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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Uncertainties in modelling the stratospheric warming following Mt. Pinatubo eruption" *by* F. Arfeuille et al.

Anonymous Referee #3

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In this study, the effect of different ways to model the Mt. Pinatubo forcing on the stratospheric stratospheric is analyzed. The authors present four different methods to compute aerosol size distributions from SAGE II extinction data that cover short wavelengths. These computed distributions allow them to calculate extinction coefficients at longer wavelength. The comparison of the extinctions derived from these 4 methods with observations show that those produced using a fit to the four wavelength of SAGE II (SAGE_4λ) reproduce the best the observed extinction profiles and evolutions of both, shortwave and longwave measurements. However, in spite of this accuracy in the representation of the extinction coefficients by SAGE_4λ, these data lead to an overestimation of the aerosol-induced stratospheric heating when using them in the CCM SOCOL.

General comment As the authors indicate, the Mt. Pinatubo eruption provides a very good example to analyze the uncertainties in modelling the stratospheric warming following volcanic eruptions. It was the last largest volcanic eruption and several measurements (satellite, balloon-borne and ground-based measurements) during the period of this eruption are available. Additionally, I also think that a study to improve the modelling of stratospheric warming due to volcanic aerosols is needed based on the discrepancies and deficiencies shown by models (e.g.: Eyring et al., 2010, Chapter 8). However, I have some concerns about the analysis of the effect of the volcanic eruption on the temperature. Whereas, as stated by other reviewers, the authors show a detailed description and discussion of the different results for the reproduction of the extinction coefficients using the four proposed methods, I find the analysis of these methodologies on the stratospheric response to volcanic eruption too short. I would really recommend extending this analysis in different ways as indicated in the specific comments.

Specific comments: Abstract: I would recommend removing the part devoted to the comparison of the SOCOL results with those of other models (GCM or CCM). As I indicate later, I think that one cannot extract general conclusions for the use of new aerosol datasets from the analysis of one single model and try to compare it with results corresponding to an ensemble of models that even use different approaches to represent the volcanic forcing.

Methods: 1) I find the description of the different methods to retrieve spectrally resolved optical properties a little bit confuse. First, in P4605 L21-P4606 L9 it is indicated that there are four methods. Then, only three of them are described: the first, the second and the fourth ones. Moreover, the third corresponds to the use of an aerosol model, whose details are only indicated in very few sentences. 2) This comment is in line with the previous one. A description in more detail of the AER-2D model and the simulations carried out with it would be very useful. 3) I would also suggest including more details about the SOCOL runs and model setup. Apart from the details about the SSTs and

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vertical resolution, as proposed by reviewer 4, I also think that more information about the radiation code of the model would be interesting to include. Questions about the number of spectral bands and how they are distributed would be very helpful.

Results/Conclusions: 1) As mentioned before, the analysis and discussion of the results of extinction coefficients is done in detail. However, it would be also useful to show the corresponding heating rates. Several models, even CCMVal-2 CCMs, implement the aerosol heating using Stenchikov et al. (1998) prescribed heating rates. Additionally, the authors refer several times to those models along the text and make some remarks about them (e.g.: P4618 L9-L15). 2) Instead of showing the effects of the different methods on the temperature by a single CCM (SOCOL), it would be maybe more interesting to analyze more in depth these effects with a detailed radiation model. SOCOL is a CCM and contains approximate models to calculate radiation processes. Thus, in order to conclude that "the overestimation of the stratospheric warming after Pinatubo arises from deficiencies in the model radiation codes", it would be more appropriate to make calculations with a precise line-by-line radiative model. 3) The authors only show the SOCOL stratospheric response to the SAGE_4λ and the aerosol model AER scenarios. It would be very useful to show the stratospheric response in SOCOL simulations using the other two scenarios as well (the one based on an older version of SAGE II dataset, ST98, and that based on a PCA, SAGE_1.2 and SAGE_1.8). This would really help to isolate the effect of the different methodologies on the stratospheric response simulated, at least, by SOCOL. 4) I think that authors should be very careful with the derivation of general conclusions from the analysis of the results with only one single model. As other reviewers already indicated, the differences between the SOCOL results and those coming from the GCMs or CCMs could be not only due to the differences in the methodology to assess volcanic forcing, but also due to the biases of the model. For instance, Eyring et al. (2010) (Chapter 3) show that SOCOL represents well climatological global mean temperatures in the middle and upper stratosphere, but not in the lower stratosphere, where it shows a negative temperature bias between 100 and 40 hPa (see Figure 3.1 of this report).

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Thus, the conclusions derived from Figure 11 should be carefully stated. (from P4617 L24 to P4618 L2). 5) Why is only the tropical temperature anomaly for GCMs shown in Figure 11? And why only the global temperature anomaly for CCMs? In both cases, the same magnitude is shown at two different levels.

Technical comments: 1) I have the impression that the figures are mentioned in the text in a chaotic order and for example, figure 9 is referred before figure 8. 2) The label of the x-axis of figure 9 is not clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 4601, 2013.