols for the use in comprehensive Earth System Models. The manuscript is very well structured, organized and written. The main part of the manuscript describes in a formal way the physics behind the EVA algorithm which is important for groups that would like to apply the algorithm for their individual models. However, for the general readership the manuscript would benefit including a number of paragraphs introducing general physical concepts in the context of stratospheric volcanic aerosols that are referred to in the body of the manuscript.

I suggest final publication of the manuscript when the minor points below are addressed in the revised version.

Specific

Introduction

The introduction lacks two or three additional paragraphs for assisting the reader that is not a specialist to get a basic overview about the physical mechanisms involved in the context of stratospheric sulfate. This relates to a general introduction into the basic concepts of the gas-particle conversion in the stratosphere and the general effect of the volcanic sulfate aerosols on the modification of incoming solar radiation (e.g. related to EXT, SSA and ASY).

We agree, and have added background paragraphs in Sec 2.

A second point relates to the availability of volcanic reconstructions carried out in former studies as introduced in chapter #2 (e.g. Crowley and Unterman,2013; Gao et al. (2008)) and uncertainties involved. This could also be used to address different sources and amounts of uncertainties involved in volcanic reconstructions. The authors state in one of their following chapters on the uncertainties involved in their reconstruction approach based on specific assumptions, for instance based on the Pinatubo eruption. However, taken into account the large uncertainties involved in the reconstruction of stratospheric aerosol loadings based on ice core data and according dating uncertainties this might put some of those EVA-specific uncertainties into perspective.

We appreciate the comment. We do not want to include a lengthy discussion of the different uncertainties involved in ice core reconstructions, since this work focuses specifically on the methods of producing optical properties from given SO₂ injection amounts. Nonetheless, we agree that the uncertainty in proxy records is important, and supports our justifications for the construction of a simple forcing (e.g., compared to the use of coupled aerosol models). We include now a mention of ice cores in the Introduction, and have added the following sentence to the Conclusions:

“For most purposes, inaccuracies in forcing due to the simple approach of EVA are likely small compared to uncertainties in our knowledge of the properties of past volcanic eruptions inferred from proxies like ice cores. “

Existing data set and approaches

There is one earlier volcanic reconstruction based on Crowley (2000) which was used as forcing by a number of climate simulations. The discussion of the reconstruction of the volcanic forcing already includes most of the mechanisms implemented for the later reconstruction of Crowley and Unterman 2013. Maybe the authors can add this data set and the references as additional source for volcanic reconstructions.

We now make reference to the Crowley (2000) reconstruction within the Crowley and Unterman (2013) subsection, since the two are very similar.

The EVA approach

p. 9, l. 19: The authors mention several times the Brewer-Dobson circulation and its significance for the aerosol distribution and transport – A small paragraph in the introduction or within this chapter would help to understand the overall significance and mechanisms controlling the BD circulation.

Yes, good point. This has been added with the radiation background material at the beginning of Sec. 2.

A second general remark relates to the importance of the specific synoptic meteorological conditions for the individual outbreaks including the timing in the annual cycle (cf. p. 9 27 ff.) – A few words on how this could impact on the eventual depositing of the sulfate in Greenland and Antarctic ice cores could help to motivate the high degree of uncertainty involved in the reconstruction based on ice cores.

Indeed, the importance of specific synoptic meteorological conditions on the hemispheric asymmetry of the aerosol distribution is discussed and dealt with in Sec 3.6. We have put a pointer to this issue in the description of the seasonal cycle of mixing and transport, and explicitly mentioned synoptic meteorology in Sec 3.6.

Sample Results

p. 15 l. 17: Is there a way to improve the accuracy of small-type extratropical high latitudes eruptions, knowing that their impact in EVA might be too exaggerated?

We have considered this, but to implement variations in loss timescales depending on altitude of injection represents a large increase in complexity of the EVA code. Also, there are presently insufficient observations to constrain how aerosol lifetime depends on eruption magnitude. Perhaps model results can help in this regard, but we prefer to address this in future work.

