

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

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We thank the reviewer for his/her critical comments. We will consider the suggestions, and will do so in full detail when the ACPD discussion phase is closed. However, in important aspects we do have a serious disagreement with the reviewer. Purpose of this preliminary response is to clarify some of these disagreements.

The differences between the SAGE II data version 6.0 used in our paper and in SPARC (2006) and earlier data versions as used by Stenchikov et al. (1998) and Sato et al. (1993) are severe, as Figure 2 of our paper demonstrates in a dramatic way. The reviewer criticizes that we “focus on aerosol IR effect and do not discuss the aerosol Short Wave (SW) effect”. We do not agree with the reviewer. Our microphysical retrieval is based on the new version of SAGE data, covering wavelengths from 386 nm to 1020

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nm, i.e. from UV to near infrared (NIR). The SAGE data V6 provides the most precise global data set on aerosol extinctions, and the fitting quality to the SAGE data of our procedure is very high (see Figure 4a,b). Whether the retrieved size distribution can reproduce the extinctions in the IR (e.g. 12  $\mu\text{m}$ ) properly or not is indeed a critical issue for the aerosol radiative properties. Therefore, the focus of this work is on the IR effects ( $\lambda \geq 1 \mu\text{m}$ ) which, as the title of the paper states, means investigating uncertainties in modeling the stratospheric warming. The reviewer further criticizes that “As a results the SW aerosol properties might be erroneous” and mentions the near IR relevance for the stratospheric heating. First, we note that no near IR absorption measurements exist to test datasets results. Second, the ability of our retrieved size distributions to produce accurate extinction coefficients for the entire UV to IR spectrum (including near IR wavelengths) indicates that our aerosol size distributions capture the aerosol radiative properties. Finally, we maintain our statement that models using datasets based on too small IR extinctions ( $\lambda \geq 1 \mu\text{m}$ ) compared to measurements would produce larger heating rates if using a more accurate aerosol forcing.

In our full response we will consider the reviewer’s suggestion to offer more information on the NIR and VIS region, in order to provide a more comprehensive view of our dataset. However, concerning his/her request to also show more heating rates, radiative fluxes and stratospheric temperatures: while informative, these are model dependent results and should be part of a full-fledged model intercomparison (e.g. within the currently planned CCMI). We view the results from the SOCOL simulations only as an additional piece of information and our claim that the use of aerosol forcings based on SAGE V5.9x data is misleading for climate models is primarily a conclusion from our spectral extinction analysis.

In referring to the outdated SAGE retrieval the reviewer also says that “some models, like CM2.1 and CM3, did excellent job reproducing observed stratospheric temperature response”. And that “Sato’s data set 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

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hurry to dismiss this useful piece of work". We want to hammer home the deficiencies in the outdated data sets. We would assume that correct answers based on bad data suggests 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; there is also no reason to denigrate Sato's work where other data is missing. We will make these two statements as clear as possible in the revised manuscript. Conversely, for the satellite era, it is inconvenient, but we think continuing to use something that yields pleasing results despite evidence that it is based on a flawed starting point does not qualify as science. It is normal that a historical data set is going through developments toward a more mature data product.

In the following we detail why data from outdated versions of the SAGE retrieval algorithm should no longer be used. There are two primary concerns:

(1) The first concern is that the data used by Stenchikov et al. is based on a version prior 6.X. The version 5 series had major difficulties handling dense layers<sup>1</sup>, where enhanced extinction would be spread over multiple kilometers away from the layer and force low values to occur at other altitudes in compensation. This happened most often in the vicinity of clouds but, during the densest parts of the Pinatubo period, it could significantly affect data as high as 30 km. Any version from 6.0 onward does a vastly better job handling the dense layer cases.

(2) The second issue is how the data used by Stenchikov et al. was extended down to the tropopause during the period immediately after the eruptive injection. During this period the line-of-sight optical depth exceeded the value for which SAGE II was able to make meaningful measurements and the profiles terminated well above the tropopause. During non-volcanic conditions this never happens above the tropopause and most often it occurs below 10 km even in the tropics. However, during Pinatubo,

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<sup>1</sup>Dense layers occur when the observed line-of-sight optical depth jumps over a relatively short vertical distance (< 1 km) from nominal, easily measured values to values in excess of  $\sim 6$ .

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this often occurred at 25 km (in the tropics) and occasionally higher. The 'saturation' altitude slowly declined during the 1991-1993 period until it was no longer an important factor by mid-1993. 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.

The reviewer rightly wonders how we managed to plot the Stenchikov extinctions in our figures: "These data are not available from the original Stenchikov et al. (1998) paper." This is correct. The extinction lines marked "ST98" (and actually also the crosses marked "Russell96") are indeed not from the Stenchikov paper, but are "Georgiy Stenchikov, personal communications". We thank the reviewer for spotting this omission and apologize for this oversight.

Concerning all the other points made by the reviewer, we will gratefully take as many as possible into account and improve the manuscript.

F. Arfeuille, B. P. Luo, P. Heckendorn, D. Weisenstein, J. X. Sheng, E. Rozanov, M. Schraner, S. Brönnimann, L. W. Thomason, and T. Peter

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