

Answers to comments of Reviewer 4

We thank Reviewer 4 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

While in the first part the different methods are compared quite sufficiently, I agree with reviewer 2 that the discussion of the stratospheric temperatures is largely incomplete missing previous work and important aspects (QBO, vertical resolution) see specific points below Overall, I recommend publication after revisions, see specific comments below:

Authors' reply

We agree with the reviewer that the discussion of the stratospheric temperatures is incomplete. The present study does not focus on the aspects related to the climate models response to the aerosol forcing but rather on the aerosol forcing itself. SOCOL results (shown in Fig. 11) are only provided to show that the benefits of the new dataset can improve a climate model in certain regions (e.g. close to the tropopause), but that it cannot overcome some other deficiencies, for which the reason must be sought elsewhere. We think that a thorough multi-model assessment should be performed to analyze the response to the new dataset and other uncertainties linked to GCM/CCM processes. The new SPARC/IGAC Climate Chemistry Model Initiative (CCMI) will take a step in the right direction.

Specific comment 1

Title: The title reflects only a small part. I think the word "extinction" belongs to the title. Abstract: The sentence "This suggests that the overestimation of the stratospheric warming after the Pinatubo eruption arises from deficiencies in the model radiation codes rather than an insufficient observational data basis" is misleading and could easily be misunderstood. It doesn't reflect the statement of the "Conclusions" where this conclusion is related to models with "... volcanic forcings with longwave extinctions for the Pinatubo eruption lower than found in this study for the aerosol peak around 40–50 hPa"

Authors' reply

Thanks, we modified the title to read: "Modeling the stratospheric warming following Mt Pinatubo eruption: Uncertainties in aerosol extinctions." Also in the abstract we follow the reviewer's advice and point to other sources of uncertainties in a more cautious manner.

Specific comment 2

The method section could be restructured in a more concisely way, in particular the introduction part starting from line 21 page 4605 to line 20 page 4606. For example the authors talk in this part about four methods, and then they explain in three subsections the 1st the 2nd and the 4th method now named as 3rd method. The three model approaches AER7, AER9 and AER10 are also introduced somehow arbitrarily

at different places. I suggest to introduce the three satellite methods at first and then add the model based approach as 4th method describing the AER7, AER9 and AER10_ASAP simulations together.

Authors' reply

We apologize that this was confusing. The method using the AER model is now described in a separate subsection. See also response to specific comment 2 of Reviewer 3.

Specific comment 3

The AER model set up/simulations could be explained in more detail in the manuscript. I miss some general information, e.g. vertical resolution of the model, meteorological wind fields of AER7 and AER9, the way how the optical parameters are calculated, is the additional aerosol heating taking into account in the simulations etc. etc.

Authors' reply

We improved this by adding AER model information to the subsection on the "AER-based method" (see response to comment 1)

Specific comment 4

Also the SOCOL simulations/model set up could be explained in a couple of sentences. I miss some information about the horizontal and in particular the vertical resolution. Is the model running in a climatological mode or with prescribed SST ?

Authors' reply

Manuscript modified. (See also response to specific comment 3 of Reviewer 3).

Specific comment 5

Results/Conclusions:

For the short wave wavelengths the different approaches are compared to SAGE measurements as references which are somehow the backbone of the applied methods. I suggest an additional comparison of SW flux anomalies with ERBE satellite data as a more independent approach.

Authors' reply

This is indeed a valuable test, but as SW flux anomalies are model dependent, comparison to the ERBE (available for non scanning, all sky) satellite data should be assessed in a future study as part of a model intercomparison project. The SOCOL results given shortly in this study only concern the stratospheric warming response to the Pinatubo eruption.

Specific comment 6

The overestimation of the stratospheric temperature anomalies after the Pinatubo eruption in ECHAM4 is not a new story. I am aware of the various efforts of the Zürich group in the last years to understand this effect. Hence, I am a little bit surprised to find no references in this paper to the earlier work e.g. Heckendorn et al. (2009) and an assessment of the current results with respect to the old ones. This is definitely missing in the current paper.

Authors' reply

Yes, this was our negligence. Reference to Heckendorn et al. (2009) is now added with some comparison to the AER approach results from this previous work.

Specific comment 7

Another important aspect in the discussion of the overestimation of the stratospheric warming following Pinatubo is missing: the vertical resolution of the global models. It might not only be the radiation code of the models, it might also be the relative coarse vertical resolution of most global models in the stratosphere of more than one kilometer. Observed vertical stratospheric aerosol profiles after the Pinatubo eruption (e.g. Deshler et al, 1993) show relative thin stable aerosol layers. This effect should at least be discussed in the paper

Authors' reply

We improved this by mentioning potential factors as vertical resolution in the text. We also changed the abstract accordingly. See also response to Specific comment 5 of reviewer 3.

Specific comment 8

Figures: Fig 9 and Fig 10 could be combined, eventually also with Fig 6, so six panels in total (one has to be added Extinction 5.26 μm EQ). In the upper row the extinction profiles for the tropics could be displayed, in the lower row the ones for 35 N. This would give a nice easy overview figure for the behavior of the different methods at different wavelengths.

Authors' reply

Figures are now modified to show the different wavelengths together. We chose to keep the 2 different latitudes in two separate figures to keep a good visibility of the symbols/lines in each figure.

Specific comment 9

Fig 10 One can hardly see the horizontal line

Authors' reply

Modified.