Answers to comments of Reviewer 2

We thank Reviewer 2 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 corresponding authors replies are given.

Note: Page/Lines numbers used by the referee do not correspond to the ones of the published ACPD manuscript but to the version sent by the publisher prior to the technical check. Corresponding page/line numbers in the discussion paper are given in the authors replies.

General comment

The Pinatubo eruption of 1991 provides an invaluable test for better understanding the volcanic impact on the Earth system. The paper compares several approaches for calculating of the Pinatubo aerosol optical characteristics and discuss their impact on the stratospheric volcanic heating due to aerosol absorption in Infra Red (IR). I appreciate that the authors did step forward using the improved SAGE-ASAP extinction data set. I think this has a good promise. However, the study in parts is misleading. The authors focus on aerosol IR effect and do not discuss the aerosol Short Wave (SW) effect. As a result the SW aerosol properties might be erroneous, but 30% of stratospheric heating is coming from SW absorption in Near IR. They did not present any calculations of aerosol heating rates, and aerosol effect on radiative fluxes. The discussion of stratospheric temperature response is largely incomplete, does not include all data sets, does not exclude the QBO effect. I would suggest the authors to thorough test the radiative code in their own model and present these results before blaming radiative codes in all other models.

Authors' reply

See our "Preliminary response to Reviewer 2"

Specific comment 1

P3, L17: This is incorrect. See figures 6, 8, 10 in the paper.

Authors' reply

Refers to P3, L20-21 in the manuscript.

See our "preliminary response": "In the data used by Stenchikov et al., the missing data was filled by interpolating the last reported measurement down to the tropopause supported by a very limited use of lidar data. This approach leads to the flat shape in extinction seen in Figure 2, petering out into the troposphere. Originally this extrapolation was an 'art' consideration for the well known SAGE II stratospheric optical depth plots. At that time, it was the best that could be done to fill the missing data. As a part of the SPARC ASAP effort (SPARC, 2006), one of us (LT, also coauthor of Stenchikov et al.) pulled together some ground-based and aircraft based data that could yield a better gap-filling than was possible in the Stenchikov et al. study. While even this could use some additional consideration, this method of filling the missing data is a better image of the true aerosol distribution than that used in the earlier data set." To describe more accurately the ST98 gap-filling, the sentence in the manuscript is now modified from "assuming a constant extinction coefficient below 24km altitude" to "via interpolation of extinction coefficients down to the tropopause".

Specific comment 2
P3, L18-19: Incorrect.
St98 used Claes-Isams data to estimate effective radius and width of the distribution.

Authors' reply
Refers to P3, L21-24 in the manuscript.
We thank the reviewer for this comment. Sentence rephrased to: "ST98 used infrared data (CLAES-ISAMS) to fit the effective radius and width of the distribution (assuming a lognormal size distribution with width σ=1.25). However, as we will show below, no systematic comparison with extinction coefficients from HALOE at 3.46 µm and 5.26 µm (Russell et al., 1993; Hervig et al., 1995) or ISAMS at 12.1 µm (Lambert et al., 1997) was performed to verify the applicability for extinction coefficients in the terrestrial IR, which is a requirement for climate simulations"

Specific comment 3
P3, L29: We know that the models have to be compared using the same aerosol data set.

Authors' reply
Refers to P4, L4 in the manuscript.
The community is indeed aware of this as we already acknowledged by citing relevant articles which mention this important issue (SPARC 2010 report, Morgenstern et al., 2010).

Specific comment 4
P4, L1-5: GFDL CM2.1 and CM3 produced excellent response to stratospheric forcing.

Authors' reply
Refers to P4, L5-9 in the manuscript.
We do not state here that every GCM/CCM overestimate the stratospheric warming but that many do (as shown by yellow ranges in Fig. 11). We think that the fact that some models reproduce post-eruptive stratospheric temperatures is irrelevant for the necessity of testing and implementing the updated aerosol data (SAGE vs. 6 or younger) in *all* climate models (even those seemingly not having a problem). See also our preliminary response: "We want to hammer home the deficiencies in the outdated data sets. We would assume that correct answers based on bad data suggest some error compensation that yields these results. There is simply no doubt that the data set from which the present work originates is FAR better than that from which the Stenchikov work proceeded. This is not at all Stenchikov et al.'s mistake, as they did the best that could be done at that time."

Specific comment 5
P4, L20-22: Lidar measurements could differ by an order of magnitude from best collocated SAGE observations. Lidar retrievals require aerosol size distribution, which is largely unknown.

Authors' reply
The Lidar backscatter are converted to extinction coefficients and shown to be in global agreement with SAGE measurements (see Figures 4.32 and 4.33, p143 of

SPARC ASAP report 2006).

Specific comment 6
P6, L3: It is two times less SO2 in comparison with the other estimates.

Authors' reply
Refers to P6, L13-14 in the manuscript.
We thank the referee for this comment. The amount correspond to a mass of sulfur (S) and not a mass of SO2 as we wrongly wrote in the manuscript. This explains the factor 2 difference. Sentence is now corrected.