We have slightly expanded the explanation of the source of the overestimation for extratropical weak eruptions, and note that there are few observations on which to base a relationship of lifetime versus eruption magnitude.

p. 16 l. 3ff: Is there an explanation why the linear scaling used in EVA for Tambora produces lower AOD values than the non-linear scaling in CU13 ?

The reason comes back to the different linear scalings, discussed in Sec 3.7:

“The linear scaling used in EVA is significantly less steep than that of CU13, a result of the lower peak global mean AOD estimate for Pinatubo from CCMI compared to Sato/GISS, and the larger estimate of SO₂ injection from satellite sensors compared to the ice core-derived estimate of CU13.”

As a result, the linear relationship for EVA lies “below” the non-linear curve from CU13, up to SO₂ injections a little larger than Tambora, as shown graphically in Fig 13.

Conclusion and outlook

The EVA algorithm will most likely have also an important role in the PMIP4 simulations and the VolMIP project. A concluding paragraph on the linkages with PMIP4 could also serve as an outlook for applications of EVA in producing consistent volcanic data sets for paleoclimatic modelling of the Holocene and the last 2,000 years.

We have added references to PMIP and VolMIP in the concluding paragraph.

Reviewer 2 (Allegra LeGrande)

The EVA product is a much needed addition to doing long-term simulations that include volcanoes. It is an intermediary between having a full-scale (and computationally expensive) aerosol microphysics module and a highly parameterized sulfate aerosol forcing – or worse, alterations in solar constant or similar as a stand-in for actual volcanoes.

This paper is well-written and well-thought out. My main suggestion is how the considerable uncertainty in the measurements of volcanic aod/reff etc. influences the principal results of the EVA product.

Also, N.B., I am not the best person to comment on the radiation tables and methods applied, etc. – I leave it to other reviewers to provide commentary as necessary.

There are quite a few acronyms. It would be useful to have a small table / glossary.

[A table of acronyms has been added in Appendix A.](#)

Abstract

The abstract could be changed a small bit to make this more than a summary of the methods: A sentence or two at the top for motivation might illustrate why this is such a useful tool (as in the first paragraph of the intro). A sentence at the end to say its results fall within the spread of sophisticated modules (i.e., as in Zanchettin 2016) would emphasize this.

We have added a short motivational intro to the abstract as suggested. We have added mention of the result of Zanchettin et al. (2016) in the Conclusions and Outlook: since it is not a result of the present paper, we feel it belongs more naturally there than in the abstract.

Intro

I think maybe a short summary of how models approximate volcanoes would be helpful. (And show how this is many-steps better than the altered solar-constant variety of ‘volcano’.)

We have added a short paragraph introducing some of the ways volcanic forcing has historically been implemented in climate models, within Sec. 2.

Line 20: the volmip paper is Zanchettin et al 2016, is this what you mean?

We originally referred to the Zanchettin et al. (2015) PAGES magazine article, specifically to Fig. 1, but since the same material is included in the 2016 GMD paper, we have now changed this reference as suggested.

Section 3.1

18 Tg for Pinatubo SO₂ is totally reasonable – but some use estimates as low as 14 Tg. Can you acknowledge this uncertainty here and address later on.

