Answer to comment of Reviewer 3

We thank Reviewer 3 for his/her review, which adds value to our manuscript. Comments are addressed below. Each comment by the reviewer is first recalled (in italics), then the authors' replies are given.

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.

Authors' reply
We fully agree with Reviewer 3 that the analysis of the model temperature response is short and only meant to be indicative. The paper focuses on the analysis of the aerosol forcing, whereas resulting temperature changes depend on specific climate model responses, whose analysis is beyond the scope of this paper. The manuscript is now modified to better convey the specific focus of this study. Also, as suggested by Reviewer 4, we changed the title to reflect more accurately the focus of this study.

Specific comment 1
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.

Authors' reply
We agree with Reviewer 3 that one cannot extract general conclusions for the use of new aerosol datasets from the analysis of one single model. Rather, we focus on aerosol extinctions. However, the differences to previous data sets are so large that even results from a single CCM can be telling. Our intension is to address the GCM/CCM community and we hope that the SAGE_4λ data set will be used in future. We therefore prefer keeping reference to models in the paper. However, we tried to clarify the corresponding parts of the paper and its abstract.

Specific comment 2

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.

Authors' reply

The third method (AER model) is now described in a dedicated subsection, including information on the AER-2D model and simulations set-up.

Specific comment 3

3) I would also suggest including more details about the SOCOL runs and model setup. Apart from the details about the SSTs and 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.

Authors' reply

Following recommendations by Reviewer 3&4, details on the SOCOL model and setup, including SST input, vertical resolution and information about the radiation code, are now added to the manuscript.

Specific comment 4

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).

Authors' reply

We chose not to show heating rates as this is model dependent. When previously modelers used the heating rates from another model to force their model which does not include aerosol effects in its radiative code, this may have been a pragmatic procedure. But this is not fully consistent because both models have differing parameters which affect the heating rates, such as temperature and greenhouse gas profiles and underlying albedos. Yet, we agree that comparing heating rates among models would be interesting investigate in the upcoming international model intercomparisons. We agree with Reviewer 3 that remarks about models using directly heating rates should then be made with caution, and we modified the manuscript in this way.

Specific comment 5

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.*

Authors’ reply
True. Comparing results from a line-by-line radiative model and diverse CCM/GCMs would indeed be interesting. However, we stress here that the SOCOL results are only meant to show that also with the new dataset we cannot avoid lower stratospheric temperatures to become too high in this model (except close to the tropopause, where the situation clearly improves). Although it is likely that other models will produce similar responses, we cannot state this with any certainty and we cannot provide an exhaustive view on uncertainties in forcing implementations and responses in climate models. We thank the reviewer for this comment and made the statement on “deficiencies in radiative codes” more cautiously.

Specific comment 6
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.

Authors’ reply
We added results from the SAGE_1.2 and SAGE_1.8 scenarios to Fig. 11. We unfortunately do not have at this time ST98 data suited for SOCOL input. A more thorough comparison of SOCOL results together with other CCMs could be done in a future study.

Specific comment 7
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). Thus, the conclusions derived from Figure 11 should be carefully stated. (from P4617 L24 to P4618 L2).

Authors’ reply
We thank the reviewer for this comment. We now mention the uncertainties due to model biases and modified the statements on SOCOL results compared to other GCMs/CCMs results.

Specific comment 8
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.

Authors' reply
Comparisons are done for GCMs and CCMs results available in the cited literature (see Figure 11 caption).

Technical comment 1
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.

Authors' reply
Thanks, we have corrected this.