Specific comment 7
P12, L8: This is incorrect.

Authors' reply
Refers to P13, L1-2 in the manuscript.
Sentence rephrased to: "The ST98 extinctions are based on similar early versions of the SAGE data as the 5 km resolution optical depths values provided by Sato et al. (1993), both having to fill the gap between the point of termination at high altitude towards the tropopause by some extrapolation, and thus showing large differences compared to the state-of-the-art SAGE ASAP data (Fig. 2), which peaks around 22 km. Also, SAGE V5.93 data show larger values than ST98 above the termination altitude, which might be due to differences in altitude resolution."

Specific comment 8
P12: Where is the description of the forth method?

Authors' reply
Refers to P13 in the manuscript.
The AER method is briefly mentioned in some parts of the section 2 but not as a complete subsection (as explained in the text before section 2.1). To follow the reviewer's comment we now added a more complete description of the AER method, in a separate subsection.

Specific comment 9
P14, L. 10-15: What is the difference in terms of heating rates?

Authors' reply
Refers to P15, L13-19 in the manuscript.
The differences in terms of extinctions are large enough to cause important differences in heating. How it translates in terms of heating rates is not shown because this depends on atmospheric composition and temperature, i.e. is model dependent. However, Fig. 11 shows that differences in stratospheric warming are significant.

Specific comment 10
P14, L. 22-24: The 1.024 um to IR extinction ratio is a derived value, does not explain anything.

Authors' reply

Refers to P15, L28-P16, L2 in the manuscript.
The 1.024 to IR ratio provides information on whether a specific aerosol size distribution leads to correct spectral extinction variations. In the same way the analysis of the visible to near IR optical depth ratio has been used (Russell et al., 1996, Stenchikov et al., 1998). Here, we noted that even in the regions where the ST98 data is in good agreement with SAGEv6 1.024um extinction coefficients, its extinctions in the IR underestimate the observations (Fig. 8).

Specific comment 11
P16, L. 2: It is not boundary conditions.

Authors' reply
Refers to P17, L11 in the manuscript.
Modified to "space and time-dependent forcings".

Specific comment 12
P.16, L 6-19: The description is too sketchy. You, probably, show the result of both solar and IR heating, right? What are the solar heating rates in SOCOL? What are the IR heating rates in SOCOL?

Authors' reply
Refers to P17, L16-P18, L2 in the manuscript.
This is indeed total heating (solar+terrestrial). This final part of the manuscript is meant to provide some information about the SOCOL model response to the SAGE_4λ forcing. The paper focuses on the development of an aerosol forcing in close agreement with observed extinctions in the solar to IR spectrum, but it is beyond the scope of the paper to analyze the resulting heating rates in climate models. We fully agree that more work should be done to evaluate the responses of diverse CCM/GCM to the optical properties provided here, for heating rates and resulting warming and dynamical changes.

Specific comment 13
P17, L1-10: Thermal response and radiative forcing associated with a particular aerosol data set is an important information for the data set assessment. Both solar and IR effects are important and have to be balanced. You have to provide your radiative forcing and total spectral optical depth to make it clear what you are doing. You can not just blame raditive codes. Some models, like CM2.1 and CM3, did excellent job reproducing observed stratospheric temperature response.

Authors' reply
Refers to P18, L10-21 in the manuscript.
Thermal response associated with a particular aerosol dataset is indeed important information. However, this is a model-dependent internally calculated quantity, whereas the present study focuses on developing an aerosol forcing dataset that is in agreement with available extinction coefficient measurements. Net radiative forcings or heating rates should not be direct inputs for climate models as they are model dependent and may lead to an erroneous stratospheric warming response.

Comparisons are done here to the level of the extinction coefficients, which is more precise than looking at column integrated optical depths (especially when assessing stratospheric warming). Extinction coefficients are compared to observations at both solar and IR wavelengths.

Those parts of the manuscripts where we propose that the heating overestimation in climate models may be due to error in the radiative codes have been further downtuned. We however maintain that given the large improvements between SAGEv6 and SAGEv5.9x, stratospheric temperature responses in models that are based on the latter cannot be used to validate nor justify the use of SAGEv5.9x datasets.

Specific comment 14
P.17, L13-14: Sato's dataset provides data from 1850. It is good to have Pinatubo period to be consistent with the rest of the data set. So please do not be in a hurry to dismiss this useful piece of work.

Authors' reply
Refers to P18, L24-26 in the manuscript.
We agree with this statement and did not mean to dismiss the entire dataset in this paper which focused on the Pinatubo eruption. Sentence rephrased to: "should no longer be used in studies covering the SAGE II period".

Specific comment 15
Finally, the ST98 extinctions are plotted on a number of figures in your paper. These data are not available from the original Stenchikov et al. (1998) paper. What is this? Why these data are not properly referenced and acknowledged?

Authors' reply
Thanks for this comment, we have corrected this. See also our preliminary response.