# Answers to comments of Reviewer 1

We thank Reviewer 1 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 replies are given.

**General comment**
*"The paper would gain a lot if the parts on deficiencies in SOCOL are skipped or at least shortened, especially in the abstract, which is confusing now, because it is not clear what error is due to the retrieval and what is due to CCM artifacts. The parts on the satellite data are very important for the scientific community and should be the main message. The analysis for the visible region should be extended."*

**Authors' reply**
We agree with the reviewer that the part on the extinction coefficients and the comparisons to satellite data is the main focus of this paper. However, if the reference to the CCM artifacts was skipped completely, the paper would not reach a part of the community it is intended for. In order to overcome this deficiency we modified the final manuscript and shortened the part on the SOCOL deficiencies. We also try to clarify our statements on the potential uncertainties in the aerosol forcing and in the CCM implementation. Finally, as asked by the reviewer we extended the analysis of the extinction coefficients in the visible spectral range.

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**Specific comment 1**
*"In the abstract the main focus should be on the new SAGE dataset. If SOCOL is mentioned (might be not necessary there), model problems should be clearly separated from problems due to the old SAGE datasets."*

**Authors' reply**
As suggested we removed the specific reference to SOCOL, rather refer to "a CCM" and from there refer to GCMs and CCMs in general. The abstract should now better separate the dataset issue from the model issues. (This also addresses in part the response to specific comment 1 of Reviewer 4.)

**Specific comment 2**
*"For the comparison with HALOE and ISAMS it is important, to use appropriate refractive indices for the sulphur aerosol (these differ from the ones cited at the beginning of section 4). This issue might be addressed in the introduction or later (not essential). For CCMVal it might be also an issue that models discarded the worst heating rates based on old data below the tropopause in different ways."*

**Authors' reply**
For clarification we have added information for the appropriate use of refractive indices. However, this is additional, not corrective action, and we are not sure what the reviewer means by "these differ from the ones cited". We are not aware of newer or more relevant citations. The issue related to tropopause height and the implementation of the stratospheric aerosols in models is also addressed.

**Specific comment 3**
*"The near infrared channel in section 4.2 is the most used SAGE channel but a more detailed discussion of the channels in the visible should be included here also, maybe moved from the previous section and expanded (split of Fig.4?)."*

**Authors' reply**
The discussion of visible channel data is now expanded (see start of Section 4.2 and Fig. 5).

**Specific comment 4**
*"The figure on the SOCOL results needs a better description or should be skipped. The reader is not interested in compensating errors. Isn't there also ERA-Interim available for comparison? ERA40 is known for biases. Why is there a bias before the eruption for all curves in Fig.11? The bias of SOCOL there causes most of the difference to ERA40 in the Pinatubo-period which is misleading. What is the zero line? If the figure is kept, also SOCOL results with the outdated SAGE data or Sato (1993) should be shown for separation of effects."*

**Authors' reply**
SOCOL figure is now modified, using ERA-interim. The bias before the eruption which can reach 1° is hard to attribute and may be due to internal variability. It is however of smaller amplitude than differences in the Pinatubo period. We now mention this source of uncertainty in the text. Zero line is mentioned in the figure caption.
We did not perform model runs with SOCOL using the Sato (1993) or ST98 data. Reasons are that SOCOL requires extinctions (β), single scattering albedos (ω) and asymmetry factors (A) for each of its spectral bands, whereas these datasets come either as single wavelength extinctions or as heating rates, which leave the required information underdetermined. It would entail substantial effort and uncertainty to apply this within SOCOL or any other model based on β, ω and A. Also, it was not possible to obtain the aerosol size distributions used by ST98, from which we could have calculated β, ω and A. Therefore, we think it is not worth this effort given the differences in extinctions speak already a sufficiently clear language as shown for instance in Fig. 2.

**Specific comment 5**
*"In the model section and/or the conclusions also the consequences of a bad heating rate at the tropical tropopause for stratospheric water vapour should be addressed."*

**Authors' reply**
We thank the reviewer for this comment, and we added text on this issue in the updated manuscript.

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**Technical comment 1**
*"Please use a consistent spelling of the sulfur mass unit (case!). What is the correct version of Fig.1? Please check symbols in Figure 2, the legends appear to be inconsistent. Better indicate months and years at x-axis of Figs. 4, 5, 7 and 11. Typos in caption of Fig. 8. Use _ (4 _). In the caption of Fig. 10 better write 'horizontal lines*

*in symbols'."*

**Authors reply**

Changes done. Correct version of Fig1 is now integrated to revised manuscript and figures 2,4,5,6,7,8,10 are updated according to the reviewer's comments.