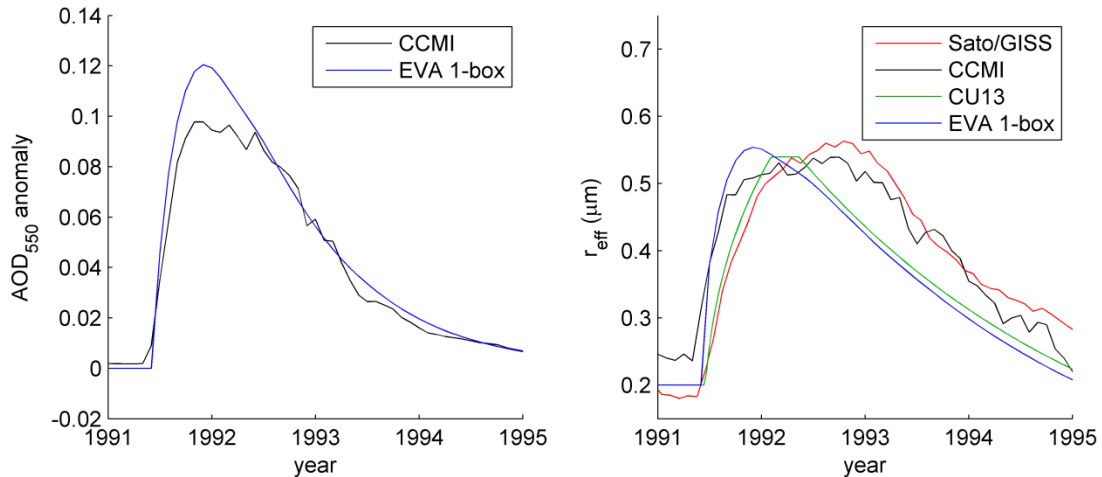
We have added the uncertainty for the Guo et al. (2004) SO₂ injection estimate (i.e., 18 ± 4 Tg SO₂), and noted the corresponding uncertainty in the parameter A later in Sec 3.1.

Depending on when you downloaded the Sato / GISS strataer files, the Reff and AOD may not include the most recent updates to the SAGE II estimates. Probably it is best to ask Makiko directly about when the update was of the data you downloaded. It is important because, for instance, the shape of 91Pinatubo AOD, for instance, is considerably different in [Vernier et al., 2011]. The shape looks more like an exponential increase with slow decay – reaching max AOD much more quickly than in the previous SAGE II interpretations (eg Sato93). This SAGE II does feed into Makiko’s updates.

After direct contact with Dr. Sato, we are confident that the Sato et al. (1993, with updates) we have downloaded and used is, for the Pinatubo period, consistent with early SAGE retrievals, and has not been subsequently updated with newer SAGE retrievals. It appears that Dr. Sato has used updated Pinatubo-period forcing data (provided by J.P. Vernier) in recent studies, but the “official” data set, downloadable from the NASA/GISS website has remained static since 2012.

The Vernier group redid its Reff estimates, too.

The SAGE_4λ (or CCMI) dataset contains estimates of the mean aerosol radius (r_{mean}). Under the assumption of a single-mode, log-normal distribution, we can translate r_{mean} into r_{eff} (see below). This estimate is quite similar to the older Sato dataset. Nonetheless, we will include the CCMI-based r_{eff} estimate in an updated Fig 1.



It is likely that the Sato method or CU13 method is capable of getting r_{eff} for eruptions larger than Pinatubo ($>0.15\text{aod}$), and this uncertainty should be explored here as well.

It is important that there are no observations, or even indirect evidence of the size distributions of aerosol from eruptions larger than Pinatubo. Hence, there is considerable uncertainty in the effective radius for such eruptions. We have added a few sentences to make this clear, in Sec. 4.2 when discussing the EVA-produced r_{eff} for Tambora.

Perhaps taking r_{eff} from some previous full aerosol microphysics simulations? (Which I know has its own problematic spread).

We plan to compare the EVA results with interactive aerosol model results in future work, but at the moment, the models show a very large spread and don't really help to reduce uncertainty. We include now a reference to Stoffel et al., (2015) who report the maximum r_{eff} produced by their aerosol model in simulations of Tambora.

Hemispheric Asymmetry

For the 91Pinatubo case – is the Cerro Hudson contribution considered? (Maybe it helped give extra aod to the SH).

