

Interactive comment on “Size-Resolved Stratospheric Aerosol Distributions after Pinatubo Derived from a Coupled Aerosol-Chemistry-Climate Model” by Timofei Sukhodolov et al.

M. Toohey (Referee)

mtoohey@geomar.de

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General comments

This paper describes a series of experiments of stratospheric aerosol following the 1991 Pinatubo eruption with the SOCOL-AER model. The model results are compared extensively with available observations, and the paper serves therefore as a valuable model validation exercise, showing that in general the model reproduces many of the observed aerosol properties rather well. Some differences between model and obser-

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...vations are noted, and sensitivity experiments are used to gauge the importance of a few uncertainties related to the eruption itself and structural uncertainties in model parameterizations.

The results are generally quite impressive, however, for a paper in GMD, there is very little description of the model included here. There is reference given to Sheng et al. 2015, and probably one can find more description there, but some more details should be included in this paper. For instance, in the Results it is mentioned that ECHAM5 is the core GCM of SOCOL-AER—this should be included in the model description section. Also, the model description should include some text relevant to each of the processes that are altered in the sensitivity studies, e.g., the standard sedimentation and coagulation parameterizations used in SOCOL-AER should be described. More details on the coupling between aerosol and radiation would be quite useful, e.g., exactly what optical properties are required by the radiation code, and what assumptions and simplifications go into the Mie theory calculations (refractive indices, etc.).

In the conclusions, it is stated that the “main modelling deficiency found” is the 1-2 K larger lower stratospheric warming compared to reanalyses. This distinction is rather subjective, and I’m surprised there wasn’t also mention of the fact the model results show differences compared to observations in aerosol number density of orders of magnitude in the 25-30 km range.

Specific comments

P1, I16: “Anthropogenic... sulfur emissions” sounds like tropospheric aerosols from surface sulfur emissions, but probably you are referring to geoengineering through stratospheric sulfur injection—this sentence could be improved to make the message clearer.

P1, I21: Important to be clear that -3 W/m^2 is the peak, or maximum radiative forcing.

P2, I11: Distinction between models using prescribed and prognostic aerosols is not

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really an “approximation”, a better word could be found here.

P2, I15: Models using prescribed volcanic forcing are not strictly dependent on either observations or prognostic aerosol models—simple reconstruction methods have been used for eruptions before the satellite era (e.g., Sato et al, 1993, Gao et al., 2008, Ammann et al., 2003, Toohey and Sigl, 2017).

P2, I16: What “climate feedbacks” are specifically meant here? It’s clear that prescribing aerosols does not allow for feedbacks from atmospheric dynamics onto the aerosol transport and distribution, but the relevance of this on climate seems likely to be small—“climate feedbacks” usually refer to those feedbacks between components of the climate system like atmosphere, ocean, cryosphere, etc.

P2, I19ff: “size-bin resolving” doesn’t sound right to me, “size-resolving” is clear.

P2, I28: remove “problem”

P2, I31: “and is therefore often...”

P3, I4: The model results hint at *differences* in how the models treat aerosol processes—how these differences relate to *uncertainties* in processes is another question.

P3, I5: VolMIP is currently ongoing, please replace “A recent” with “An ongoing” or something like this. I’d recommend also removing the “However” in line 6, this seems to shine a light of disappointment on the development of the VolMIP activity!

P3, I19: describing

P4, I11: prolongs

P4, I17: the quasi-biennial oscillation

P4, I28: Are SSTs and SIC climatological values or transient? From what data are they based?

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P4, I30: Guo et al., 2004 estimate 18 or 19 Tg SO₂ injection by Pinatubo. The 14 Tg SO₂ injection used in this study is within the 1-sigma uncertainty of the estimates from Guo et al. (2004), but some explanation for using a value less than the central estimate should be included here. Similarly, the vertical distribution of the injection is different than that estimated from the satellite observations, which suggest a peak at ~25 km. Some words should be included here to describe why a different vertical distribution was used (“optimized according to Sheng et al, 2015a” doesn’t really help the reader).

