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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A perturbed parameter model ensemble to investigate 1991 Mt Pinatubo's initial sulfur mass emission" *by* J.-X. Sheng et al.

Anonymous Referee #2

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1 General comments

The conclusion that the lowermost estimate of earlier studies for SO_2 injected by Pinatubo should be selected is only of limited value because it appears to be significantly perturbed by model artifacts and arbitrary weighting of 'scores' for differences to observations. Giving the MLS SO_2 measurements a higher weight and the rather uncertain burdens estimated from SAGE during saturation of the instrument a lower one would completely change the conclusions. From text and figures it is also often not clear how the scores were calculated. In the table and the figures important cases are missing.

In the introduction the ATMOS observations of Rinsland et al. (1995) should be cited. They should be included into the scoring scheme too.

2 Specific comments

Section 2.1: Is the pre-calculated OH based on the updated chemistry? Why OH is pre-calculated? Most chemical 2D-models calculate that in interactive mode. Errors in the troposphere due to simplified hydrocarbons are not relevant for Pinatubo. I suppose meteoric dust is not treated explicitly.

Section 2.2: Due to the low vertical resolution the 3D model cannot have an internal Quasi-Biennial Oscillation. Is nudging applied? Especially if this is not the case the tropical water vapor tape recorder is artificially fast. Please expand.

Section 3, metrics, paragraph 1: An equation should be given for scores. What is in the denominator? The exact definition is especially important for the extinction which varies on a logarithmic scale. Now the text is rather confusing. Paragraph 2: I cannot follow the arguments for weighting. Before September 1992 SAGE derived burden is very uncertain due to saturation and gap filling. What is month 12 in line 14? Is this December 1991 (Fig. 3) or June 1992? It is not appropriate to give SAGE burdens (and extinctions in lower stratosphere) a large weight from January 1992 to September 1992.

Section 3, scoring table: The case with rank 1 for SO_2 should be listed in Table 1 too. The cases with peak emission at 29.5km should be skipped because that is against any observation. The conclusion in line 19 is strongly dependent on the arbitrary weighting and too early. All scenarios having rank 1 in one criterion should be discussed in more detail.

Section 3, matching SO_2: Here also the scenario with rank one in this criterion should

be discussed.

Section 3, matching burden: The sulfate mass without water should be given too because there is often confusion in the literature on this. There is the common problem that simulations are too high in the early phase and too low in the second and third year after the eruption. Elaborate more in this. The sentence with 'age of air' is confusing.

Section 3, matching extinctions: All shown 2D-simulations overestimate extinction above about 23 km and underestimate it in the lowermost stratosphere 1 year after the eruption. All scores are rather poor in this criterion but have a large weight.

3 Technical corrections

Page 4605, line 12: Typo?

Page 4606, lines 16ff and Table 1: μ is a bad choice for an altitude parameter since it is normally used in atmospheric sciences for totally different quantities. Better use for example z_0.

Page 4608, lines 16f: improve structure, sentence is confusing.

Page 4608, lines 25ff: the numbers should go also into the table caption

Introduce subsections in section 3.

Figure 1: Include all relevant simulation numbers of Table 1 in legend.

4 References

Rinsland, C. P., M. R. Gunson, M. K. W. Ko, D. W. Weisenstein, R. Zander, M. C. Abrams, A. Goldman, N. D. Sze, and G. K. Yue, H_2SO_4 photolysis: a source of sulfur

dioxide in the upper stratosphere, Geophys. Res. Lett., 22, 1109–1112, 1995.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 4601, 2015.