Estimates of SO_2 injection by the Aug 1991 eruption of Cerro Hudson are around 1.5 Tg SO_2 (Doiron et al., 1991), i.e., about $1/10^{\text{th}}$ that of Pinatubo. Inspection of the SAGE/CCMI data shows clearly that due to its lower stratospheric injection height, the aerosol cloud from Cerro Hudson was short-lived, and its contribution to the maximum post-Pinatubo SH AOD, in around December 1991, was likely negligible. We have added a sentence to be clear that we do not believe Cerro Hudson had a large impact on the satellite AOD record, i.e., that Pinatubo aerosol really was evenly distributed to the two hemispheres.

Section 3.6

This uncertainty for large eruptions is important and needs to have more mention up top. It is not clear that a single threshold is the appropriate way to implement the $2/3$ power. Can there be a continuous function.

We agree that from a theoretical perspective, a single function would be satisfying. Obviously, both processes (nucleation of new particles and condensation onto pre-existing particles) can happen at the same time, and the best parameterization would include a smooth variation of the proportion of one process or the other. We hope to investigate this in future research. However, the threshold based approach does have the advantage of being conceptually straight-forward, and is consistent with prior work. Also, since the threshold-based function is smooth and continuous, it seems likely that a more complicated parameterization would likely be practically very similar in form. Therefore, we maintain the use of the threshold-based function, until it can be shown to be deficient.

We have edited and expanded the discussion of the linear-nonlinear threshold approach in Sec 3.7 to reflect some of these issues, including the following text:

“The present scheme retains consistency with the reconstruction of CU13, and has the advantage of simplicity, at least for the majority of eruptions for which AOD is a simple linear scaling of sulfate aerosol mass. Scaling considerations for extremely large eruptions should be understood to be a major source of uncertainty in any volcanic forcing reconstruction.”

Section 4

I am surprised that the [Carn et al., 2016] paper isn't used in here. It is new, but it is comprehensive, and has emerged as one of the principal papers used for volcanic SO_2 from measurement.

We used the Brühl et al (2015) estimates for the most recent eruptions, since these are based on vertically-resolved MIPAS SO_2 measurements, and therefore more accurately estimate the SO_2 injection above the tropopause than the OMI-based, vertically integrated estimates of Carn et al. (2016). However, we agree that the Carn et al. (2016) data set is very comprehensive, and have added a pointer to it in the conclusions, regarding the potential use of EVA with different SO_2 injection data sets.

Carn, S. A., L. Clarisse, and A. J. Prata (2016), Multi-decadal satellite measurements of global volcanic degassing, *Journal of Volcanology and Geothermal Research*, 311, 99-134, doi:10.1016/j.jvolgeores.2016.01.002.

Vernier, J. P., L. W. Thomason, J. P. Pommereau, A. Bourassa, J. Pelon, A. Garnier, et al. (2011), Major influence of tropical volcanic eruptions on the stratospheric aerosol layer during the last decade, *Geophysical Research Letters*, 38(12), n/an/a, doi:10.1029/2011gl047563.

Stoffel, M., Khodri, M., Corona, C., Guillet, S., Poulain, V., Bekki, S., Guiot, J., Luckman, B. H., Oppenheimer, C., Lebas, N., Beniston, M. and Masson-Delmotte, V.: Estimates of volcanic-induced cooling in the Northern Hemisphere over the past 1,500 years, *Nat. Geosci.*, advance on, doi:10.1038/ngeo2526, 2015.

Doiron, S. D., Bluth, G. J. S., Schneltzer, C. C., Krueger, A. J. and Walter, L. S.: Transport of the Cerro Hudson SO₂ clouds, *Eos, Trans. Am. Geophys. Union*, 72(45), 489–489, doi:10.1029/90EO00354, 1991.

Zanchettin, D., Timmreck, C., Khodri, M., Robock, A., Rubino, A., Schmidt, A. and Toohey, M.: A coordinated modeling assessment of the climate response to volcanic forcing, *Past Glob. Chang. Mag.*, 23, 54–55, 2015.