P5, I5ff: experiment names like NO_QBO, NO_RAD would be more intuitive.

P5, I10: “We consider two ... experiments concerning the coagulation efficiency”—but only the COAG experiment is described hereafter. The UPWIND experiment seems not directly related to coagulation efficiency.

P5, I23: The high latitude of Cerro Hudson may play a role, but also likely the much smaller SO₂ injection amount (compared to Pinatubo) and the lower injection height. It would be important to list the injection height used in the simulations here, and mention this as potentially important to its impact.

P5, I26: teragram defined previously

P6, I4: The recent paper from Thomason et al. (2017) is a much better reference for the SAGE_3lambda data.

Fig 1: the uncertainty spread in the HIRS data are relatively small, what uncertainties are included in this estimate?

P8, I3: Some description of how the model was sampled is needed here: at the latitude of Laramie I assume, but also the longitude, or zonal mean? Was the model sampled on the days of the balloon flights, or are monthly means used?

P8, I4: what types of uncertainties are included in the OPC error bars?

P8, I6: improves

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P9, l5-6: this statement is arguable for the August comparison

P9, l7: one order of magnitude seems an optimistic generalization: for May at 28 km the difference looks closer to 3 orders of magnitude.

P9, l28ff: I disagree with this summary, the COAG experiment clearly shows a different behavior than the other experiments (e.g., Fig 3) and there are strong differences between the model results and the OPC data, suggesting the model has too many, too small particles, especially at heights above 22 km.

Fig 4 caption: this reads as if all the panels show AOD over oceans, but I assume this is only for AVHRR.

Fig 5: I was surprised at first to see that the global mean AOD for the SAGE data sets is much larger than in plots from other sources (see, e.g., Fig. 1 of Toohey et al., 2016). But this seems to be related to the inclusion of upper tropospheric volcanic aerosol in the data shown here, since the extinction is integrated over a much larger vertical extent here rather than only above the tropopause. This fact might be emphasized more in the discussion here, (perhaps the plot y-label should be clearer as “AOD anomaly”?) and also the details of the integration more clearly stated: were all altitudes used (i.e., right down to the 5km lower limit of the SAGE data sets)? Also, details of how the “background values” were determined for each data set should be explained.

Fig 5: the spike in the AVHRR global mean AOD data in NH summer of 1993 looks suspicious: it doesn't show up in Fig. 3 of Mills et al., (2016) and no obvious source for the spike can be discerned from the zonal mean values in Fig 4.

P11, l5: Importantly, this procedure doesn't remove (upper) tropospheric aerosols from the Pinatubo eruption itself!

P12, l5: “perfectly” is a strong word, and doesn't quite fit here, e.g., there does seem to be discrepancy in the meridional position of the initial tropical AOD peak.

P12, l25: some words needed here on how the annual cycle and QBO cycle were

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subtracted—over what period was the annual cycle determined? How was the QBO defined?

Fig 6: Why is the QBO experiment not shown here? This experiment might shine light on how much of the temperature anomalies shown in Fig 6 are related to the aerosol, and how much to the QBO nudging, and would seem therefore quite important to include in the discussion.

Fig 8: There is a strong QBO signal in tropical ozone—how much of the anomalies shown in this figure are simply a result of the QBO nudging, and how much are due to the volcanic aerosol? Including the QBO simulations in this plot would seem valuable to answer this question.

P15, l3: This was shown much earlier by Tie and Brasseur, 1995.

P15, l5: citation for volcanic chlorine contribution would be good.

P15, l9: Actually, most studies have shown that post-volcanic changes in ozone maximize in the midlatitudes (e.g., Randel et al., 1995, Solomon et al., 1998).

P16, l17: roles

P16, l19: I think “maintaining the tropical stratospheric aerosol reservoir” is not quite the right message here, these processes are important for more clearly definable and important properties, like the global burden evolution and global AOD.

Pg 17, l19: This sentence could well cite the recent paper from Timmreck et al. (2018).

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